MEASURING THE EFFECT OF PUBLIC LABOR EXCHANGE (PLX) REFERRALS AND PLACEMENTS IN WASHINGTON AND OREGON

Final Report of the
Public Labor Exchange Performance Measures Validation Project

Prepared under Contract No. X-6879-8-00-80-30

Prepared for:

Washington State Employment Security Department
212 Maple Park
Olympia, Washington 98504

Prepared by:

Louis Jacobson

Ian Petta

Westat
1650 Research Boulevard
Rockville, Maryland 20850-3195

October 4, 2000

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Executive Summary

EXECUTIVE SUMMARY

This report describes our analyses of the effects of direct placement services provided by public labor exchanges (PLXs) to job seekers in the states of Washington and Oregon from 1987 to 1998. A nationwide system of state-Federal PLXs was created following passage of the Wagner–Peyser Act in 1933. Our goal was to determine their value and develop procedures that the U.S. Department of Labor (US-DOL) could routinely use to provide meaningful feedback to PLX program operators and state and Federal policymakers.

Overview of Our Findings on the Benefits and Costs of PLX Services

The primary focus of our work was to develop a means to accurately measure the returns to direct placement services—referrals and placements. To do this we relied on three data sets:

1. Survey responses from 587 job seekers referred to jobs by Washington State PLXs during the first half of 1998.

2. Administrative data covering PLX use during 328,815 spells of unemployment covered by unemployment insurance (UI) in Washington State from 1987 through mid-1995.

3. Administrative data covering PLX use during 138,280 spells of unemployment covered by UI in Oregon during 1995.

We used these data to estimate the effect of placements and referrals on the duration of unemployment. We also used a simulation model developed by Professors Davidson and Woodbury of Michigan State University to examine the extent to which reductions in unemployment to PLX users comes at the expense of nonusers.

Estimating the effect of PLX services is a very difficult task because the effects of these services per person are often small and because random assignment (experimental) designs cannot be used. Technical experts agree that experimental designs offer the best means to produce unbiased estimates, but PLXs must provide universal access, making it impossible to implement those designs. Thus, much of our work was aimed at finding alternative ways to produce results that an expert panel would agree are unbiased. While we had some success in finding ways around the central problem, we did not have time to fully implement the solutions. Thus, our estimates of direct placement effects substantially narrow the range of plausible values, rather than provide tight point estimates.

Our primary conclusion from our analyses is that surveys <u>have the potential</u> to identify job seekers referred to jobs too late to obtain interviews, and that these individuals would serve as a comparison group to produce unbiased estimates of placement effects—the value of being placed relative to obtaining referrals.

The pilot procedures tested in this report produced estimates that job seekers with strong work records in our sample who were placed by PLXs experienced a 7.2-week reduction in their duration of unemployment, and placed job seekers with spotty work records experienced a 3.4 week reduction. These unemployment reductions translate into increases in earnings of $1,872 and $684 for job seekers with strong and spotty work records, respectively.

If we make the highly conservative assumption that placements are the <u>only</u> source of benefits from PLXs, placements must return more than $542, on average, for PLXs to be cost effective. The placements included in our sample returned about $978, on average. This calculation produces a respectable benefit-cost ratio of 1.8 for the sample studied.

Unfortunately, we cannot legitimately claim that the results generated from our pilot sample apply to all 11,144 claimants and 35,038 nonclaimants placed by Washington State PLXs in 1998. The primary problem is that the pilot sample was not representative of all placements. There also is some uncertainty about how close our unemployment reduction estimates are to the true values for those in the sample. This is because the small sample we used produced relatively large confidence intervals, and some bias may have been introduced because some job seekers in our comparison group may have been denied interviews because employers felt they were unsuitable. <u>Fortunately, all three problems could be eliminated in future work by surveying a large representative sample and obtaining additional information about the reason for being unable to secure interviews.</u>

A second important conclusion is that, once we have unbiased measures of placement-effects based on identifying job seekers who obtained referrals too late to secure interviews, those estimates can be used as benchmarks to produce unbiased results from administrative data alone.

Indeed, even though our survey-based estimates of placement effects are not definitive, those results for UI claimants were similar to those derived from analysis of the very large administrative databases for Washington State, especially when differences in business conditions are taken into account. The administrative data showed that benefits are about 30 percent greater in the trough of a business cycle than during its peak because there are fewer claimants to help in good times, and claimants can more readily find jobs on their own in prosperous periods. Thus, much of the differences observed could be attributed to business conditions being substantially better in 1998 than during the 1987-95 period covered by the administrative data.

We believe that the differences between the survey-based and administrative-data-based results were small for two reasons. First, the comparisons used to measure placement effects are restricted to job seekers referred to jobs. Thus, selection bias due to only some job seekers choosing to use PLXs is absent from these estimates. Usually this is the largest source of bias and the one that is most difficult to remove. Second, we speculate that only small biases were introduced due to the survey sample being nonrepresentative and some job seekers in the comparison group being rejected by employers

A third key conclusion is that placement-effect estimates substantially underestimate the total value to job seekers of direct placement services. Our estimates based on administrative data alone suggest that placement reduce claimants' unemployment by 7.7 weeks, while referrals not leading to placements reduce claimants' unemployment by 2.1 weeks. Because only about 1 in 5 claimants obtaining referrals are placed, even small per-person gains due to referrals not leading to placements would produce large benefits in total. Estimates based on administrative data suggest that about 55 percent of earnings gains come from placements, and 45 percent come from obtaining information from use of job banks and staff in the course of being referred.

Because we lack an unbiased estimate of referral effects for use as a benchmark we do not know how close our referral-effect estimates are to the true effects. However, we do know that referral effects estimates depend on comparing PLX-users to nonusers, and that selection bias in these types of comparisons consistently leads to underestimation of the true effect. In general, individuals volunteering to use government services have special difficulties that make them need the aid more than nonusers, but the factors that are associated with these differences often are not described well with available data.

Indeed, referral effects were zero prior to adjusting the raw differences between referred claimants and those not referred to account for selection bias. Also, experimental evidence on the effect of job search assistance (JSA) uniformly suggests that JSA has small positive effects. However, we believe that PLX direct placement services are considerably more potent than the types of JSA studied using experimental designs. We, therefore, are confident that the true effect is considerably greater than zero, even if it is not as great as the 2.2-week estimate we produced. Thus, we feel that it is reasonable to believe that there are substantial benefits derived from obtaining referrals, even when jobs are ultimately located from other sources.

Clearly, obtaining unbiased referral-effect estimates for use as a benchmark is of enormous importance in estimating the value of PLX direct placement services with accuracy. Obtaining such a benchmark is difficult because it is not feasible to create a control group by denying access to PLX job-listings. However, it may be possible to develop unbiased benchmarks from an experimental design that would randomly call in claimants to review job listings. Although we could not prevent job seekers who were not called in from using PLX services voluntarily, we still could measure the bias associated with using nonexperimental estimators. Also, it may be possible to develop a reasonable estimate of referral effects based on experimental evidence of the value of job search assistance programs that do not require granting universal access.

A fourth key conclusion is that the per-person placement and referral effects for claimants in Oregon were considerably smaller than the effects in Washington State. Oregon administrative data suggest that placements reduced claimants' duration of unemployment by 4.6 weeks, and referrals not leading to placements reduced claimants' duration of unemployment by 1.1 weeks. Even though we have no unbiased estimates for use as benchmarks, we feel that it is reasonable to believe the biases in the Oregon and Washington results are similar. Thus, the differences in the results are primarily due to differences in the true effects.

These differences could stem from two key differences in the way claimants interact with PLXs in the two states. First, Oregon applies a more stringent work test and requires claimants to register with PLXs in person, where they are likely to also review PLX job listings. These actions makes it more likely that Oregon claimants who do not obtain PLX aid will quickly accept suitable jobs or stop claiming benefits, and those who examine listings will also quickly find jobs and pursue leads they develop on their own more vigorously. Second, Oregon spends more state funds on PLXs than does Washington, even though both states substantially boost expenditures above those provided by Federal programs. The higher spending in Oregon translates to more job orders available per PLX user in Oregon. Having more job orders per person also allows Oregon to refer and place about the same number of clients as Washington, despite having about half as many jobs available overall in Oregon as in Washington.

We suspect that the combination of claimants viewing listings early in their spells of unemployment and having more openings to choose from helps claimants who, on average, are more likely to find jobs quickly on their own. Certainly, the administrative data in both states shows that the per-person effects of direct placement services are far greater after the tenth week of unemployment. <u>We could test these hypotheses more definitively by combining the data from the two states</u>. However, that was not possible in this study because Oregon could not release its data to us, and we lacked the time to transfer the Washington data to Oregon.

Our fifth conclusion is that PLX direct placement services substantially reduce UI payments. However, these reductions equal about <u>one-quarter</u> of the gains in earnings. This evidence rests on estimates that use administrative data alone. However, we suspect that there is little bias in the estimate of the split between reductions in total unemployment, which raise job seekers' earnings, and unemployment covered by UI payments, which reduce UI payouts. Importantly, employers often only focus on the direct benefits of reductions in UI payroll taxes owing to reductions in UI payouts. However, they often overlook that they also benefit directly from vacancies being filled more quickly. Similarly, they often overlook that they benefit indirectly from being able to reduce wages they must pay their workers to compensate them for the risk of job loss and temporary unemployment stemming from PLXs making these situations less costly to job seekers.

Our final key result is that 80 percent of the benefits to claimants were derived from helping employers fill vacancies more quickly. This directly leads to expanding the production of goods and services and reducing their price. The simulation model we used also suggested that the negative "crowding-out" effects on nonclients are small per person, equal to only about 2.5 hours of work. As with our other evidence, we do not claim that we proved that the crowding-out effect is exactly 20 percent, but that the true effect is in the neighborhood of that value.

In summary, our most important achievement is developing a procedure that we are confident would produce unbiased estimates of placement effects, if fully implemented. Also, we have developed several additional procedures that might produce unbiased estimates of referral effects. Having measures that technical experts agree are unbiased is of enormous importance because these estimates could be used as benchmarks for developing measures to use administrative data alone to also produce unbiased estimates.

While technical experts do not agree that our current estimates are unbiased, our evidence on the effectiveness of direct placement services suggests that the benefits are substantially

greater than the costs— returning perhaps as much as $2 for each $1 spent. Our best evidence for this view is that the placement-effect estimates that use the administrative data and survey data in Washington are similar and the bias in these estimates is likely to be relatively small. Also, our analysis suggests that referral effects are considerably greater than crowding out effects. In short, while we do not have point estimates that experts would agree precisely identify these effects, the estimates we do have considerably narrow the likely range of plausible effects. Importantly, we have outlined additional analyses that would further shrink the plausible range of these effects.

Overview of Our Findings on Ways to Improve Monitoring of PLX Activities

Although the primary focus of our work was to develop ways to accurately estimate the benefits and costs of direct placement services, we also examined the value and feasibility of using the measures we created to routinely monitor PLX performance. Our central conclusion is that **it would be highly feasible to routinely use the measures derived from administrative records** because the data required are not very different from those needed to implement the measures called for in the Workforce Investment Act (WIA).

Of great importance, while the measures are not perfect, **they provide information that is likely to help PLX managers and staff substantially increase the value of PLX services**, as well as provide a reasonably accurate view of the total value of PLX services. In particular, the measures we produce here have the potential to assist in making key decisions about:

- When, relative to the start of unemployment spells, claimants should be given PLX services.
- How effort should be divided between securing and filling job orders versus providing labor market information that can help clients find jobs on their own.
- What types of clients benefit the most from placements versus information that helps finding jobs on one's own.

In sharp contrast, maximizing WIA measures, such as the entered employment rate, is likely to lead to decisions that reduce the value of PLX services. The central problem with WIA measures is that they give incentives for PLXs to serve clients most likely to find work on their own rather than clients who will benefit the most from PLX aid. Importantly, the problems with use of descriptive statistics as performance measures is much greater for PLXs that grant universal access than for targeted programs such as those funded under JTPA. Specifically, creaming and other negative consequences of using measures like the entered employment rate were minimized under JTPA because program operators were required to enroll clients with substantial impediments to finding jobs on their own.

We learned a great deal about the feasibility of a state agency creating the measures because the Oregon Employment Department (OED) did all of the data processing for the Oregon study with little help from us. However, the data processing has to be carefully executed. In particular, the completeness of the coverage of individuals in the raw files needs to be checked against published statistics, and the transformation of each variable needs to be checked by comparing the input and output files at each stage.

Finally, it is our view that the appropriate criterion for use of the measures in this report is whether they are superior to other measures. The measures do not need to be perfect in order for them to be highly useful. At the same time, every effort should be made improve the statistical quality of the estimates. Thus, the additional work outlined in the preceding section would be of substantial value. However, even that work would not be sufficient to measure referral effects accurately using administrative data for nonclaimants and to expand the range of PLX services included in the analysis. Producing these measures would further increase the usefulness of the measures. Considerable progress in developing those measures could be made using an expanded mail survey that included telephone followup.

The major threat to developing a comprehensive measurement system, however, is the rapid spread of PLX computer systems that allow clients viewing listings to <u>obtain contact information without staff intervention</u>. Only in Oregon are self-referrals tracked, but **without such tracking, it is almost impossible to measure the benefits of direct placement services.** Thus, the benefits and costs of the Oregon system merit careful study. **If the analysis is positive, serious consideration should be given to requiring that self-referrals be tracked nationwide.**

Details of the Individual Studies

The above sections summarize our overall findings for all four of the studies presented in Chapters 3 through 6. The next few sections of the executive summary provide additional information to make the results and estimation procedures of the individual studies clearer. Additional background information about PLX operations, estimation techniques, and results are found in Chapters 1 and 2. Chapter 7 presents an expanded discussion of the issues raised in this overview, and presents more information about the conceptual framework of our analysis. Chapters 3 through 6 present our work in sufficient detail for technical experts to assess the quality of that work independently. Obtaining feedback from experts is important because the accuracy and relevance of the innovative estimation procedures used **need to be independently judged in order for the results to be widely accepted**. Chapter 8 presents our expert panel's comments, a summary of areas of agreement and disagreement, and suggestions for future analysis.

As noted earlier, our primary focus was estimating reductions in unemployment due to referrals and placements of job seekers with strong work records, most of whom were unemployment insurance (UI) claimants, and of job seekers with weak work records. All those benefiting from PLX services had reached Step 5 on the job search path shown in Table 1. These job seekers had decided to search for work (Step 1), decided to obtain assistance from PLXs (Step 2), were able to look at PLX job listings (Step 3), looked at PLX job listings (Step 4), and found promising listings for which they wanted contact information (Step 5).

Table 1. Job Search Path from Deciding to Search for Work Through Deciding to Use PLXs to Placement by a PLX

Steps to surmount		Path ending outcomes
Step 1.	Unemployed worker decided to search for work	a. Recalled by former employer b. Retired c. Dropped out of labor force
Step 2.	Job seeker decided to use PLX	No desire to use PLX
Step 3.	Job seeker gained access to PLX	Unable to use PLX because services were unavailable or too difficult to access
Step 4.	Looked at PLX listings	Found no suitable jobs
Step 5.	Found promising listings	Decided not to interview for those jobs
Step 6.	Tried to obtain an interview	a. Job or interview slots filled b. Employer rejected job seeker based on prescreening
Step 7.	Obtained interview	Did not receive an offer
Step 8.	Received an offer	Rejected offer
Step 9.	Accepted offer	Did not show up for work
Step 10.	Showed up for work	Placed by PLX system

Our research focused on measuring the value of direct placement services because:

- Maintaining a universal system for employers to list job openings and for job seekers to view those openings is the distinguishing feature of PLXs and absorbs most of its costs;

- PLXs' provision of direct placement services plays a central role in the shift from the government's "train-first" to "work-first" policy, and it was an opportunity to determine how well the policy was working; and

- Little is known about the value of direct placement services because the required universal access precludes use of a random-assignment design, and devising accurate alternative measurement techniques is extremely difficult.

We examined three benefits of direct placement services to job seekers: (1) gains in earnings attributable to reduced periods of joblessness; (2) reductions in unemployment insurance payments, which primarily benefited employers in the form of reduced payroll taxes; and (3) increases in the overall efficiency of the labor market that benefit society at large by expanding the amount of goods and services that are available and by lowering their price.

We focused on two different ways PLXs can assist job seekers. The first is by directly placing individuals at jobs listed with the PLXs (Step 10 in Table 1). The second is by providing information that helps job seekers find jobs more rapidly on their own or accept jobs to which PLXs supplied referrals (Steps 4 through 9 in Table 1). The benefits of direct placement are obvious. Less obvious is that looking at listings and obtaining information about job prospects from PLX staff can provide job seekers with a more realistic assessment of the pay and other characteristics of jobs they are likely to find on their own and better ways to locate suitable jobs. The literature on job search suggests that lack of accurate information is a major impediment to finding work quickly.

Accurate measurement of the effect of referrals and placements hinges on comparing what actually happened to job seekers receiving those services, which is directly observable, to what would have happened had those services not been received, which is not directly observable. Our research explored two alternatives to the use of a random-assignment design for determining what would have otherwise happened. The first was to take advantage of a natural experiment identified through use of a mail survey. When properly used, this information can come close to the ideal of comparing PLX placed job seekers to job seekers who were identical to those placed except that they were unable to secure interviews after being referred.

The second alternative was based on attempting to obtain sufficient administrative information about job seekers to identify differences in individuals that affected their job-search outcomes and use of PLXs. This information permits estimating what happened to job seekers in a comparison group of those individuals who were not referred but whose characteristics were identical to those who were placed or referred by PLXs.

Natural Experiment Placement Results from the 1998 Washington Mail Survey

We determined through use of a mail survey that many job seekers were unable to secure interviews after being referred to desirable jobs. Information provided by PLX staff suggests that in almost all of the cases interviews could not be secured because lags in removing the listings led PLXs to make referrals after jobs (or interview slots) had been filled. This natural randomization created a situation similar to a "true" experiment in which randomly selected job seekers, who decided to interview for promising listings, would be told by employers that the job was already filled (whether or not that actually was the case).

We conducted a pilot test by mailing questionnaires to 3,000 individuals who were referred to jobs by Washington State PLXs in the first half of 1998 but not placed at those jobs (or at any other PLX-listed job in the subsequent 4 weeks). This test was designed to determine (1) if sufficient numbers of individuals were unable to secure interviews because the jobs (or interview slots) had been filled and (2) if we could obtain a sufficient number of responses to measure the value of placements. We also mailed questionnaires to 3,000 individuals who were placed at the **same** PLX-listed jobs during the same 1998 period.

We received 1,115 responses from the 6,000 mailings; 43 percent were from referred-but-not-placed individuals. A total of 587 contained sufficient information to measure the effect of placements. This information showed that 33 percent of those referred tried but were unable to obtain interviews. Our analysis showed that placed job seekers with considerable work experience found jobs 7.2 weeks sooner than they would have had they found promising PLX openings but were unable to secure interviews. Placed job seekers with little work experience found jobs 3.8 weeks sooner than otherwise would have been the case. (In both cases, job seekers at Step 10 were compared to those at Step 6.)

The above results are not the same as those that would be generated from a true experiment mainly because employers may have denied interviews to some job seekers who were not well qualified for their jobs. Given the information we obtained from PLX staff, we doubt that this seriously biases the results. A far greater problem to accurately measuring total benefits is that the sample itself was small, and only a small fraction of those sent surveys returned them. Thus, it is possible that our results differ substantially from the true average effect due to nonresponse bias.

Despite these shortcomings, we use the above results to provide a ball-park illustration of the size of the total benefits. To do this we multiplied the above results by published data on the number of individuals placed by Washington State PLXs in 1998, and then multiplied that product by an estimate of post-unemployment weekly earnings. This procedure produced an estimate of $45 million in job seekers' earnings gains resulting from placements alone—a figure equal to 1.8 times the total cost of running the Washington PLXs ($25 million) in 1998, which already is a respectable ratio for any government program.

It is our view that this 1.8 figure is a reasonable first approximation of the true benefit-cost ratio. Even if we have considerably overestimated the true value of placements, the value of the information provided by PLXs that does not lead to a direct placement as well as the value of other services, which is omitted from this estimate, is most likely considerably greater than our estimates of the crowding out effects. There are defects in the analysis due to both a small and nonrepresentative sample, and also from the fact that employers have screened out some job seekers requesting interviews. Importantly, these can be overcome by using telephone followup to secure a large, representative sample and by revising the survey to determine whether job seekers were asked any questions when they tried to set up interviews that could have allowed employers to screen out applicants.

Indeed, had we known in advance that 33 percent of those referred tried but were unable to secure interviews and that the mail response rate only would be about 20 percent, we would have asked the US-DOL to make the substantial investment needed to conduct telephone followup. However, without this information, we felt that it was prudent to first determine the potential value of the mail survey approach.

A shortcoming of the mail survey study, which probably cannot be remedied, is identifying a natural experiment that would permit us to measure the effect of referrals that do not lead to placements. If we were going to use a random assignment design, our key goal would be to intervene at Step 3 on the placement path shown in Table 1 to create a control group of job seekers who wanted to view PLX job listings but were unable to do so. Establishing this control group would permit us to determine the value of information that job seekers obtained from viewing listings and discussing their suitability with PLX staff. However, with the possible exception of job seekers living in isolated rural areas, all job seekers can easily visit PLX offices or view listings using computers at libraries and other public places. Also, job seekers with access to personal computers in both rural and urban areas can view listings using the Internet.

Not having reliable experimental evidence about the value of referrals that do not lead to placements is an important shortcoming. First, experimental evidence suggests that job search assistance that is less intensive than obtaining information from viewing PLX listings and interacting with PLX staff is of substantial value. Second, even a small per-person referral effect would greatly increase total PLX benefits because four out of five job seekers who obtained referrals were not placed by PLXs.

Nonexperimental Referral and Placement Results for Washington Claimants from 1987-95 Administrative Data

Although we could not produce experimental estimates of referral effects, we were able to obtain a plausible range of estimates by applying nonexperimental techniques to PLX administrative data. We did this by comparing the duration of unemployment of job seekers who were referred but not placed (who reached Step 4 in Table 1 but did not reach Step 10) to job seekers who were not referred (did not reach Step 4) and in most cases did not use PLXs at all (did not reach Step 3).

Importantly, we also used the same technique to replicate the estimates derived from the natural experiment identified with the mail survey to estimate the value of placements (reaching Step 10) relative to obtaining information from the listings and PLX staff (reaching Steps 4 through 9). These results were similar to those generated from the mail survey, which suggests that biases in the techniques using the natural experiment and administrative data are reasonably small.

Because administrative data only provide the detailed information needed for this analysis for UI claimants, we limited the nonexperimental analysis to this one group. In particular, the data describe how long claimants have been unemployed when they receive PLX services and, in most cases, when they returned to work. Being able to produce separate estimates based on how long claimants were unemployed at the point they received PLX aid proved to be a particularly potent way to take into account factors that influence PLX use and subsequent duration of unemployment that were not directly observable.

Also of considerable importance, UI claimants are likely to either be reemployed or searching for work, rather than having retired or dropped out of the labor force. Using these data, therefore, greatly reduces measurement problems stemming from an inability to distinguish between jobless individuals who are looking for work and those who are not looking.

Thus, it is reasonable to believe that our analytic technique explicitly or implicitly held constant many of the factors that affect job search outcomes, as well as those leading to an individual's decision to use PLX services and, thereby, was relatively free of bias. However, as mentioned earlier, we could not measure the amount of residual bias in our measure of referral effects because we could not create a benchmark derived from a random assignment design.

As shown in Table 2, we used administrative data alone covering 1987 through 1995 to estimate that Washington State claimants who were referred but not placed returned to work 2.1 weeks sooner than they would have if they had not obtained referrals. As noted earlier, both placed and referred-but-not-placed claimants may benefit from having more accurate information about the difficulty of finding suitable work, as well as from having more opportunities to interview for jobs. Thus, PLX users may more quickly accept job offers they obtain on their own or receive as a direct result of PLX referrals than they would if they had less accurate information about the state of the job market.

Table 2 also displays our estimate that the reduction in joblessness of placed claimants (those reaching Step 10) was 7.7 weeks less than those who were referred-but-not-placed (those reaching steps 4 though 9). The 7.7-week estimate measures precisely the same benefit source as the 7.2-week estimate derived from the natural experiment revealed by our mail survey for 1998, but applies to the 1987-95 period. Importantly, our year-by-year analysis of the Washington administrative data indicates that the effect of being placed in 1987-95, a period strongly affected by recessions, is at least 15 percent greater than being placed in 1998, a prosperous year.

Applying the 15 percent differential to the 7.2-week estimate suggests that the effect of being placed in 1987-95 would be about 8.3 weeks. Thus, if anything, the nonexperimental estimator produces conservative results. Importantly, a direct comparison using the 1998 mail survey and 1998 administrative data also suggests that the nonexperimental measures underestimate placement effects. Also, unlike the mail survey results, these results are based on an exceptionally large, representative sample.

Table 2. Study Characteristics and Measures of PLX Benefits

Data source	Population studied	Back to work effect of: Placement relative to referral	Referral relative to no referral[1]	Total PLX benefits per year[2]	Benefit – cost comparisons[3]
Study-1 Washington Mail Survey and Administrative Data for the first half of 1998	A sample of 587 individuals referred to PLX job openings	7.2 weeks sooner for job seekers with strong work records 3.8 weeks sooner for job seekers with weak work records	Not examined	$45 million for all 1998 PLX users from placements alone	Annual cost $25 million Benefit-cost ratio 1.8
Study-2 Washington Administrative Data for 1987–95	A sample of 328,815 spells of unemployment experienced by UI claimants	7.7 weeks sooner	2.1 weeks sooner	$11 million for claimant placements alone 1987-95 $25 million for claimant placements and referrals 1987-95	Annual cost $25 million 35 percent spent on claimants Benefit-cost ratio between 1.2 and 2.8
Study-3 Oregon Administrative Data for 1995	A sample of 138,280 spells of unemployment experienced by UI claimants	4.6 weeks sooner	1.1 weeks sooner	$15 million for 1995 claimant placements alone $30 million for 1995 claimant placements and referrals	Annual cost $26 million 38 percent spent on claimants[4] Benefit-cost ratio between 1.6 and 3.1

[1] Referral effects measure the value of information obtained by viewing PLX listings and obtaining staff aid that improves the decisionmaking of placed and nonplaced PLX users.

[2] Study 1 uses published statistics to estimate the number of placements. Study 2 uses tabulations of person-level files to measure the number of placements and referrals. Study 3 uses both sources of information. Use of published data for 1995 raised benefit estimates for Study 2 to $42 million for placements and referrals together and $13 million for placements alone. This increased the 1995 benefit-cost ratios to 4.5 for placements and referral and to 2.1 for placements alone.

[3] Benefit-cost ratios are not adjusted for crowding-out effects analyzed in Chapter 6. Their inclusion would reduce the ratios by about 20 percent.

[4] Only 25 percent of Washington PLX costs went to referring claimants in 1995.

If we ignore the value of information obtained in the course of being referred, the total benefits in terms of job seekers' earnings gains are about $11 million per year for 1997-95. This amount equals about 55 percent of the entire yearly cost of running the PLXs. But we estimate that only about 35 percent of PLX costs went to helping claimants. Reductions in UI payments to job seekers who were placed equaled about $2.6 million per year. Thus, job seekers' net income gain was about $8.4 million each year. However, employers benefited from the reduced UI payouts by having their tax burden reduced.

The above calculations produce a highly respectable 1.7 benefit-cost ratio. The benefit-cost ratio was particularly high in the 1991–93 recessionary period because jobs were hard to find, many claimants needed help, and UI payments were extended to cover much longer periods than usual. Total benefits in today's economic conditions are considerably less than in 1991–93, mainly because in this boom time, PLXs are assisting far fewer claimants. Nevertheless, economic conditions in 1990 were not much different from today's, and total benefits accruing to claimants in that year equaled 40 percent of the total cost of running the entire PLX system in Washington

If we accept as accurate the 2.2-week estimate of the per-incident value of information not leading to a placement, adding these benefits ($14 million) to those for claimants who were placed increases average total benefits to about $25 million per year for 1987-95. This is roughly equal to the entire annual cost of running the PLXs. About 55 percent of the benefits are due to placements and the remainder to referrals that do not lead to placements. Placements account for most of the benefits because placement effects are about five times greater than referral effects, even though four times as many claimants are referred but not placed, as are placed.

We feel that a careful comparison between our referral effect estimates and existing experimental evidence on the value of job search assistance would be very useful to determining whether our 2.2-week estimate is unreasonably high. Our quick review of the differences between PLX services studies here and the types of job search assistance studied using random-assignment designs suggest to us that the benefits are much closer to $25 million per year than to $11 million. Unfortunately, we lack experimental evidence that can provide a precise estimate of the effect of measurement bias on our estimates. Thus, we have presented a plausible range for our estimates.

However, we can further refine our estimates using information from Davidson and Woodbury's simulation of the effect of PLX services on overall employment and unemployment in Washington State presented in Chapter 6. Their analysis suggests that our 1.7 benefit-cost ratio should be reduced to 1.4. This reduction occurs because about 20 percent of the benefits gained by job seekers who obtained PLX referrals came from crowding out job seekers who did not obtain referrals but who would have found out about these jobs without PLX aid, secured interviews, and possibly been hired.

This simulation, which used Westat's measures of PLX effectiveness, also suggests that the crowding-out effect is dispersed across tens of thousands of workers. The negative effect, therefore, is extremely small per capita, amounting to a loss of about 2.5 hours of work per person. Overall, the positive effect of PLX activities far outweighs the negative effect and leads to a reduction in the average duration of job search. This reduction creates a small but measurable increase in employment and a decrease in unemployment. These changes benefit society at large by increasing the total output of goods and services and benefit employers by helping them fill vacancies more quickly.

In summary, our Washington State analyses suggest that the benefits from PLX direct placement services are at least 1.4 times the cost of helping claimants. The analyses also suggest that the benefit-cost ratio was considerably greater during the economic recessions that occurred in the early 1990s when extended benefit programs were in place.

Nonexperimental Referral and Placement Results for Claimants from 1995 Oregon Administrative Data

The final component of our work was to replicate the Washington State claimant analysis using Oregon administrative data covering claimants. The Oregon Employment Department carried out all the data processing for this project to our specifications. The key results, shown in Table 2, are that in 1995, claimants placed by the Oregon PLX were unemployed 4.6 fewer weeks than they would have been if they had only obtained the information associated with being referred; claimants who obtained the information associated with being referred were unemployed 1.1 fewer weeks than they would have been had they not been referred (and mostly not obtained any PLX service).

While the per-person effects were considerably smaller for Oregon than for Washington, the total benefits were similar because Oregon referred and placed far more claimants. The higher referral and placement rates were entirely unexpected because in 1995 Washington had about 50 percent more job vacancies than did Oregon. However, Oregon employers listed a much higher proportion of their vacancies with local PLXs. We believe that Oregon PLXs were able to secure so many listings because state funds were used to boost PLX spending to roughly the same level as Washington's despite receiving 50 percent less in Wagner–Peyser and other Federal funds.

As shown in Table 2, we estimate that Oregon PLXs spent about 38 percent of its budget on claimants, compared to 25 percent by Washington PLXs. Because it took more resources for the Oregon PLXs to make referrals and placements, Oregon's benefit-cost ratio is considerable less than Washington's. However, we feel that it would highly worthwhile to include the effect of additional PLX services and work-test enforcement in the analysis. A more comprehensive analysis might boost the total benefits of Oregon PLX expenditures to bring the benefit-cost ratio up to Washington's level. Indeed, Oregon's per-incident effects could be smaller than Washington's because the comparison group has been positively affected by services and procedures that were not included in our analysis. Moreover, this analysis might suggest ways to further increase benefits by altering the mix of services. For example, the analysis we have completed suggests that shifting resources to give more attention to claimants with long durations of unemployment might substantially increase benefits.

Our confidence in the Oregon results could be greatly improved by using a mail survey with telephone followup to identify job seekers who were unable to obtain interviews because jobs (or interview slots) were already filled. Also, the Oregon administrative data appeared to incompletely cover claimants and their receipt of PLX services. Although we do not know the source of this problem, the identical problem occurred in the first 2 years covered by Washington administrative data. Thus, we believe that it may take about 2 years to properly test and organize the administrative data needed to estimate the benefits of PLX direct placement services. However, the experience we gained working closely with Oregon State officials suggested several ways to improve the data assembly process so that the type of data used in this study could be routinely collected and analyzed to provide meaningful ongoing feedback.

Summary of our Main Conclusions

Overall, these studies of PLX benefits have:

- Produced results suggesting that PLX direct placement services are highly cost-effective in two states;

- Developed procedures that can be used at a reasonable cost and on an ongoing basis to produce:

  - Highly accurate measures of placement effects that resemble those that would be derived from a random-assignment design;

  - Measures of referral effects that substantially reduce uncertainty about the plausible range of these effects;

- Shown that only a small fraction of the gains to referred PLX users were at the expense of crowding out job seekers who were not referred; and

- Demonstrated that it is feasible for state employment security agencies to produce value-added estimates, and that these estimates should be able to be produced within the same time frame and at about the same cost as measures that would not be nearly as useful for improving services and evaluating overall success.

While we have made substantial progress in determining ways to accurately estimate the value of direct placement services, ways that also could be used on an ongoing basis, we do not claim that our estimates are definitive. Indeed, it is our view that a lot more work needs to be undertaken to fully exploit the leads developed in this report.

Thus, the insights developed in the course of completing this study should be of value in completing a broader benefit-cost analysis of PLX services in Oregon, Washington, as well as Colorado, Massachusetts, Michigan, and North Carolina. The US-DOL also could use them to create meaningful performance measures for monitoring ongoing PLX operations in all states, and justify ensuring that all referrals and placements, even those made by fully automated job banks, are tracked with administrative data.

Chapter 1

1. INTRODUCTION AND BACKGROUND

This report summarizes a 3-year research project examining use of Public Labor Exchanges (PLXs) between 1987 and 1998 in the states of Washington and Oregon. The project was designed to determine the value of referrals and placements made by the PLXs established under the Wagner-Peyser Act. Our goals were to:

- Measure the effect of aid provided by PLXs to job seekers.
- Develop procedures that would routinely provide feedback to PLX program operators and state and Federal policymakers concerning PLX operations.

However, measuring these effects was a challenge because PLXs must provide universal access to their computerized job banks at PLX offices, public buildings such as libraries, and Internet sites. This open access precluded assessing PLX effectiveness using a random-assignment (experimental) design—the means technical experts agree yields the most valid measurements. Herein lay our primary challenge.

Universal access also encourages an exceptionally large population to use PLXs, a population whose motivations and needs vary. Thus, a second challenge was finding a means to examine PLX effectiveness for different groups of job seekers. Administrative data that currently are routinely collected provide a wealth of information about unemployment insurance (UI) claimants, but administrative data alone are much less adequate for examining the job search of PLX clients with spotty work records and those searching while employed.

In the end, we conducted the following four studies designed to produce reliable measurements without use of a random-assignment design:

1. A study of the effects of PLX placements made to all types of jobs in the first half of 1998 in Washington State using a mail survey that identified a naturally occurring group that resembled a control group derived from a random-assignment design.
2. A study of the effects of PLX referrals and placements made to UI claimants from 1987 through 1995 in Washington State using administrative data alone.
3. A study of the effects of PLX referrals and placements made to Oregon UI claimants in 1995 using administrative data. This study was designed to determine if the highly positive Washington study results were typical of those in other states and to

determine if a state employment security agency could develop the required database largely on its own.

4. A study of the possible adverse crowding-out effects of referrals and placements on Washington State claimants who were not referred to jobs, using a simulation model developed for a UI work-test experimental study.

The Washington State Employment Security Department provided administrative data for the first two studies and permitted us to collect the mail surveys under their auspices. In contrast, the Oregon Employment Department processed its administrative data to our specifications. Professors Davidson and Woodbury of Michigan State University carried out the simulation study using findings from study 2.

This report is organized as follows. In the remainder of Chapter 1, we provide background information that places the studies into an appropriate context and helps explain our choice of topics and techniques. First, we discuss how PLXs operate. We then briefly describe prior studies of PLXs and why interest has shifted from studies of training programs to studies of programs aimed at rapidly getting participants into jobs. In Chapter 2, we describe estimating techniques that can be used to resolve the formidable estimation problems in studying employment and training programs. We then discuss how we applied these estimating techniques and what results we obtained in examining the effect of PLX referrals and placements.

Chapters 3, 4, 5, and 6 detail the four studies listed above**.** This material is designed to allow technical experts to form independent judgments about the merits of the work and to provide details that may be of general interest. Chapter 7 summarizes our findings and key conclusions. Finally, Chapter 8 presents the comments of our expert panel and discusses their implications.

1.1 Overview of PLX Operations

Under the Wagner-Peyser Act (1933) every state receives Federal funds to run a PLX. The PLXs provide universal access to employers in listing job openings and to job seekers in viewing those listings, but the design of the PLX varies in important ways across the states. With two exceptions,[1] PLXs

[1] Colorado PLXs are run and staffed by county employees; in Massachusetts, three counties have PLXs run and staffed by a consortium of public and private nonprofit agencies, and one county has a PLX staffed by a private for-profit company.

are run by state employment security agencies (SESAs) using state employees. The PLXs are usually called either the state employment service (ES) or state job service (JS).

Wagner-Peyser outlays to individual states have been stagnant for the past 10 years at about $850 million per year. PLXs receive modest additional Federal funds to pay for special veterans programs and to collect labor market information for the Bureau of Labor Statistics. Some PLXs also receive contracts from local agencies (often using Federal funds) to provide services to clients of welfare and other employment programs. In some states, a major source of funding comes from state-financed programs to help UI claimants quickly return to work.

In studies 1 and 2, we examine the PLXs in Washington State. According to the survey, roughly 75 percent of Washington PLX-referred job seekers used computers at job service centers to identify promising listings. Fourteen percent obtained referrals through phone calls made by staff members who found job matches through use of a computerized search engine, 6 percent were referred by calling a PLX 800 number to learn that PLX computers had found suitable openings by matching information supplied by the job seeker to information supplied by employers, and 5 percent viewed PLX listings over the Internet using their own computers or computers at libraries or similar public places.

We estimate that in over 90 percent of the cases, a staff member worked with the job seeker to review his or her qualifications for promising openings and then provided contact information so that the job seeker could directly apply for those jobs. In some cases, staff assisted job seekers to identify more suitable matches. If the job seeker was not in the office when the match was made, staff usually would assess registrants' suitability for the match and provide contact information over the phone. In the remaining 10 percent of the cases employer contact information was included with the listings, and no further contact with staff was needed.

We also examined PLXs in Oregon in 1995 where visits to job centers also were the primary means for job seekers to identifying promising listings. However, Oregon did not have an 800 number for call-ins and was much less likely than Washington to have staff search listings and then notify job seekers when a match was made. Thus, it appears that a higher proportion of referrals was obtained by office visits in Oregon than Washington.

In 1995, Oregon PLX staff also provided contact information after interviewing job seekers. This made it easy to record each referral to jobs, and equally important, track placements resulting from the referrals. Thus, we could use administrative data from both states to identify referrals and placements made to each job seeker.

Many states moved from systems like Washington's and Oregon's of 1995 because these systems required high levels of staff involvement. Oregon and other states are now using systems where staff play much less of a role and self-service use of computers has become the primary means to obtain contact information. However, we are not aware of any state other than Oregon that requests job seekers enter identifying information at the point they request computerized contact information. Without this identifying information, it is very difficult to track who receives direct placement services and the outcomes stemming from their use. In our view, failure to collect identifying information jeopardizes the development of low-cost systems to effectively manage PLX operations.

PLXs also provide additional services to job seekers and employers. Job seeker services include providing workshops designed to help job seekers effectively find jobs on their own and resource rooms that provide the following: (a) access to word-processors to prepare resumes; (b) faxes and telephones to communicate with employers; (c) newspaper want ads; (d) access to Internet job banks; (e) a library dedicated to job search and career planning. PLXs also provide information about the availability of social services, including vocational training and special services to veterans.

Services to employers include the following: (a) assisting in tailoring wages and qualifications specified in job listings to local labor supply conditions; (b) allowing employers to conduct interviews at PLX facilities; (c) using PLX staff to recruit workers for specific firms; (d) conducting job fairs; and in some cases, (e) allowing employers to directly view job seekers' registration information or resumes. PLXs also collect and disseminate labor market information designed to help both employers and job seekers set reasonable expectations about the likelihood of matching workers to jobs at various wage rates.

Last, but far from least, PLXs ensure that UI claimants are adequately seeking employment. In most states, claimants are required to register with the PLX. In addition, states routinely call claimants into PLX offices to: (a) attend job search workshops; (b) review the adequacy of job search; and (c) develop individualized job search plans. Washington State recently adopted a unique program to routinely match claimants' qualifications to job orders, notify claimants when a match has been made, and have claimants follow up on that notification as part of the weekly telephone procedures used to establish continued claim eligibility.

Claimant services may be provided by staff paid either with UI or ES funds. Because UI and ES staffs usually are cross-trained and located in the same offices, the funding source is largely irrelevant. Also, these services have expanded in recent years because of Federal requirements to profile claimants and call in those most likely to exhaust benefits to receive job search assistance. Service also has expanded because employers in most states have put pressure on SESAs to relieve labor shortages by reducing claimants' duration of unemployment.

The effect of attendance at workshops and receipt of other mandatory services as part of profiling and work-test enforcement programs differs from the effect of voluntary direct placement and other services. Because failure to comply with the requirements of mandatory programs can lead to the denial of UI payments, these programs often lead to claimants stopping benefit collection but not returning to work. Thus, sometimes these services simultaneously have the positive effect of reducing the UI taxes paid by employers and have the negative effect of reducing the income of claimants. In contrast, voluntary direct placement services simultaneously help claimants and other job seekers find suitable jobs more quickly, and help employers to fill job vacancies more quickly to reduce their UI tax burdens.

1.2 Context for this Study

This study measures increases in job seekers' earnings and reductions in UI payments due to PLX direct placement services—making referrals and placements through use of job banks. We feel such emphasis is appropriate because, as the name PLX suggests, maintaining a universal system for employers to list job openings and for job seekers to view those listings is the feature that distinguishes PLXs from other government employment and training programs and absorbs most of PLXs' resources.

Also, little information about the value of direct placement services exists. More attention has been paid to measuring the benefits of other Federal employment and training programs. As a result, we have better, but by no means perfect, assessments of the returns to vocational training and job search assistance workshops provided under the Job Training Partnership Act (JTPA) and similar funding sources.

This lack of attention to direct placement services partly reflects the view that other employment and training programs have higher payoffs. It also is partly due to the unusual difficulty in obtaining accurate measurements of PLX programs. As noted earlier, PLXs must provide universal access. Thus, they cannot be studied through use of a random-assignment design that experts agree provides the best measures of program performance.

The new work presented here directly builds on one of the author's earlier research studies (Jacobson 1993) examining the effect of a special Washington State-funded program designed to reduce the unemployment duration of UI claimants through expanded use of PLX referrals. That study built on a similar study of Katz and Jacobson (1994) using UI and ES administrative data from Pennsylvania.

That study, in turn, used as its starting point the only benefit-cost study of PLXs funded by the U.S. Department of Labor (US-DOL) prior to 1998. It is particularly noteworthy that although this study conducted by Johnson et al. (1984) was published in the *Journal of Human Resources*, its positive conclusions were not widely known by policymakers, and because the results were not derived from an experimental design, it is not regarded as definitive by researchers.

Indeed, a major impetus for the Katz-Jacobson study was to more rigorously assess the commonly held "inside the beltway" view that PLX services were of little value**.** The best evidence of the low opinion of PLXs is that twice since 1980 the Secretary of Labor has led an effort to devolve the JS to the states. In both cases, a notice in the Federal Register was posted describing why poor JS performance justified such action**.**

An analysis of the information cited to support the view that PLXs performed poorly suggested that the evidence, although factually correct, did not come close to demonstrating the value of PLXs was low (Jacobson 1989). In particular, the negative evidence did not take in to account job seekers' tendency to turn to PLXs for aid after exhausting other means of finding work or not having access to other means in the first place. Thus, in the absence of PLX services, PLX users would be expected to have worse job search outcomes than apparently similar nonusers to whom they were compared. In particular, the Katz-Jacobson study cited above and the work presented here both show that claimants using PLXs have worse job search outcomes than nonuser claimants even when a wide range of preunemployment characteristics is taken into account.

The correlation between unmeasured attributes that affect job search outcomes and use of PLXs introduces what is called "self-selection bias." Such bias is the chief obstacle to making accurate assessments of most employment and training programs. Usually, self-selection bias leads to underestimation of program effects because problems finding and/or holding good jobs is what triggers program use. However, some comparisons lead to overestimation because job seekers using PLXs may be more interested in returning to work than those to whom they are being compared.[2]

Similarly, the ETA-9002 statistics routinely reported by the US–DOL's Employment and Training Administration and cited extensively in the Federal Register notices to support the view that PLX performance was deteriorating over time did not take into account reductions in funding relative to the size of the labor force or changes in the characteristics of PLX clients that made them more difficult to place at jobs. But most basically, these attacks did not consider whether or not the benefits of the services outstripped the costs, which should have been the primary criterion for judging the worth of the program.

However, PLXs have been held in low esteem largely because of a faulty comparison between PLX job placement and the Job Training Partnership Act (JTPA) job training and placement outcomes. Programs funded under JTPA used as a primary measure of success the "entered employment rate." This is the percentage of participants employed 90 days after they complete the program. The primary measure used by PLXs is the "placement rate." This is the percentage of individuals who registered with the PLX and who were placed at jobs to which they were referred. For JTPA dislocated workers, the entered employment rate often exceeds 85 percent, while for PLX users, the placement rate is often only about 6 percent.

Policymakers have often ignored the fact that the two measures are not comparable. For example, Representatives pointed out this flaw in response to Congressional testimony provided by the U.S. DOL [U.S. House of Representatives, 1989]. Indeed, applying the JTPA entered employment rate measure to PLX users produces rates that usually equal, and often exceed, the JTPA results. Perhaps of even greater importance, policymakers ignored the fact that PLXs placed roughly 10 times more individuals than JTPA programs. Yet, JTPA's budget was 50 to 100 percent greater than was that for PLXs. Moreover, PLXs placed many JTPA participants, and overall placed more individuals eligible for JTPA programs than did JTPA.[3]

[2] Chapter 2 discusses these and other measurement issues in far more detail. That chapter's key point is that only through use of a properly executed random-assignment design or natural experiment can analysts be sure self-selection and other biases are eliminated. However, once experimental evidence is available, that evidence can be used to develop valid nonexperimental estimation techniques.

[3] PLXs can place only 6 percent of registrants and still place many more individuals than JTPA programs because PLXs have universal access, while JTPA programs were able to limit participation and spent about 10 times more than PLXs per participant. PLXs, therefore, had hundreds of times more registrants than JTPA programs had participants. This is the case even when registrants and placements are limited to those made to individuals eligible for JTPA programs.

Nevertheless, during the 1980s, there was a tendency to extol JTPA programs over PLX programs. JTPA was viewed favorably in large part because it had much more resources to spend per person and, therefore, had the potential to do more good to those served. However, an additional factor in its favor was that JTPA was a highly decentralized program that established partnerships with local businesses, community colleges, and political leaders. In contrast, PLXs were exclusively run by state governments and state employees.

In the early 1990s, several factors combined to reduce enthusiasm for programs such as JTPA, which primarily provided relatively high cost training, and to increase interest in programs such as those created by PLXs, which provide "job search assistance" (JSA). A key factor was that several well-conceived analyses of dislocated worker programs indicated that on average the benefits of JSA were about as high as those of job training programs, but JSA costs from one-third to one-tenth as much as training.

A second factor was that as the economy gained strength during the 1990s, most job seekers could quickly find employment and later use their earnings to pay for training that would further enhance their careers. Also, it was recognized that community colleges provided a broad array of training that was easy to access and affordable by almost all employed individuals. Thus, there was less need for Federal programs to directly contract for these services.

A third factor was that as worker dislocation spread beyond blue-collar manufacturing in the "rust-belt" because of downsizing and mergers, the country needed a broad, low-cost program of assistance rather than one focused so strongly on job training.

As a result, government policy began to shift from "train first" to "work first." Today, work-first approaches are central both to welfare reform and to the restructuring of U.S.–DOL employment and training programs under the Workforce Investment Act (WIA). Of paramount importance, WIA mandates the formation of partnerships between Wagner-Peyser-funded PLXs and local workforce programs established under JTPA in developing One-Stop Career Centers and managing the full range of employment services.

In summary, changes in labor market conditions and the needs for public assistance created an openness to rethink how public aid can best be provided. At the same time, research became available that suggested that, with few exceptions, low per-capita cost programs primarily designed to help clients rapidly find jobs would be as effective as high per-capita cost programs that provided individualized assessments and training. In this environment "work-first" programs became highly attractive over "train-first" programs. It, therefore, made sense to create One-Stop Career Centers under WIA that would use PLXs funded under the Wagner-Peyser Act to provide direct placement services—referrals to jobs through the use of job-banks—to all clients, and reserve the more expensive individualized services funded under WIA to clients who could not be sufficiently helped through job placement alone.

A natural byproduct of this major policy shift is a new interest in assessing the strengths and weaknesses of the PLXs, which have not been studied for about 20 years. This study was designed to provide that assessment.

Chapter 2

2. MEASURING THE RETURNS TO PLX DIRECT PLACEMENT SERVICES

We begin this chapter by describing the options for accurately measuring the returns to PLX services given to job seekers, especially direct placement services—referrals to jobs through the use of job banks. We then describe the techniques we used to produce our measures. Next we provide an overview of our main results and their limitations. This discussion is primarily designed to help all types of readers understand what we did, why we did it, and what we learned. Subsequent chapters present details of the work needed for technical experts to independently judge the strengths and weaknesses of the studies.

To measure the value of PLX direct placement services we need to compare the job search outcomes of individuals who are referred by PLXs, which are directly observable, to what would have happened to those referred in the absence of PLX use, which is not directly observable. Producing this measure of returns to PLX direct placement services shares most of the formidable estimation problems as measuring the effect of other government employment and training programs.[1] In particular, we need to remove "self-selection bias." This bias arises because individuals who use a particular government service may appear to be identical to nonusers based on a host of observable characteristics, but they actually have problems finding work (or holding jobs) that are very different from those of nonusers, problems that strongly affect reemployment outcomes and are extraordinary difficult to measure with readily available data.

The extensive literature on program evaluation points to three basic methods to accurately determine what otherwise would have happened, in the absence of PLX use (see, for example, Bell et al., 1995).

One method is to establish a **control group** by denying the program's service to a group of willing participants who qualify for the service. This method is also commonly called a random-assignment design. A properly executed random assignment design creates a group of individuals who are identical to those receiving services in every way that affects their job search outcomes, with the exception that they did not receive help from the given program.

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[1] One key difference that makes measuring the effects of PLXs easier than measuring the effects of many other programs is that PLX effects are evident within 26 weeks from the receipt of the service. In contrast, assessment of the effects of the returns to training and other lengthy treatments requires 3 to 5 years.

The second method is to obtain sufficient information about program participants and nonparticipants to identify the difficult to quantify characteristics that affect participants' probability of entering the program and their subsequent employment outcomes. If sufficient information is available, it is possible to use multivariate statistical techniques to hold constant the relevant differences between participants and nonparticipants.

The third method is to identify some naturally occurring factor that precludes or limits program entry, but does not otherwise affect employment outcomes. This factor can be used as an "instrument" to create the equivalent of a control group. This "instrumental variable" technique has been used successfully in an evaluation of the Job Corps where distance from the Job Corps center served as the "instrument." However, under most circumstances, this technique is very difficult to apply.

Although all three methods can produce accurate results, there is powerful evidence that a random-assignment design is needed to validate the other two methods. In particular, there are many equally plausible ways to estimate program effects without using a random-assignment design. These techniques produce different results, but without a random-assignment study to use as a benchmark, there is no way to determine which technique produces accurate results. However, once a random-assignment design is successfully implemented, it is possible to develop a technique that produces results matching those from a random-assignment design. (These issues are thoroughly discussed in LaLonde 1986.)

2.1 Measures Based on a Mail Survey

In this report, we present results based on the second two methods, one comparing participants to nonparticipants, and the other using a natural control group (identified through use of a mail survey). We start by describing our most recent research based on a pilot effort to use a mail survey to:

1. Determine why referrals do not lead to placements, and thereby, identify job seekers who were **denied** the opportunity to be interviewed for jobs listed with PLXs to which they were referred. This group closely resembles a control group generated from a random-assignment design.

2. Obtain detailed information about the job search of PLX users prior to being referred to PLX job openings—information that is absent in administrative data, but could identify factors that affect PLX use and job search outcomes.

3. Determine how long job seekers were unemployed at the point they received PLX aid and how long it took them to find jobs—information that is only present in administrative data for unemployment insurance (UI) claimants, but is essential to measure the value of referrals and placements. Thus, having this information allows expansion of the analysis to all types of PLX users.

The denials noted in point 1 did not result from a plan to randomly deny services, but PLX managers and staff uniformly report that most were an outcome of the lag in removing listings from job banks after jobs are filled or after the employer has sufficient applicants. Also, officials uniformly report that PLX screening is sufficient to ensure that almost all referred job seekers have the credentials to warrant being interviewed.

Because few, if any, of these denials were a result of employers rejecting PLX referred applicants, they provided an opportunity to obtain unbiased results based on a natural experiment. Once we have such results we can then use them as benchmarks to (a) assess the accuracy of nonexperimental estimates generated using administrative data, and (b) determine which variables in the administrative data and mail survey improved the accuracy of the nonexperimental measures.

This analysis has four main limitations. However, only one of these limitations would be difficult to surmount in future work. First, the mail survey generated a relatively small sample of usable responses and did not represent the universe of referred PLX users. Thus, some of our estimates have large confidence intervals, and may not reflect the effect of PLX use on the average job seeker. (The results may lack external validity.) This is a highly surmountable limitation because a large, representative sample could have been secured through use of telephone followup, at a cost of about $30 per completed interview. We did not use telephone followup in this case because we felt that the method's inherent worth should be demonstrated before making the required investment.

Second, we did not obtain information that could distinguish job seekers who were denied interviews because of lags in removing job listings, from those whom employers rejected because of the candidates' shortcomings. While PLX officials told us that employer rejections prior to interviewing are rare, it would be desirable to know for certain how often they occur. This information could be secured in future studies by (1) using administrative records to determine if placed individuals obtained referrals well before individuals who tried but failed to obtain interviews, (2) calling at least some employers to determine if there were lags in removing listings or if they routinely prescreen candidates over the phone, and (3) asking PLX users additional questions that could help determine if employers screened them out. For example, we could have asked whether employers acquired any information such as resumes, applications, or answers to pertinent questions asked over the phone that could be used to screen candidates.

Third, there is nothing preventing the comparison group of referred but not placed job-seekers from obtaining additional referrals and ultimately being placed as a result of a PLX referral. In contrast, a pure experimental design would prevent control group members from obtaining services from the program being examined.[2] Our inability to prevent subsequent placements creates a quandary. Including subsequently placed job seekers in our comparison group certainly leads to underestimation of the value of placements. But excluding them also causes results to deviate from those from a pure experimental design. The magnitude and direction of the difference probably cannot be determined.

In this study, we limited the incidence of subsequent placements by selecting for comparison with each placed job seeker, a nonplaced job seeker whose most recent referral was to the same job listing. If that was not possible, we selected a nonplaced job seeker who was referred to the same job and who did not obtain another referral within the subsequent 4 weeks.

Asking PLX users about their most recent referral, a referral that was separated from others by a considerable period, or most recent placement is useful in a mail survey. Because these incidents of use stand out, it is relatively easy for respondents to recall the details of these events. Moreover, the distorting effect of this selection process probably is small because only about 10 percent of those referred in any 4-week period are placed in the subsequent 4-week period. Nevertheless, it would be worthwhile to obtain additional information in any future mail survey with telephone followup by randomly selecting a comparison group among all those referred to job orders (even those placed at other jobs within 4 weeks), and then determining how big the difference is if comparisons are limited to those not placed within 4 weeks.

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[2] Although a “pure” random-assignment design would avoid providing services to control group members, most actual experiments suffer from some control group contamination because efforts to prevent program use are not totally successful. An even bigger problem is that control group members may get the same service from an alternative source. For example, in the National JTPA study, some control group members, who were told during initial screening that they would benefit from training, obtained identical training from other sources, sometimes with the help of JTPA staff. As a result, great care needs to be taken in explaining that experimental evaluations measure the value of the program, not necessarily the value of the treatment.

Fourth, the "treatment" we measured was "having the opportunity to interview for a job" rather than "obtaining information that could improve job search outcomes." This is an important limitation because (a) it is highly likely that viewing PLX job listings provides job seekers with useful information about the state of the market, even if it does not directly lead to a placement, and (b) tens of thousands of job seekers view listings and obtain other aid, but only 1 out of 5 are placed.

Both personal observation by the authors and information supplied by PLX staff indicate that it is common for job seekers to repeatedly use PLXs until new jobs are found and spend an hour or more during each visit using computerized listings and other resources provided by PLXs to determine the characteristics of available jobs that best match their skills and interests. Indeed, almost 95 percent of survey respondents noted that they would use PLX services again to find jobs, and more than 85 percent said PLX direct placement services were useful or very useful. The positive responses were correlated with success in being placed and securing interviews, but even those who did not followup referrals or were unable to secure interviews had highly favorable views.

Thus, it is reasonable to conclude that the information secured in the process of obtaining a referral would improve job search outcomes. This improvement could arise from helping the PLX users to (a) set reasonable expectations about the pay and other attributes of vacancies they would accept, (b) determine what search techniques are likely to lead to finding these vacancies, and (c) obtain interviews to jobs not listed in the PLX job bank, but obtained from newspaper want ads, web sites, or direct inquiries to employers listed in directories at the PLX or its web site.

However, there is no obvious way to obtain an experimental estimate of the value of PLX information that does not lead to a placement. What is needed is to identify a group of nonusers who would have liked to visit a PLX but were unable to do so. One could envision denying access to PLX offices (and web sites), but rules about universal access and inability to oversubscribe the services preclude implementing such a design. Also, there does not seem to be any naturally occurring group that resembles a control group. What may be possible is using an instrumental variable technique based on distance from a job service center, but accessibility may only be an issue in rural areas.

Probably the best hope of securing the type of information needed from a random-assignment design would be to examine the returns to PLX-like services—inexpensive screening, counseling, and job search assistance—that often are a prelude to the provision of relatively expensive training or intensive counseling and other types of support from programs that are not required to provide universal access. Unfortunately for our work, studies of these employment and training programs focus on measuring the return from expensive treatments while ignoring the potential benefits from their PLX-like services.

A noteworthy example of how studies that use random-assignment designs ignore the value of PLX-like services is the $23 million National JTPA Title II study (Bloom et al., 1993). In that study, a control group was created through random assignment after eligible volunteers received the screening needed to assign them to appropriate treatments. Focusing on the benefits of appropriate high-cost treatments is important. But had randomization occurred **not only** at the time job seekers were assigned to treatments, but also at application time, we would have obtained highly useful information about the value of low-cost treatments and the nature of selection bias.[3] Similarly, random-assignment studies of treatments given to UI claimants also might have provided useful information about the value of PLX-like services and the nature of selection bias had they used a two-stage randomization procedure.

Our objective is not to criticize other evaluations, but to point out that natural experiments and random-assignment designs cannot be used to accurately measure the value of the information provided to job seekers by viewing listings and obtaining counseling from staff that does not necessarily lead to placement. Thus, the next best alternative would be to obtain information for similar services delivered by programs that can use random-assignment designs. Given that such information is not available, our only alternative is to use the best possible nonexperimental techniques to provide a range of plausible estimates. This analysis is presented in Section 2.2.

2.1.1 Summary of Key Findings from the Mail Survey

1. About one-third (34.4 percent) of job seekers referred by PLXs tried but failed to obtain an interview. Information from PLX staff suggests that most of these individuals applied too late to obtain interviews because jobs (or interview slots) were already filled. While additional evidence is needed to remove individuals who were screened out by employers or failed to obtain interviews for other reasons, this evidence is sufficient to conclude that it should be possible in the future to identify a large group of job seekers that closely resemble a control group derived from a random-assignment design.

[3] It is possible that excluding prescreening effects leads to substantial underestimation of the value of JTPA Title II program benefits. The returns to classroom vocational training vary enormously depending on selection of curricula and whether the trainee excels in school. Therefore, helping potential participants to intelligently decide between taking the best available job and entering a training program could have large positive effects on the returns to Title II spending (and large benefits to the potential clients).

2. About one-third (28.1 percent) of job seekers referred by PLXs did not try to secure interviews. This group should be excluded from the comparisons designed to assess the value of a placement because these individuals had no chance to be placed as a result of the referral.

3. About one-fifth (22.2 percent) of job seekers referred by PLXs were not given job offers, rejected job offers, or did not show up for work after accepting offers. Estimates of the job search outcomes of these individuals can be subtracted from the outcomes of all job seekers who tried but failed to obtain interviews. The resulting measure describes what would have happened to those who were placed if they had been unable to secure interviews. It is the difference between that measure and what actually happened to those placed that most accurately measures the value of a placement. Also, this difference can serve as benchmark for assessing the bias in comparing the outcome of placements to the outcome of referrals that do not lead to placements.

4. About 50 percent of job seekers referred by PLXs had little, if any, employment in the year prior to obtaining referrals. Measuring the effect of placements for this group with administrative data, but without the survey information, is extremely difficult because it is impossible to determine how long those referred were searching for work before and after receiving the referral.

5. Analysis of our survey data indicates that being placed reduces the unemployment duration of job seekers **with considerable work experience** in our sample, most of whom are UI claimants, by about 7.2 weeks, and that of job seekers **with little work experience** in our sample by about 3.8 weeks (relative to viewing listings, obtaining a referral, but not being placed).

6. That the measured effect of being placed was almost twice as great for individuals with substantial work experience than for those with little work experience is consistent with the view that job seekers with substantial experience (a) will search for long periods until they locate jobs that are well matched to their skills and interests, and (b) can afford to prolong the search because they receive UI benefits, have more savings, and have other financial resources on which to draw.

7. At this point we do not know how close the estimates presented in point 5 come to the true effects for the universe of placed job seekers. The primary problem is that our survey sample was small and the response rate across different job seekers was not uniform. Also, some job seekers in the comparison group may have been denied interviews because employers felt that their credentials were not as good as those already interviewed. Nevertheless, we feel that the results are sufficiently accurate to produce a first-approximation of the overall value of placements.

8. Using the estimates in point 5, which only imperfectly reflect the average reduction in unemployment duration, estimates of post-unemployment earnings from administrative data, and published statistics on the number of placements we estimate that the total increase in earnings due to placements was about $45 million in 1998. This represents about 1.7 times the total cost of running the Washington State PLXs in that year.

9. We feel that the highly respectable benefit cost ratio of 1.7 is a reasonable ball-park figure because we omit from the benefit calculations the value of information obtained from viewing listings and interacting with staff that did not lead to a direct placement. Experimental evidence on job search assistance, which we believe is less potent than PLX aid, suggests that those services reduce unemployment by about 1 week. Even if referral effect were this small, the total effect would be large because 4 out of 5 referred PLX users are not placed. We also omit from the calculation the crowding out effect of PLX aid, which negatively affects other job seekers. However, our estimates suggest that the reductions in benefits due to crowding out are considerably smaller than the increases in benefits had we included the value of referrals not leading to placements.

10. Comparing the results in point 5 to those derived from use of nonexperimental techniques with the same dataset suggest that the nonexperimental techniques substantially underestimate placement effects. Unfortunately, the sample was not large enough to directly test the specific model used in Chapters 4 and 5. Such a test would be of great value, and would be possible if we assembled a larger database.

11. Information about why referrals did not lead to placement and the duration of job search before and after referral were very useful. However, the other detailed information did not substantially improve our estimates over those we obtained with administrative data alone.[4]

Our key conclusion is that use of a mail survey with telephone followup has the potential to provide highly accurate estimates of the effect of having the opportunity to interview for a job as a result of a PLX referral for all types of job seekers. We see no alternative but to use this method, at least until sufficient information is obtained to create benchmark results that can validate nonexperimental estimators. However, a survey cannot identify job seekers who resemble the control group needed to estimate the value of obtaining information from PLXs that do not lead to direct placements.

Surveys also can provide the information needed to improve measures of unemployment duration using administrative data alone. This information is especially important for using nonexperimental techniques to measure the effect of placements for nonclaimants.

[4] The additional mail survey data may not have affected our results because for every person in our sample placed at a job, we selected one person referred to the same job. This prematching may have greatly reduced the variation in our sample. In the near future, we will determine the effect of the prematching. This was not possible in this study because we lacked time to assemble the followup administrative information to make the relevant comparisons.

2.2 Measures Based on Administrative Data Alone

We used the 1998 mail survey to benchmark nonexperimental estimates of the value of being placed relative to being referred but not placed using very large samples of administrative data covering the job search of UI claimants. The administrative data and nonexperimental techniques were also used to measure the value of obtaining the information that leads to referrals relative to not obtaining referrals and usually not obtaining any PLX help. Our administrative databases included 328,815 separate spells of unemployment from 1987 through 1995 in Washington State, and 138,280 separate spells in 1995 in Oregon.

The nonexperimental results use administrative data to hold constant differences in key characteristics of claimants who were (a) placed at jobs to which the were referred by PLXs, (b) referred to jobs but not placed at those jobs, and (c) who did not obtain referrals (and mostly did not use any PLX services). The data permit us to hold constant demographic characteristics, prior work histories, UI benefit entitlements, and most important, unemployment duration. These results are restricted to UI claimants for the simple reason that the key variables used to measure job search outcomes and control for factors associated with PLX use are only available in the administrative data we used for claimants.

We make two different comparisons in the administrative data studies. We estimate the placement effect by comparing the job search outcomes of placed claimants to the outcomes of claimants who are referred but not placed, and we estimate the referral effect by comparing the job search outcomes of referred but not placed claimants to the outcomes of nonreferred claimants. The nonexperimental placement measures are quite similar to the results used to make the same comparisons with the mail survey. If anything, it appears that the nonexperimental results slightly underestimate the true effect. However, we would be more confident about this conclusion if the mail survey covered 1995, a year covered by both the Washington and Oregon administrative studies, and if the mail survey were larger and we were certain that it represented all claimants.

Unfortunately, we have no evidence from a random-assignment design or natural experiment to validate our estimates of referral effects, which represent the value of information obtained in the course of being referred. Thus, we have no means to determine precisely how much of the positive effect we observed represents measurement bias. However, we have several pieces of information that suggest the effects are positive First, experimental evidence on the effect of other forms of job search assistance suggests that those treatments decrease unemployment duration by about 1 week. The key issue is whether use of PLX job banks is likely to be more potent than the treatments studied using random assignment designs. We suspect that PLX treatments are more potent because most of the studies examine the effect of workshops that last only a few days. In contrast, most claimants who are referred but not placed persist in using PLXs over a long period, usually until they find a job by some other means, and spend hours looking at job orders and obtaining other information on each visit. Second, the fact that survey respondents who were not placed told us that they were highly satisfied with PLX services and that they would use those services again bolsters the view that those services are valuable.

Third, regression adjusting the duration of unemployment for those who are referred but not placed, initially substantially reduces the duration relative to the unadjusted mean. This reduction is strong evidence that those who are referred but not placed have characteristics associated with longer than average unemployment durations. Because selection bias usually leads to underestimation of program effects, the regression adjusting appears to substantially reduce that bias, but probably does not eliminate it entirely.

Perhaps of even greater importance, regression adjusting makes less and less difference as the duration of unemployment lengthens. This suggests that those referred but not placed and those not referred become more and more similar to each other as time passes. This is highly relevant evidence because the primary test that a random-assignment design is properly implemented is to show that the difference between simple means and regression adjusted means of the outcome variables are zero.

It also is worth noting that regression adjusting the duration of unemployment for those placed substantially increases the duration of unemployment relative to the unadjusted mean. This increase suggests that those placed have characteristics associated with shorter average durations of unemployment. That the adjustments are far larger than those for job seekers referred but not placed, but use the same set of control variables, suggest that more bias is present in the placement estimates, and that the regressions effectively remove much of that bias. However, we cannot be certain that this is the case. That the regression adjusted results resemble the results derived from the survey supports the view that much of the bias is eliminated, but is not definitive. To obtain more definitive results we need to obtain unbiased estimates from a natural experiment using a representative sample.

Despite having considerable evidence that referral effects are positive and some evidence suggesting that the nonexperimental techniques we used probably underestimate the true effect, we feel that the positive effects are so large relative to those derived from random-assignment studies of similar treatments that they cannot be considered definitive. We, therefore, present three estimates of the total benefits of direct placement services. The first uses the placement effect alone. The second adds half the referral effect to the placement effect, and the third adds all of the referral effect to the placement effect.

Because it is most unlikely that all of the observed referral effect is due to measurement bias, we are highly confident that the true effect is considerably greater than the placement effect alone. It is hard to believe that obtaining information from a PLX makes job seekers worse off. This is particularly so when according to our mail survey 92 percent of those referred but not placed would use PLX job banks and other services to look for jobs in the future, 36 percent claimed that the information they obtained was highly valuable, and another 34 percent reported that the information was valuable.

2.2.1 Summary of the Washington Results

Use of the exceptionally large Washington State administrative database for 1987-1995 provided highly positive assessments of the benefits of both referrals and placements. Overall, our most important conclusions are that:

1. On average, placements reduce the duration of unemployment by 7.7 weeks relative to being referred but not placed. This nonexperimental estimate of the per-incident effect of placements relative to obtaining referrals are similar to those generated from use of a survey. While several steps should be taken to increase our confidence that these results reflect those from a natural experiment, we feel that the bias in these results is small enough for the similarity to support the view that the nonexperimental estimates are reasonably close to the true values.

2. We produced plausible nonexperimental estimates of the per-incident effect of obtaining information in the course of being referred relative to not being referred, and usually not obtaining any PLX help, appear plausible even though we have no experimental evidence to serve as a benchmark. On average, our procedures produce estimates that referrals reduce the duration of unemployment by 2.1 weeks relative to not being referred. Even if measurement error accounts for half of this figure, the contribution to total benefits is large.

3. Estimates of the total benefits hinge on multiplying the per-incident effect times the number of incidents and average weekly post-unemployment earnings.

    a. The administrative data provided accurate estimates of the incidence of being placed and being referred once the state employment security agency decided to use these data for in-depth analysis. On average, PLXs refer about 25,000 claimants per year, of whom about 7,000 are placed.

b. Our procedure for estimating post-unemployment earnings is reasonably accurate, but could be greatly improved by applying techniques used with these administrative data in estimating the earnings losses due to job loss. Post-unemployment earnings of claimants average about $260 per week.

4. Estimates of the year-by-year variation in the pre-incident effect of placements and referrals as well as the total benefits derived from direct placement services are much in keeping with expectations. Total benefits are between 30 and 40 percent greater in the trough of a business cycle than near its peak.

   a. About one-third of the peak-to-trough reductions are due to reductions in the per-incident effect of placements and referrals as economic conditions improve. Placements, in particular, are more potent when good jobs are more difficult to find and extended benefit programs considerably lengthen the period over which UI can be collected.

   b. About two-thirds of the reductions are due to there being fewer claimants in total during prosperous periods.

5. The cost of running the entire PLX system in Washington is easily derived from budget figures. Those costs average about $25 million per year and vary very little from year to year. Sixty percent of the funds come from the Federal Wagner-Peyser allocation, 15 percent from Federal Veterans funding, and the bulk of the remainder from state funds earmarked to assist claimants. It is not nearly as easy to determine how much of the total goes to providing direct placement services, nor how much of those funds goes to aiding claimants. We use a ball park estimate of 35 percent for the portion spent on direct placement services going to claimants included in the benefit calculations.

6. We produce the following three different benefit-cost estimates:

   a. The lowest estimate assumes that the value of the information obtained in getting referrals is zero. Thus, this measure only includes the value of placements relative to being referred but not placed. This produces a benefit-cost ratio of 1.2 (using the 35 percent estimate of the portion of costs going to direct placement services and including 1987-89, and 1995 years for which the incidence of placements and referrals is substantially underestimated).

   b. A less conservative estimate assumes that half of the measured referral effect is due to measurement error. This assumption produces a benefit-cost ratio of 1.9.

   c. The least conservative estimate accepts the referral effect measure as being accurate. This assumption produces a benefit-cost ratio of 2.8.

   Because the per-incident cost of placements and referrals is very low, we feel that even the least conservative estimate is plausible. Key reasons for this view are that: (1) receipt of UI payments greatly restricts the types of jobs claimants are likely to accept before exhausting benefits; (2) some claimants would have great difficulty finding suitable work on their own after they have been jobless for several months; and (3) we included 1987-89 and 1995 years for which the incidence of placements and referrals is substantially underestimated. Thus, we think it is likely that placements and referrals will be particularly potent for claimants.

The trend in unemployment reductions stemming from placements and referrals obtained after successively longer periods of being unemployed strongly supports the view that, as unemployment duration lengthens, claimants have more and more difficulty finding jobs on their own. The per-incident effects just about double from between the 9th and 19th week of unemployment spells.

7. Separate estimates suggest that UI payment reductions equal about 22 percent of the gain in earnings. Payment reductions are lower because weekly payments are about one-half of weekly earnings and because payments end after about 30 weeks, on average. Thus, except during periods covered by extended benefits, reductions in UI payments are modest relative to the cost of providing direct placement services to claimants. Nevertheless, employers benefit both from the reduction in UI payments, which usually will reduce their UI tax burden, and from claimants' increased earnings. By lowering the cost of job loss, placement services reduce "the risk premium" workers would require to take jobs that are likely to end with permanent layoffs.

2.2.2 Summary of the Oregon Results

Like the Washington study, the Oregon study used an exceptionally large claimant database drawn from administrative records, and almost the same estimation techniques. However, there were important differences between the Oregon and Washington studies. First, the Oregon database covered only 1 year, 1995. Second, the data had not been previously used for a similar research project. Third, we did not conduct a mail survey in Oregon. Fourth, all the data processing was done by the Oregon Employment Department.

Our biggest problem stemmed from the fact that the data had not previously been used for similar research. Thus, we were able to determine that the Oregon person-level database did not cover all claimants and all services those claimants received, but we were unable to determine why that was the case. The problems we encountered were identical to those we had to overcome in working with similar data from Washington and other states. Thus, we are confident that eventually they can be overcome. Also of substantial importance, we learned a great deal about how to assist state employment security agencies to assemble, process, and use these highly valuable data on their own.

We used the Oregon database to obtain critical evidence about the per-incident effect of referrals and placements. Those results were derived from a model almost identical to the one used in Washington. However, we had to use published statistics about the number of referrals and placements to estimates of the total effect.

Measured referral and placement effects were smaller both per incident and in total in Oregon than in Washington in 1995. Our measure of being referred but not placed relative to not being referred indicates a reduction of unemployment by 1.1 weeks in Oregon compared to 1.6 weeks in Washington. Our measure of being placed versus being referred but not placed indicates a reduction of 4.6 weeks in Oregon, compared to 6.6 weeks in Washington.

However, major differences in the procedures used to provide services and monitor job search in Oregon versus Washington may explain why the results differed between these two states. In particular, Oregon spent more funds on assisting claimants than did Washington, and focused many of these activities in the first 10 weeks of each claimant's unemployment spell. Unfortunately, this focus may have substantially reduced the average effect because the per-incident effect is considerably higher after the tenth week in Oregon. Also, the difference in effects between Oregon and Washington is particularly great after the tenth week.

Perhaps of even greater importance, Oregon's procedures for calling in claimants to obtain referrals and have their job search plans reviewed may have led some Oregon claimants to search for work more effectively, even if they did not obtain referrals, and others to stop collecting benefits (become nonclaimants) if they were unable or unwilling to assiduously search for work. Thus, a major limitation of the Oregon analysis could be not including the effect of all types of services. This shortcoming can be overcome, but it probably would require use of an experimental design or analysis of the effect of Oregon procedures on the incidence of benefit collection.

Using only the per-incident placement effects cited above together with published statistics on the number of claimants placed produced total benefits of $15.4 million in Oregon, compared to $19.8 million in Washington. Moreover, we estimated that both states spent about the same amount on their PLXs in 1995; however, Oregon spent about 38 percent of the total PLX budget to aid claimants, compared to Washington's 25 percent. Thus the lower-bound benefit-cost ratio is 1.56 in Oregon, compared to 2.13 in Washington.

The least conservative estimate of total benefits in Oregon is $30.3 million versus $42.5 million in Washington for 1995. These estimates include referral effects that capture the value of information obtained in the course of being referred. The benefit-cost ratio is 3.06 in Oregon and 4.50 in Washington. Thus, in both states the least conservative estimate suggests that direct placement services received by claimants more than covered the entire cost of running the PLXs in each state in 1995.

Research presented in Chapter 6, however, suggests that crowding-out effects that adversely influence nonreferred claimants reduce the benefits-cost ratios by about 20 percent. On the other hand, the least conservative results omit the benefits from PLX services other than referrals and placements. The value of these services could be large, particularly in Oregon.

2.3 Concluding Remarks

The main focus of this chapter was to provide a nontechnical discussion of the estimation problems that must be overcome to produce accurate results. We, therefore, focused on how experimental and nonexperimental techniques can be used to reduce self-selection and other forms of measurement bias. We then summarized our key findings and some of the estimation problems encountered in producing the results for each of the three separate studies that measured per-incident referral and placement effects and related benefits to costs.

The first study used a Washington mail survey to produce estimates of placement effects for all types of PLX clients. These results are reasonably close to those that would be derived from using a natural experiment where placed job seekers were compared to job seekers who requested referral to the same jobs, but were unable to secure interviews because the jobs or interview slots were already filled. Importantly, while we do not consider our results to be definitive, we detail several steps that would likely eliminate shortcomings in the data that would allow us to produce definitive estimates.

The second study used Washington administrative data to estimate referral and placement effects for claimants alone. The nonexperimental estimation technique used produced placement effect estimates that were similar to those derived from use of the mail survey in the first study and the regression adjustments substantially reduced the effect relative to unadjusted means. We feel that this evidence, taken together, suggests that our estimates are close to the true effect, but improving the survey databases is necessary for us to judge more precisely how much bias remains.

Unfortunately, we could not produce any referral estimates based on any type of random-assignment design or natural experiment to use as a benchmark for assessing bias in our nonexperimental referral effect estimates. The best we could do is compare our results to those from random assignment designs applied to other types of job search assistance provided to claimants and other experienced workers. We recognize that our referral effects estimates are considerably larger than those derived from experimental studies. However, we feel that it is plausible that the support provided by PLXs is sufficiently longer lasting and more potent to account for the effect being larger. Also, we believe that residual selection bias is likely to cause our techniques to underestimate referral effects. Nevertheless, while we produce total benefit estimates based on assuming our estimates are unbiased, biases lead to overestimation equal to half the estimates, and all of the positive effect is due to measurement bias.

The third study used Oregon administrative data to examine referral and placement effects for claimants alone. That study produced results suggesting the effects were not quite as large as in Washington. However, other evidence suggests the effects are smaller mainly because Oregon requires claimants to participate in activities that increase incentives for claimants to search hard and effectively or stop collecting benefits.

The next three chapters present the technical details of the three studies that estimate per-incident effects. Chapter 6 presents the technical details of our estimate of crowding out effects. Chapter 7 presents an overall summary of our findings and policy-oriented conclusions that is meant to be understandable to individuals with little interest in the technical details.

Chapter 3

3. DETAILS OF OUR RESEARCH USING THE WASHINGTON STATE MAIL SURVEY

Up to this point our goal was to make clear in a general way what we did and why we did it by providing sufficient information about public labor exchanges (PLXs), the context for this study, the measurement problems, our results, and our conclusions. In this chapter, we provide the details of our analysis so that technical experts can independently judge the value of our work and others can gain a much deeper understanding of precisely what we did and what we found.

3.1 Design of the Survey

The primary purpose of the survey was to produce estimates of the value of placements that closely resemble those that would be derived from a random-assignment design. Those results could then be used to assess the accuracy of estimates derived from administrative data alone and develop ways to improve their accuracy.

In this chapter we use a natural experiment to produce separate estimates for claimants and other job seekers with a great deal of prior work experience and for job seekers with little prior work experience. We do this to complement results presented in Chapter 4 that use administrative data to examine PLX effects for claimants alone.

We also test the usefulness of data obtained from the mail survey that was not available in the administrative files but strongly affected decisions to obtain PLX services and job search to improve the accuracy of the nonexperimental estimates.

In our previous work, presented in Chapter 4, we compared all those referred but not placed to all those placed. As a result, many of those referred but not placed were referred to jobs that required very different skills and interests than those required by the jobs to which individuals were placed. Although the control variables derived from administrative data might adequately take these differences into account, we also test restricting the comparison group to job seekers referred to the same jobs to which the targets were placed.

An important by-product of selecting those placed and referred to the same job orders was that we also were able to add a great deal of information to the person-level files about the jobs to which sample members were referred. This information included whether the job was full time or part time, the hourly wage rate, the number of slots to be filled, the number of referrals requested, and the required experience, education, and age.

Delays in obtaining wage data prevented us from fully assessing the effect on our nonexperimental results of restricting referrals to jobs to which others were placed and adding job order data to the person level files. Also, somewhat to our surprise, we could not identify any variables, other than those in administrative data, that substantially affected our nonexperimental results. Thus, in our subsequent discussion we primarily focus on what we learned from the unique information about why referred job seekers failed to be placed.

We also considered covering three groups of job seekers in the survey: (1) those placed as a result of a PLX referral, (2) those referred but not placed, and (3) those not referred. Comparisons between group 1 and group 2 could provide an estimate of the value of being placed, given that a job seeker reviewed PLX job listings and obtained other information. Comparisons between group 2 and group 3 could provide an estimate of the value of obtaining information from a PLX that did not lead to a direct placement.

Ultimately, we decided to limit the survey to groups 1 and 2 because we recognized that this is the only comparison where we might be able to mimic results from a random-assignment (experimental) design. The natural experiment we could study was based on identifying referred job seekers who wanted to obtain a job interview but were denied the opportunity because the jobs were already filled. However, we could not identify a natural experiment that could isolate individuals who wanted to obtain information from PLXs but were denied that opportunity.

Additional factors affecting our decision to limit the sample were that (a) except for UI claimants, we had no way to obtain contact information for job seekers who did not visit PLXs and (b) much larger samples would be needed to produce statistically meaningful referral effect measures because those effects were likely to be small.

Another key design issue was whether to use telephone followup with a mail survey. This decision rested on whether the added information was worth the relatively high cost of using telephone followup. We knew that it would probably cost about $30 per completed interview with telephone followup, but that a mail survey could be sent out at about one-tenth that cost. Also, we were fairly sure that the response rate would be low from a mail survey, but we did not know precisely how low the response rate would be. Perhaps most importantly, we were uncertain about how useful the information would be that we obtained from the survey.

Because we were uncertain about both the value of the information we would collect and how low the mail response rate would be, we felt that it would be prudent to conduct a low-cost pilot study without telephone followup. We, therefore, decided that a sample of 3,000 placed job seekers and 3,000 referred but not placed job seekers would provide sufficient information from a mail survey at a reasonable cost. We thought that even if we got a very low response rate we would get returns from at least 500 placed job seekers and 500 referred but not placed job seekers. This sample would be sufficient to evaluate the methods we used.

We then developed a mail survey instrument to meet our goals by obtaining detailed information from referred job seekers about:

1. The reasons referrals did not lead to placement;
2. The duration of job search before and after securing a referral;
3. The methods used to search for work and how successful these methods were;
4. Recent employment history and prospects for being recalled or holding on to current jobs; and
5. Services received from the PLX other than referrals, such as participation in job search workshops and use of resource rooms.

The survey instrument is presented in Appendix A.

3.2 Implementation of the Design

To implement our design we:

1. Identified job orders to which there was at least one placement and one referral not leading to placement during the first 7 months of 1998.
2. Identified the subset of job orders where there was at least:
   a. One placement that was the most recent placement made to a job-seeker; and
   b. One referral that either was the most recent referral or the only referral made within 4 weeks.

Steps 2a and 2b improved the chances that respondents would have clear memories of these events and that contact information would be correct. A second important attribute of our procedures is that those selected because they were not placed as a result of a specific referral could not be placed within the subsequent 4 weeks as a result of another referral. If we did not take this step, we might have seriously underestimated placement effects.

Selecting the most recent placement had almost no effect because it was rare for a person to be placed more than once. Selecting the most recent referrals may have had some effect on the results. The sample was slightly altered because about 15 percent of those referred but not placed in one 4-week period would be placed subsequently and a true experimental design would have prevented those individuals from obtaining interviews. In the future, we could use a telephone survey to obtain information about the sequence of referrals that could determine the effect of alternative selection procedures. However, obtaining this type of information was not feasible in a mail survey.

It turned out that there were many job orders to which PLX users were referred, but no users were placed and some cases where users were placed, but no one referred was not placed. Overall, only 19 percent of all job orders received by Washington PLXs from January to July 1998 included at least one placement and at least one person referred to that job who was neither placed at that job nor referred to any other job listing within 4 weeks. However, those job orders accounted about 46 percent of all referrals and 29 percent of all placements.

We then selected a sufficient number of job orders to obtain a sample of 3,000 placements and 3,000 individuals referred but not placed. If for a given job order only one person was placed <u>or</u> one person was referred but not placed, we selected one person from each group. If there were three or more placed people and three or more referred but not placed people, we selected three people from each group. Otherwise, we selected two people from each group. We also oversampled job orders with multiple referrals and placements to facilitate the determination of how the value of placements differed with the characteristics of the job orders.

Our survey sample included only 7 percent of job orders, but those orders included just under 25 percent of all referrals and just over 15 percent of all placements.

3.3 Improving the Mail Response Rate

We expected a low response rate because job seekers referred by the PLX often had little prior work experience and were looking for relatively low-wage jobs that sometimes were temporary or part time. We, therefore, tested fancy packaging, cash incentives, and multiple mailings to improve the response rate at modest cost. One reason for conducting these tests is that we felt that states would be encouraged to employ these methods if we could find a low-cost way to obtain a response rate in the neighborhood of 50 percent.

Table 3-1 describes the response rates to our mailing. Part A of the table shows the results from the initial mailings, which tested three packages:

1. A "plain" mailing, which included a $1 bill and black and white survey, costing about $1.50 each;
2. A "fancy" mailing with postage stamps (not metered mail), multicolored instruments, inclusion of a special pen, and a $1 bill, all of which cost about $2.75 each; and
3. A mailing with added "money"—inclusion of a $5 bill instead of a $1 bill in the "plain" mailings, which cost about $5.50 each.

Column 2 shows that, compared to use of the plain package, the response rate from the fancy package was somewhat higher, and that the response rate from the added money package was considerably higher. However, the response rate was only 13.1 percent overall or 18.2 percent after adjusting for undelivered mailings.

The second mailing brought the unadjusted response rate up to 19.2 percent. As anticipated, the response rate and mail return rates were much lower from the second mailing than the first. Clearly, those most likely to respond already had done so before the second mailing was made.

Table 3-1. Mail Response Rates and Cost for Four Different Designs

	Number mailed	Response rate	Returned forwarding address:		Adjusted response rate	Unit cost	Total cost	Cost per response
			No	Yes				
	1	2	3	4	5	6	7	8
A. First Mailing								
1. Plain	2,500	11.1%	14.4%	17.3%	15.5%	$1.50	$3,750	$13.49
2. Fancy	3,000	14.0%	14.4%	14.2%	19.5%	$2.75	$8,250	$19.64
3. Added Money	500	17.2%	5.2%	2.8%	24.0%	$5.50	$2,750	$31.98
4. Total	6,000	13.1%	13.7%	14.5%	18.2%	$2.46	$14,750	$18.81
B. Second & Third Mailing								
5. Lottery	4,396	8.3%	5.2%	10.1%	9.9%	$1.50	$6,594	$17.97
C. All Mailings	6,000	19.2%	17.5%	--	23.2%	$2.05	$21,344	$18.54
Number of responses	1,151							

Notes:

1. In Section A the adjusted mail response rate (col 5) = responses (col 2) / [1 - average return rate (line 4)]. The actual return rate was not used because it was artificially lowered by inclusion of $5 cash in the "added money" mailing.
2. The third mailing was to 141 of those for whom we had forwarding addresses from the second mailing. We received 20 responses. (The third mailing was limited by the materials we had on hand.)

The bottom line of column 8 shows that the overall cost-per-complete was $18.54. This is relatively high for a mail survey because the response rate was low. Indeed, we suspect that the cost-per-complete from a telephone survey would not have been much higher because the response rate would have been above 75 percent. Even with a response rate of 50 percent, the cost-per-complete of a telephone survey would have been about $30.

Column 8 also shows that, although the response rate was higher with the added packaging and cash, the additional responses did not cover the additional cost. Thus, if the only goal was to obtain "n" responses, the plain package would be most cost effective. However, if the goal is to increase the representativeness of the responses, the added cost probably was justified.

An additional major problem with the package that included a $5 bill is that it reduced the mail return rate, which was 31.7 percent with the plain package, to a mere 8.0 percent. Further, only 2.8 percent of the packages with $5 bills were returned with forwarding addresses compared to 17.3 percent of the plain packages. This largely precluded us from reaching individuals who were sent $5 bills, moved, and left a mailing address.

3.4 Differences between Responders and Nonresponders

Overall, we obtained 1,115 usable responses from the 6,000 PLX-referred people to whom surveys were mailed. Fortunately, there were more than enough respondents to reach our primary objective of determining the value of the survey data. This is the case even if the respondent sample is not particularly representative either of all referrals or referrals to job orders where there was at least one placement and one referral not leading to a placement.

Nevertheless, understanding what characteristics made the survey recipients more or less likely to respond can provide information that should be kept in mind when analyzing the survey responses and help determine the best way to collect survey information from those referred by PLXs in the future.

Table 3-2 displays the results of an ordinary least squares regression using the dummy variable "responded-to-the-survey" as the dependent variable. Only variables that were statistically significant at least at the 15 percent level were included. Table 3-2 also includes for each value its mean, standard deviation, and the coefficient times 10 percent of the variable mean. This last measure shows how a 10 percent change in the variable would affect the response rate. The variables are presented in order by the size of this effect, with those with greatest effect presented at the top of the list.

Table 3-2. Regression on the Mail Response Rate

	Variable name	Regression Coefficient	Mean	Standard Deviation	coef x .1 x mean
1.	Placed	0.0829**	0.512	0.50	0.0042
2.	Race: white	0.0552**	0.743	0.44	0.0041
3.	Age in years	0.0021**	35.03	11.1	0.0023
4.	Gender: female	0.0586**	0.382	0.49	0.0022
5.	"Plain" initial mailing of mail survey	-0.0359**	0.421	0.49	-0.0015
6.	Months of experience asked for on job order	0.0013**	7.082	11.2	0.0015
7.	Job order requires less than high school education	-0.0633**	0.228	0.42	-0.0014
8.	Highest earning quarter in 1997	0.0000*	2775	3402	0.0014
9.	Referred in the first quarter of 1998	-0.0272**	0.490	0.50	-0.0013
10.	Job seeker must be available to work 40 or more hours	0.0191	0.676	0.47	0.0013
11.	Job seeker lives in the eastern part of WA	0.0316**	0.347	0.48	0.0011
12.	Job order Missing educational requirement	-0.0285	0.347	0.48	-0.0010
13.	Age 26-40 years	-0.0199+	0.467	0.50	-0.0009
14.	Job seeker must be willing to relocate for job	-0.0298*	0.260	0.44	-0.0008
15.	Hourly wage missing in highest earnings quarter of 1997	0.0251*	0.298	0.46	0.0007
16.	Job seeker lives in Seattle	-0.0297*	0.212	0.41	-0.0006
17.	Agricultural industry classification from SIC	-0.0638**	0.090	0.29	-0.0006
18.	Minimum acceptable wage less than $6.60	0.0245	0.137	0.34	0.0003
19.	Applicant must be greater than 18 years old on job order	-0.0474*	0.059	0.24	-0.0003
20.	Handicapped or disabled program	0.0705*	0.031	0.17	0.0002
21.	Job seeker attending school	0.0425	0.037	0.19	0.0002
22.	College education required on job order	0.0727	0.013	0.12	0.0001
23.	Minimum acceptable wage greater than $13.20	0.1121+	0.008	0.09	0.0001
24.	Years of experience missing from job order	-0.3700+	0.001	0.03	0.0000

Note: ** indicates coefficient is significant at the .01 level.

* indicates coefficient is significant at the .05 level.

\+ indicates coefficient is significant at the .10 level.

The variable with the largest effect is whether the person was placed as a result of a referral. The survey sample included equal numbers of individuals placed and individuals referred but not placed. These results suggest that individuals who regarded the referral as a “significant” event were more likely to respond. We suspect that this is because they could more easily remember those events, and possibly because they wanted to show their appreciation for the services they received. This view is reinforced by the strong positive effect of being referred to full-time job on the probability of responding.

Demographic and work-history factors also are of considerable importance. Whites are much more likely to respond than people of other races; women are more likely to respond than men; older individuals are more likely to respond than younger individuals; and individuals with more education are more likely to respond than those with less education. Also, even holding constant demographic characteristics, individuals with high earnings are more likely to respond than those with low earnings, and those applying to jobs that require more experience are more likely to respond than those applying for jobs requiring less experience.

These results are consistent with the responses to a broad range of government-sponsored surveys indicating that individuals who are more likely to trust the government are more likely to respond and that individuals with an educational background that makes responding to a pencil and paper survey easy are more likely to respond.

Finally, the statistical analysis confirms that individuals were less likely to respond to the plain packages than to either the fancy packages or the packages containing $5 bills. Also, individuals who were asked about events that occurred further in the past were less likely to respond.

3.5 Design of the Analysis

Our primary goal was to determine the difference in job search outcomes between what happened to those who received PLX referrals (and other information), which is directly observable, and what would have happened to those same people had this aid not been obtained, which is not directly observable. Researchers agree that the best means to determine what would have otherwise happened is to create a control group by denying services (at least for a short period) to a randomly selected group of individuals requesting services. Comparisons between targets, who volunteer to receive a service, and controls, who volunteer but are denied the service, produce unbiased results because all the factors influencing post-service outcomes except service receipt itself are distributed identically between the targets and controls.

In contrast, other measurement approaches require use of statistical techniques to take into account differences between the comparison group and target group in factors influencing both the decision to obtain a service and job search outcomes. Because many of these factors are very difficult to measure, there is always some doubt about whether the results are unbiased.

Unfortunately, it is not feasible to use a random-assignment design for estimating the effect of PLX services. First, the program requires universal access. Second, even if services could be denied, it would require major modifications to the way PLXs currently operate to effectively deny access. Ordinarily, PLX services are exceedingly easy to obtain from office visits, visits to public sites such as libraries that have computers containing the PLX listing, and even from home computers through the Internet.

Nevertheless, it is worth describing the steps involved in obtaining a placement from a PLX referral to determine (a) what would be learned from alternative random-assignment designs, and (b) whether there are naturally occurring groups that resemble those that would be created by a random-assignment design.

Table 3-3 (which also was presented in the Executive Summary) breaks the process of obtaining a placement in Washington State into 10 steps. If we were going to set up a random assignment experiment to measure the value of PLX direct placement services, our top priority would be to create a control group by randomly preventing some job seekers at Step 2 from reaching Step 3. In other words, we would deny job seekers who have decided that they want to use PLX services the opportunity to do so.

This could be done in practice if PLXs were run like a health club and required an ID card to gain access to PLX offices and entry into PLX computers. Essentially, we would randomly tell people who wanted to join the club that no more applications were being taken.

Table 3-3. Job Search Path from Deciding to Search for Work Through Deciding to Use PLXs to Placement by a PLX

	Steps to surmount	Path ending outcomes
Step 1.	Unemployed worker decided to search for work	a. Recalled by former employer b. Retired c. Dropped out of labor force
Step 2.	Job seeker decided to use PLX	No desire to use PLX
Step 3.	Job seeker gained access to PLX	Unable to use PLX because services were unavailable or too difficult to access
Step 4.	Looked at PLX listings	Found no suitable jobs
Step 5.	Found promising listings	Decided not to interview for those jobs
Step 6.	Tried to obtain an interview	a. Job or interview slots filled b. Employer rejected job seeker based on prescreening
Step 7.	Obtained interview	Did not receive an offer
Step 8.	Received an offer	Rejected offer
Step 9.	Accepted offer	Did not show up for work
Step 10.	Showed up for work	Placed by PLX system

We would then compare the outcomes of those who used PLX services to those who wanted to use the services but were denied the opportunity to do so. In this case, the treatment is "obtaining information about the job market." In some cases this treatment leads to direct placements and in other cases leads to acquiring information that improves decisions about (a) the types of jobs to search for, (b) when to accept job offers, and (c) how to search for those jobs.

Thus, all job seekers who visit PLXs would be potential beneficiaries. Even those who do not find any suitable jobs in PLX listings could benefit because not seeing a suitable job would indicate to these job seekers that finding a suitable job may be difficult and it might be reasonable to lower their expectations. Thus, randomization at any other point could lead to underestimation of the value of PLX services.

Randomization at the point job seekers decide to use PLXs is likely to be particularly valuable because (a) job seekers who choose not to use PLXs are likely to have different employment prospects from those who choose to use PLX services, and (b) obtaining detailed data that could identify those differences is likely to be particularly difficult. Indeed, it appears that the evidence suggesting PLX services are of little value suffers from not adequately taking into account PLX use being triggered by lack of access to better sources of information or having exhausted those sources.

In addition to obtaining a benefit estimate for the respondent sample as a whole, it would be highly desirable to determine how benefits differ for those who are referred but not placed versus those who are referred and placed. Having this information could greatly improve PLX operations.

If, for example, the net benefit of placements is more valuable than providing information that does not lead to placements, then more effort should go to tasks that lead to placements, such as working with firms to obtain listings. However, if the reverse is true, more effort should go to helping job seekers use the information to locate jobs on their own. Moreover, the relative merits of information and placements could vary across job seekers with different characteristics.

Also, knowing more about the value of different elements of the help provided by PLXs could strongly affect how we measure the returns to PLX services. In particular, if we knew that almost all of the benefits of the PLX accrue to those directly placed, we could focus on measuring the value of placements by using administrative data to compare job search outcomes of those placed to those referred but not placed.

Even if we could use a random-assignment design, it is unclear whether we could devise a way to separately measure the value of the information in cases where the job seeker is, and is not, directly placed. Part of the problem stems from placement being an outcome, not a treatment. However, we could ask employers to randomly reject candidates to whom they otherwise would offer jobs at Step 8. But it is dubious that doing this would give us the information we need because the rejection would likely affect the job search of those rejected, and thereby, distort the results.

A more viable alternative is to randomize after the job seeker has obtained information from a PLX, obtained a suitable referral, and decided to follow up that referral by arranging an interview. For example, we could ask employers to tell job seekers assigned to the control group that they have filled all interview slots at Step 6 (whether or not this is the case). The results of randomization at this point would allow us to measure the value of interviewing for a job, given a suitable referral is found.[1] We could then subtract this value from the net benefit to all PLX users to get an estimate of the value of viewing job listings and getting related information, but not getting an interview.

Rather than use experimental evidence, we also could compare the outcomes of individuals who were placed following receipt of a job offer to individuals who rejected job offers or did not show up for work after accepting an offer, holding constant observable characteristics. However, there is a strong likelihood that those who rejected an offer would have superior outcomes because they were taking preferable jobs they found on their own, and this information might not be readily observable.

Of enormous importance, the mail survey can provide the information we need to identify a naturally occurring group of job seekers who resemble a control group derived from randomization at the point they try to set up an interview after receipt of referral contact information. The naturally occurring group is not identical to a control group because (a) group members could subsequently obtain interviews from PLX-supplied contact information, and (b) some members would have been rejected by employers.

However, the positive value of the subsequent interview might offset the distorting effect of not being able to secure interviews at all, and PLX officials at all levels concur that, with rare exceptions, PLX screening is sufficient for employers to want to interview referred job seekers. Officials believe that lags inherent in removing listings easily can result in large numbers of job seekers obtaining referrals too late to secure interviews. Thus, unless the jobs are filled, the vast majority of referred job seekers who request interviews can secure interviews.

By identifying referred job seekers who reach each <u>Step</u> from 4 through 10, the mail survey can also provide the information needed to determine the bias in using administrative data to compare the job search outcomes of those placed to those who would have been placed had they tried to obtain an interview. In estimating the bias, we make the simplifying assumption that only those placed were affected by interviewing. If this was the case, we could use information about the number and outcomes of job seekers who were interviewed but not placed (and therefore reached Step 7 but not Step 10) to estimate what would have happened to those stopping at step 6a had they secured interviews but not been

[1] As would be the case in being rejected for a job, being told interviews were unavailable also could affect subsequent job search. For example, job seekers might wrongly assume suitable jobs are scarcer than they previously believed. Such distortions limit the value of a random-assignment design when the information itself is valuable. Even when expensive, long-lasting treatments are being offered, it is possible to provide information that affects the behavior of control group members. For example, in the National JTPA Title II evaluation, screening the members of the control group to the point they are willing to volunteer for a training program could increase the likelihood they would seek out this treatment through other sources.

placed. Subtracting out the outcomes of those interviewed but not placed from the comparison group outcomes allows us to estimate the effect of being placed relative to individuals who would have been placed had they secured interviews.

In reality, information obtained during the interviews could affect the distributions and outcomes of all those interviewed. Because this effect cannot be distinguished from the effect of being placed, the simplifying assumption makes it easier to calculate the effects and discuss the results. Also, because it likely that the effect of having the information from an interview is small relative to the effect of being placed, the assumption produces results that are close to being exactly correct anyway.

Of greatest importance, the net benefit estimates are not affected in any way because we multiply what probably is a slight overestimate of the effect on those placed times the number placed. The alternative would be to multiply an accurate estimate of the interview effect by the total number interviewed.

Thus, even though we cannot use a random-assignment design, we focus on using the mail survey to learn as much as we can about (a) the returns to interviewing for a job based on a PLX referral and (b) biases in using administrative data to measure the value of placements, given the PLX user is referred.

3.6 Estimators Used in the Analysis

Estimator 1a is the primary estimator of interest. It measures the difference between those interviewed and those who tried but failed to obtain an interview. The estimator produces results similar to those from a random-assignment design to the extent (a) those who tried to secure an interview were unable to do so because the jobs were filled or no more interviews could be scheduled, and (b) those who tried to secure an interview to one job listing, did not try, or were unable to secure interviews to other job-openings.

(1a) Interview Effect = W(intv) - W(ntintv)

where:

W(j) = weeks from referral to job is found for group j

for j equal to:

intv = interviewed as a result of a referral (reached Step 7)

ntintv = not interviewed, but tried to obtain an interview (stopped at Step 6)

In comparisons designed to measure the bias in estimates derived from use of administrative data alone to measures that more closely resemble those from a random-assignment design we use the following two estimators.

Estimator 1b is used with mail survey data. It is derived by multiplying estimator 1a by the inverse of the proportion of job seekers who were interviewed who are placed. This estimator is based on the assumption that only placed individuals benefit from information obtained in the interviews. An alternative way of looking at the assumption is that for job seekers who are not placed, the effect of receiving the treatment, which in this case is obtaining an interview relative to trying but failing to obtain an interview, is zero.

(1b) Placement Effect-1 = INTV/PLC x [W(intv) - W(ntintv)]
where:
INTV = total number interviewed
PLC = number placed
W(j) = weeks from referral to job is found for group j
for j equal to:
intv = interviewed as a result of a referral
ntintv = not-interviewed, but tried to obtain an interview

Estimator 2 is used with administrative data that identify placements and referrals but do not identify why job seekers were not placed.

(2) Placement Effect-2 = W(plc) - W(ref)
where:
W(j) = weeks from referral to job is found for group j
for j equal to:
plc = placed (reached Step 10)
ref = referred, but not placed (reached Step 5, but not Step 10)

Because estimator 1b produces results similar to those from a random-assignment design, we will use the difference between estimator 1b and estimator 2 as an indicator of the bias in estimator 2. Before looking at the empirical evidence, we can get an idea of how the two estimators differ conceptually by breaking each down into its constituent parts based on the following six referral outcomes:

1. PLACED Were placed at the job to which they interviewed. (Reached Step 10);
2. NO-SHOW Did not show up for work, after accepting an offer. (Stopped at Step 9);

3. NO-ACCEPT Did not accept a job offer, after receiving an offer. (Stopped at Step 8);

4. NO-OFFER Did not obtain a job offer, after an interview. (Stopped at Step 7);

5. NO-INTERVIEW Tried, but failed, to obtain an interview. (Stopped at Step 6); and

6. NO-TRY Did not try to obtain an interview. (Stopped at Step 5).

Figure 3-1 shows how the six outcome groups fall into the target and comparison groups inherent in each of the two estimators.

	Estimator 1	Estimator 2
1. PLACED		target-2
2. NO-SHOW	target-1	
3. NO-ACCEPT	(INTV)	
4. NO-OFFER		comparison
5. NO-INTERVIEW	quasi-control	(REF)
6. NO-TRY	- -	

Figure 3-1. How referral-outcome groups are organized to produce Estimators 1 & 2

Estimator 1 is based on the ratio of the number placed (group 1) to the number interviewed but not placed (groups 2, 3, and 4); and the difference in weeks unemployed among those interviewed (groups 1, 2, 3, and 4) and those who tried but failed to obtain an interview (group 5). Estimator 2 is based on the difference in weeks unemployed among those placed (group 1) versus those referred but not placed (groups 2, 3, 4, 5, and 6).

The two estimators are identical if all job seekers referred by PLXs are either placed (group 1) or tried but failed to obtain an interview (group 5). The two estimators also are the same if weeks unemployed is identical among all those who were not placed (groups 2, 3, 4, 5, and 6).

Conversely, the differences between the two estimators are greatest when: (1) large numbers of individuals are in groups 2, 3, 4, and 6, (2) weeks unemployed among members of groups 2, 3, 4, and 6 are similar to each other; and (3) weeks unemployed among members of groups 2, 3, 4, and 6 are very different from weeks unemployed among members of group 5.

However, the difference between the two estimators could be small even if large numbers of job seekers are in groups 2, 3, 4, and 6, and their weeks unemployed differ sharply from those for members of group 5. This is the case because the differences in weeks unemployed across groups 2, 3, 4, and 6 could offset each other. For example, offsets would be complete if the number of individuals in groups 2 and 3 equaled the number in groups 4 and 6, weeks unemployed among members of groups 2 and 3 were far above weeks unemployed among members of group 5, and weeks unemployed among members of group 4 and 6 were equally far below weeks unemployed among members of group 5.

Although we cannot predict the outcome of the analysis, the mail survey should be able to secure the information needed to produce the two estimators and pinpoint the source of the bias in estimator 2.

3.7 Empirical Analysis

To determine the difference between estimator 1b and estimator 2, we used a sample of 587 job seekers who (a) returned the mail survey, (b) had a clear memory of what happened after they were referred to a PLX job opening, (c) provided all the information needed to place the person in one of the six referral outcome groups, and (d) responded to the survey questions needed to estimate the number of weeks from referral to employment.

Table 3-4 shows how each of the four restrictions reduced the usable sample. By far, the biggest reduction stemmed from the low mail response rate. Small reductions stemmed from individuals not having a clear memory of the referral and from individuals not providing the information needed to determine the referral outcome.

Table 3-4. The Effect of Restrictions on the Sample Used to Study Weeks of Unemployment

1. Number of surveys sent out	6,000
2. Number of surveys returned	1,115
3. Number of respondents with clear memory of referral	954
4. Number with clear memory who provided information about referral outcome	915
5. Number with referral outcomes who provided information about weeks of joblessness	587

The reduction stemming from not providing information about weeks between referral and employment is relatively large, but most of those who did not respond were members of the placed group (outcome 1) who were not asked about weeks unemployed. Overall, the sample is large enough for assessing the difference between the two estimators, and the low response rate does not affect the internal validity of this test.

Column 1 of Table 3-5 shows how the 587-person sample is distributed across the six outcome groups. Over 56 percent of the sample were placed (group 1). This number is high because we oversampled those placed, and those placed had a higher response rate than those referred but not placed. Column 2 shows the distribution in the underlying population had we not oversampled those placed. Only about 15 percent of those referred were placed at the jobs to which they were referred.[2]

Table 3-5. Distribution of the Sample and PLX Referred Job Seekers by Referral Outcome

	Full sample (1)	Natural distribution (2)	Distribution if interviewed (3)
1. Placed at the job to which they interviewed.	56.2%	15.0%	40.0%
2. Did not show up for work, after accepting an offer.	2.4%	4.6%	12.4%
3. Did not accept a job offer, after receiving an offer.	2.9%	5.6%	15.0%
4. Did not obtain a job offer, after an interview.	6.3%	12.2%	32.6%
5. Tried but failed to obtain an interview.	17.7%	34.4%	
6. Did not try to obtain an interview.	14.5%	28.1%	
	100.0%	100.0%	100.0%

Note: The full sample includes roughly four times more placed individuals than the natural distribution because the mail survey was stratified to include equal numbers of those placed and those referred but not placed.

The most interesting (and important) information in column 2 is that 34.4 percent of the individuals referred to a given job opening tried but failed to obtain an interview. This means that there are plenty of individuals in the quasi-control group needed to produce estimator 1. Also, of considerable interest is that 28.1 percent of individuals obtaining referral information did not even try to obtain interviews.

In addition, column 2 shows that 12.2 percent of the sample failed to receive an offer following an interview. But almost as many job seekers, 10.2 percent, turned down offers or failed to report to work after accepting an offer.

[2] The 15 percent placement figure applies to the one referral that was used to select the sample. Since most job seekers are referred to several openings the probability of placement after using the PLX is much greater than 15 percent. For members of our sample, it probably is around 30 percent.

Finally, column 3 shows that the probability of being placed given the job seeker obtained an interview is quite high, 40 percent, and that the probability of having the opportunity to take the job is a very respectable 67 percent (40+12.4+15.0).

To our knowledge, this is the only information available about the reasons PLX users fail to be placed at jobs to which they were referred. Being unable to secure interviews is an important factor accounting for 40 percent of the failures. As noted earlier, as far as we can determine almost all of the failures to set up interviews are due to either the jobs already being filled or no more interview slots being available at the point the job seeker contacts the employer. Not receiving a job offer is relatively rare, accounting for only 14 percent of the failures. The remaining 46 percent of failures are due to factors under the job seekers' control, such as not attempting to set up interviews, rejecting offers, and not showing up for work after accepting offers.

As noted in Section 3.7, there would be no difference between estimator 1b and estimator 2, if all referred job seekers were either placed or failed to obtain an interview. That just under 50 percent of those referred are in these two categories suggests that there is plenty of opportunity for estimates based on comparisons of those placed to those referred but not placed to be biased.

Column 1 of Table 3-6 shows the mean value for weeks of unemployment (following receipt of a referral). This variable is the actual reported weeks of unemployment for those who had found jobs when surveyed. For those who had not found jobs, we assumed that they would be unemployed for 18 weeks. This is a conservative estimate because it is only slightly longer than the actual average elapsed time between when individuals received referrals and when they returned their surveys.

A key reason for using 18 weeks for those who had not found new jobs is that about one-half the sample had very little work experience in the year before obtaining the referral. Thus, it is likely that many of the individuals who did not find work within 18 weeks stopped looking. In contrast, we used a much longer maximum jobless duration in our earlier work restricted to UI claimants because: (a) many claimants would not take jobs paying little more than their UI benefits until those benefits were exhausted after 26 or more weeks, (b) claimants had a much stronger attachment to the labor force, and (c) most claimants unemployed for many weeks were searching for work in a period where jobs were much more difficult to find than in 1998.

Table 3-6. Weeks of Unemployment by Referral Outcome

	Mean (1)	Mean relative to no-interview (2)	Regression coefficient (Std-error) (3)
1. Placed at the job to which they interviewed.	4.22	-5.95	-4.70 (0.94)
2. Did not show up for work, after accepting an offer.	9.78	-0.39	-1.25 (2.38)
3. Did not accept a job offer, after receiving an offer.	4.41	-5.76	-6.52 (2.16)
4. Did not obtain a job offer, after an interview.	10.73	0.56	1.65 (1.59)
5. Tried, but failed, to obtain an interview.	10.17	0.00	- -
6. Did not try to obtain an interview.	8.09	-2.08	-1.36 (1.20)

Column 2 of Table 3-6 shows the mean weeks of unemployment for each outcome group relative to the mean for group 5, those who tried but failed to obtain interviews. These figures are derived by subtracting 10.17, the mean for group 5, from each value in column 1. These results show that those placed (group 1) were unemployed about 6 weeks less than those unable to secure interviews (group 5). This is a strong indication that placements substantially reduce the duration of unemployment.

Those who did not accept an offer (group 3) also had substantially fewer weeks of unemployment. This indicates that job seekers who rejected offers tended to have other jobs lined up. Although not nearly as large (nor expected), job seekers who did not try to obtain interviews (group 6) also had fewer weeks of unemployment. This too suggests that they did not pursue the openings because they had other job offers in-hand or on the horizon.

Finally, those who accepted jobs but did not show up for work (group 2) had slightly fewer weeks of unemployment than those who tried but failed to obtain interviews (group 5). In addition, as might be expected, those who did not receive job offers took longer than those in group 5 to return to work.

Column 3 of Table 3-6 shows the regression adjusted differences in weeks of unemployment relative to those who tried, but failed to obtain interviews (group 5). These results are based on regressions that use 112 administrative variables and 59 survey variables. After considerable experimentation, we decided to use all the variables that were available in the database, that reflect factors observable before the referral date. (Appendix B describes the regression equation and the variables in considerable detail.)

The regression-adjusted values are similar but not identical to the unadjusted mean values shown in column 2. Importantly, the regressions increased the duration of unemployment relative to group 5 for individuals who (a) were placed (group 1) by 1.25 weeks, (b) did not receive offers (group 4) by 1.09 weeks, and (c) those who did not try to obtain interviews (group 6) by about 0.72 weeks. These results indicate that members of these three groups had characteristics that made them more likely than average to find work quickly.

In contrast, the regressions decreased the duration of unemployment for individuals who did not show up for work (group 2) by 0.86 weeks, and did not accept offers (group 3) by 0.76 weeks. This is evidence that members of these two groups had characteristics associated with longer than average durations of unemployment.

The information in Tables 3-5 and 3-6 is sufficient to produce estimator 2, which compares the duration of unemployment of those placed (group 1) to those referred but not placed (groups 2, 3, 4, 5, and 6). As shown in section A of Table 3-7, the regression adjusted duration of unemployment for those placed is -4.7 weeks (relative to those who did not secure interviews), and the value for those not placed is -0.7 weeks (based on summing the regression adjusted means times the proportion of individuals in each of the remaining groups). Thus, estimator 2 indicates those placed were unemployed 4.0 less weeks than those referred but not placed.

Table 3-7. Differences Between Estimator 1 and Estimator 2

A.	Estimator 2—Placed versus Referred, but not Placed		
	1.	Duration of those placed relative to those unable to obtain an interview	-4.7 weeks
	2.	Duration of those referred, but not placed, relative to those unable to obtain an interview	-0.7 weeks
	3.	Duration of those placed relative to those referred but not placed (line 1 – line2)	-4.0 weeks
B.	Estimator 1—Interviewed versus Unable to Obtain an Interview		
	4.	Duration of those interviewed	-2.5 weeks
	5.	Fraction of those interviewed who were placed	.40
	6.	Duration of those placed, relative to quasi-control group of those unable to obtain an interview (-2.5/.40)	-6.2 weeks
C.	Difference between Estimator-1 and Estimator 2		
	7.	Difference in weeks	-2.2 weeks
	8.	Difference in percent (line 7 / line 3)	55.0 percent

The information in Tables 3-5 and 3-6 also is sufficient to produce estimator 1b, which compares the duration of unemployment of those placed (group 1), to a measure of what it would have been had those individuals been unable to secure an interview. To obtain this estimate, we first calculate the duration of unemployment of all those who obtained interviews (relative to the duration for those who tried, but were unable to secure interviews). As shown in section B of Table 3-7, this is -2.5 weeks (based on summing the regression adjusted means times the proportion of individuals in groups 1, 2, 3, and 4). However, because only 40 percent of those in the quasi-target group were placed, we multiply -2.47 times 2.5 (1/.4) to obtain -6.2 weeks, which is the effect of being placed using estimator 1b.[3]

As shown in section C of Table 3-7, the difference between estimator 1b, which resembles the results of a random-assignment design, versus estimator 2, which is about the best one can do in the absence of information about why referrals do not lead to placements, is an additional 2.2 weeks (6.2-4.0) decrease in the duration of unemployment. This is a 55 percent increase in the measured effect of a

[3] The following discussion attempts to explain in a more intuitively appealing way why estimator 1b produces larger estimates than the simple difference between those placed (group 1) and those unable to secure an interview (group 5). Had those in the quasi-control group (group 5) secured interviews, only 40 percent would have been placed, roughly 33 percent would have not received an offer and had longer durations of unemployment by 1.7 weeks, 15 percent would have rejected offers and had shorter durations by 6.5 weeks, and 12 percent would have had shorter durations by 1.3 weeks. The average duration for the 60 percent of the members of the quasi-control group who would have ended up in groups 2, 3, and 4 would be 0.6 weeks shorter than the average for the quasi-control group as a whole. However, if this was the case, the average duration of those members of the quasi-control group who would have been placed would have to be 1.2 (60/40) times longer than average. This increases the average difference between those placed and those who tried but failed to obtain interviews who would have been placed by 1.5 weeks to 6.2 weeks in total.

placement and suggests that estimates that lack information about reason for not being placed substantially under-estimate true effects.

3.8 Placement Effects for Job Seekers with Strong and Spotty Work Records

The initial comparison of the two estimators provides useful insights into the bias in an estimator based on the difference between those placed versus those referred but not placed. Thus, it is worthwhile to use estimators 1b and 2 to: (1) assess the bias in our previous attempts to estimate the effect of placements on claimants, and (2) produce estimates of placement effects for groups that we did not study in our previous work.

We examined several different ways to divide the sample into a group that strongly resembled UI claimants and a group that was very different from claimants. The division that made the most sense was to separately examine individuals with strong work records, whom we defined as having three of four quarters of employment in 1997, versus spotty work records, whom we defined as having 0, 1, or 2 quarters of employment in 1997.

This split resulted in an almost equal number of job seekers being in each division. UI claimants made up about 65 percent of the strong work record history group, but only about eight percent of the spotty work record group. The two groups sharply differed from each other, while within each group members were quite similar. In particular, most members of the 3 or 4 quarter group had high earnings and earnings in all four quarters, while members of the 0, 1, or 2 quarter group had low earnings and earnings in zero or one quarter. Most importantly, the estimation procedures produced highly similar results for all claimants and nonclaimants with 3 or 4 quarters of earnings, while results for individuals with 3 or 4 quarters were quite dissimilar from results for the 0, 1, or 2 quarter group.

Even before we obtained results for each group separately, we had evidence that results for the two groups would differ—by far the best determinant of duration of unemployment (next to being placed) in our regressions were the number of quarters a person was employed in the year before obtaining a referral.

Table 3-8 shows that the distribution of members in the spotty and strong work record divisions across the six referral outcomes is remarkably similar. The biggest differences are that individuals with spotty work records are (1) 4.8 percentage points more likely to not try to obtain interviews, (2) 4.7 percentage points less likely to be unable to obtain interviews, and (3) 1.3 percentage points less likely to not obtain a job offer.

Table 3-8. Distribution by Referral Outcome for the Spotty and Strong Work-Record Groups

	Spotty Work Records 0, 1, 2 quarters of employment	Strong Work Records 3, 4 quarters of employment	Difference
PLACED	15.0	15.0	0.0
NO SHOW	4.0	4.3	-0.3
NO ACCEPT	6.2	5.9	-1.0
NO OFFER	13.4	15.0	1.3
NO INTERVIEW	30.9	33.1	4.7
NO TRY	30.3	26.7	-4.8

The lower probability of following up a referral by obtaining an interview suggests that individuals with spotty work records are capable of finding comparable jobs on their own and/or less interested in finding work. The higher probability of obtaining interviews and offers suggests that individuals with spotty work records are applying for listings with many openings that do not require specialized training, experience, or education.

However, the most important result is that the differences are small. Thus, differences in weeks of unemployment for individuals in each outcome group will be the primary determinant of differences in the measured effect of placements between those with spotty and strong work records.

3.8.1 Referral Outcomes for Individuals with Spotty Work Records

Table 3-9 shows differences in weeks unemployed by referral outcome and work record. First, we compare the differences in regression adjusted weeks to the simple means. In both cases, the differences are relative to the value for group 5, individuals who tried but failed to obtain interviews.

Table 3-9. Reductions In Weeks Unemployed By Referral Outcome for Job Seekers With Strong and Spotty Work Records

	Spotty 0, 1, 2 quarters			Strong 3, 4 quarters			Difference strong versus spotty	
	reg (1)	mean (2)	diff (3)	reg (4)	mean (5)	dif (6)	regression (6)	mean (8)
1. placed	-4.10 (2.03)	-6.21	2.11	-4.22 (1.38)	-5.66	1.44	-0.12	0.54
2. no show	-5.64 (4.49)	1.12	-6.76	-2.66 (3.92)	-1.99	-0.67	2.98	-3.11
3. no accept	-2.97 (4.26)	-5.99	3.02	-7.35 (3.41)	-5.70	-1.65	-4.38	0.29
4. no offer	4.29 (3.43)	-0.94	5.23	0.44 (2.10)	2.30	-1.86	-3.85	3.24
5. no interview	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
6. no try	-3.02 (2.39)	-0.61	-2.40	2.62 (1.82)	-3.52	6.15	5.64	-2.91
7. grp 2-4	0.20	-1.88	-2.08	-1.96	-0.39	1.55	-2.16	1.41
Natural Exp.								
8. est-1b	-3.80	-9.02	$\boxed{-5.22}$	-7.17	-6.25	0.93	$\boxed{-3.37}$	2.76
9. grp 2-6	-1.03	-0.72	0.31	0.27	-1.17	-1.44	1.30	-0.45
Non-exper.								
10. est-2	-3.07	-5.49	-2.42	-4.50	-4.49	-0.01	-1.43	1.00
11. est-2 – 1b	0.73	3.53	2.80	$\boxed{2.67}$	1.76	-0.91	1.95	-1.76

NOTE: Standard errors are in parentheses.

Columns 1, 2, and 3 of Table 3-9 show that regression adjusted weeks unemployed for individuals with spotty records diverge sharply from the unadjusted means. Group 2, no-shows, shift from a mean of 1.12 weeks of unemployment longer than group 5, no-interviews, to a point estimate of 5.64 weeks less than no-interviews. In contrast, group 4, no-offers, shifts from a mean of .94 weeks less than no-interviews to 4.29 weeks more.

That regressions produce a large decrease in weeks for no-shows indicates that this group has characteristics associated with difficulty finding work. Nevertheless, those individuals who are no-shows appear to have another job lined up. The large increase in weeks for no-offers indicates that this group has characteristics associated with easily finding work. Thus, the no-offer group may have been qualified for the jobs, but indicated that they were unenthusiastic about working for the given employer.

The remaining differences are smaller than for no-shows and no-offers. Those who did not accept offers were unemployed 5.99 fewer weeks than no-interviews, but regression adjustment reduced the difference to 2.97 weeks less. Although no-accepts have characteristics associated with a somewhat quicker return to work, the results suggest that not accepting an offer is due to knowing about a more promising opportunity.

Lines 8 and 10 of Table 3-6 show placement measures based on estimators 1 and 2 using means and regression adjusted weeks of unemployment. The measures on lines 7 and 9 show why those measures diverge from the difference between those placed versus those who try but fail to obtain interviews. Given the large differences in weeks unemployed based on means and regressions, we would expect that the placement measures will differ greatly as well.

Line 8 shows estimator 1b, which is based on comparing the outcomes for those interviewed to those who tried but failed to obtain interviews and thus comes close to the results of a random assignment design. Using the regression-adjusted weeks measures we found that estimator 1b shows that placements reduce unemployment by 3.80 weeks.

Estimator 1b results are only about 10 percent less than the 4.10 reduction in unemployment shown on line 1, based on comparing those placed to those who tried but failed to obtain interviews. The difference is small because weeks unemployed for the three groups interviewed but not placed is only 0.20 weeks longer than for those who tried but failed to obtain interviews.

Regression adjusting the results for the no-try group reduces the duration of unemployment by 2.40 weeks to -3.02 weeks. This surprising result suggests that no-trys were not lazy, but knew of better opportunities than afforded by the referrals. Finally, those placed were unemployed 6.21 fewer weeks than no-interviews, but regression adjustment reduced the difference by 2.11 weeks to 4.10 weeks. This result suggests that placed individuals are not a random draw of those referred, but have better qualifications and higher motivation to return to work.

In contrast, when mean values are used, estimator 1b produces a much greater estimate of the decrease in unemployment of 9.02 weeks. The estimate is greater in part because the mean of those placed is 6.21 weeks less than the mean for those who failed to obtain interviews. However, it also is greater because the mean of those interviewed but not placed is -1.99 weeks, which suggests that those who would have been placed had they obtained interviews would have been less likely than average to obtain jobs by other means.

Line 10 of Table 3-9 shows that estimator 2, which is the difference between being placed (group-1) and being referred but not placed, (groups 2, 3, 4, 5, and 6), is about 20 percent less than estimator 1 when based on regressions and 65 percent less when based on means. In both cases, the reductions are related to individuals in groups 2, 3, 4, and 6 being unemployed fewer weeks than those in group 5.

Overall, these results reinforce the view derived from looking at individuals with all work records together that estimator 2 tends to underestimate the true effects of placements by about 20 percent. However, in contrast to results for all work record groups together, regression adjusting the results makes a big difference. This, in turn, suggests that it is important to have large samples of individuals in each of the following three groups: (a) placed, (b) interviewed not placed, and (c) tried but failed to obtain an interview.

3.8.2 Referral Outcomes for Individuals with Strong Work Records

The results shown in columns 4, 5, and 6 of Table 3-9 for workers with strong work records differ markedly from those for individuals with spotty records. For starters, the differences between the results based on means versus regressions tend to be small and often are in the opposite direction from the differences for those with spotty work records.

Regression adjustment makes a very large difference only for individuals in group 6, no-try. That regression adjustment creates a 6.15 week increase in duration is evidence that those who do not try to obtain interviews have characteristics associated with easily finding work, and as we might expect, suggests that those who do not try are simply less interested in returning to work.

Regression adjustment increases weeks unemployed by those placed by 1.44 weeks, which as with the spotty record results, suggests that those placed have better qualifications for jobs that are in demand, and are more highly motivated to return to work than the average referred person.

Regression adjustment decreases the weeks of unemployment for members of all three groups who are interviewed but not placed. This result suggests that these job seekers have characteristics associated with having difficulty finding work.

The finding that the regression adjustment tends to be in the opposite direction for those with strong versus spotty work records suggests that the regression-based estimators will be larger than the mean-based estimators. The finding that the regression adjustments tend to be smaller for those with strong work records suggests the difference between the regression-based and mean-based estimators also will be small.

The results on lines 8 and 10 show that these expectations are realized. In particular, the differences in estimator 1b using regressions versus means are much smaller and in the opposite direction for those with strong work records versus weak work records.

However, the most important result is that placements reduce unemployment by 2.67 more weeks, a 60 percent increase, when measured using regressions with estimator 1b versus estimator 2. The primary reason for this difference is that, on average, individuals interviewed but not placed are unemployed 1.96 weeks <u>fewer</u> than individuals who were unable to obtain interviews.

Based on the assumptions underlying estimator 1b, had those unable to obtain interviews had the opportunity to interview, the 60 percent who would not have been placed would have been unemployed 1.96 fewer weeks than the 40 percent who would have been placed. Thus, those who would have been placed would have been unemployed 2.85 weeks more [1.96/(40/60)], in the absence of placement.

In contrast, as shown on line 9 of column 4, there is almost no difference between what happened to those who were referred and not placed versus those who tried but failed to obtain interviews. The differences among members of the five referred and not placed groups almost exactly balance out. Thus, estimator 2 produces almost the same estimate of the placement effect as the comparison between those placed and those who tried but failed to obtain an interview.

Thus, although the above argument may be hard to follow, the difference between estimator 1b and estimator 2 is almost entirely due to the differences between what happened to those who obtained interviews but were not placed versus what happened to those who tried but failed to obtain an interview.

3.9 Summary of Reductions in Weeks-Unemployed Due to PLX Placements

In summary, Table 3-9 presents measures of weeks unemployed for:

Two types of job seekers, those with:

- Spotty work records—0, 1, or 2 quarters of earnings in 1997, and
- Strong work records— 3 or 4 quarters of earnings in 1997.

Using two estimators:

- Estimator 1b—which compares placed individuals to individuals who would have been placed had they been able to obtain interviews;
- Estimator 2—which compares placed individuals to individuals who were referred, but not placed.

With two measures of weeks unemployed:

- A measure based on mean values; and
- A regression adjusted measure.

It is our view that the results using estimator 1b based on regression adjusted weeks unemployed measures produces the most accurate results. Estimator 1b indicates that placements reduce the duration of unemployment by 7.17 and 3.80 weeks for job seekers with strong and spotty work records, respectively.

The large difference in placement effects between those with spotty and strong work records primarily stems from job seekers with strong work records, who obtain interviews that do not lead to placements, being unemployed 1.96 fewer weeks than the average job seeker unable to obtain an interview. This suggests that those who were unable to obtain an interview, but otherwise would have been placed, would have been unemployed considerably longer than average. In contrast, job seekers with spotty work records who obtain interviews that do not lead to placements are only unemployed 0.20 weeks longer than the average job seeker unable to obtain an interview.

Comparing the results using estimator 1b to those using estimator 2 suggests that measures based on the nonexperimental estimator 2, which compares all those referred but not placed, understate the true effect of placements by about 50 percent for job seekers with strong work records and by about 25 percent for job seekers with weak work records.

Regression adjusting weeks unemployed for job seekers with spotty work records makes an enormous difference when using estimator 1b. This is evidence that these job seekers are highly heterogeneous and the observable differences have large effects on the duration of unemployment. Regression adjusting weeks unemployed for job seekers with spotty work records also makes a big difference when using estimator 2, but the difference is only about one-half as great as with estimator 1b.

In contrast, regression adjusting the results for job seekers with strong work records makes only a small difference when estimator 1b is used, and virtually no difference when estimator 2 is used. These results suggest that these job seekers are highly homogeneous. One reason for the homogeneity could be that we selected the comparison group of those referred but not placed from job seekers applying to the same jobs to which the target group was placed.

It seems highly plausible that matching on job opening characteristics would have a far larger effect on job seekers with strong work records, than those with spotty records. It is far more likely that those with strong work records would confine their search to jobs similar to those that they have held in the past and would readily accept suitable offers. Most job seekers with spotty work records probably apply for jobs that require little training or experience. Thus, job seekers with spotty work records who have very different backgrounds and motivations to work would apply to the same jobs.

Our overall conclusion is that estimator 1b produces highly credible results for individuals with strong work records. Our primary reasons for this conclusion are: (1) conceptually estimator 1b should produce highly accurate estimates, (2) the results are similar to those obtained using much larger samples of UI claimants shown in Chapter 4, and (3) weeks unemployed estimated for each of the six separate referral-outcome groups (shown in column 4 of Table 3-9) are much in keeping with expectations. In particular, job seekers rejecting offers or not showing up for jobs are likely to do so because they are able to find jobs more quickly that are at least as good as those to which they were referred. Thus, it is reasonable that the adjustment inherent in estimator 1b suggests job seekers in the quasi-control group who would have been placed had they secured interviews would have been unemployed considerably longer than those who would have found jobs on their own.

We also believe that estimator 1b produces highly credible results for individuals with spotty work records for similar reasons. In particular, it makes sense that individuals with spotty work records who are not given offers would have a lot of trouble finding work. Indeed, the 4.29 weeks longer than the no-offer group is unemployed is a major reason for estimator 1b results being smaller for job seekers with spotty work records than those with strong work records.

However, unlike the results for job seekers with strong work records, we have not previously examined the effects of placements on job seekers with spotty work records. Our expectations were that individuals with spotty work records would need more help finding work than individuals with strong work records. Therefore, the placement effects would be larger for those with spotty work records. The evidence suggests otherwise.

On further reflection we think that job seekers with spotty work records have difficulty finding high-wage jobs because of their lack of work experience and experience looking for jobs, but we suspect that it is easy to locate the kinds of jobs these individuals usually take. In contrast, it is the job seekers who have strong work records who will search until they find a job matching their skills and interests. Therefore, public labor exchanges end up having a greater effect on the duration of joblessness of job seekers with strong, rather than spotty work records.

3.10 Translating Weeks Unemployed to Total Benefits

The primary challenge in accurately assessing the benefits of PLX direct placement services is determining the per-person effect of those services. The reason this element is so difficult is that we need to compare directly observable outcomes to outcomes that are not directly observable. So far, this analysis solely focused on obtaining these estimates and using them to examine the bias in alternative estimators.

In contrast, once we determine the per person effect on weeks unemployed it is simple to translate them into total effects measured in weeks. The number of people affected can be generated both from the person-level files we used for our analysis, as well as from summary statistics maintained by the PLXs.

However, the ultimate goal of the research is to obtain benefit measures that can be compared to cost measures. To translate reductions in weeks unemployed to dollars of benefits, we need to know how those reductions affect earnings. In past work, we estimated earnings effects by assuming the gain was equal to one-thirteenth of average quarterly earnings in the quarter following the return to work.

Line 1 of Table 3-10 presents an estimate of the total increase in earnings generated by each placement reducing the duration of joblessness of claimants and other job seekers with strong work records by 7.2 weeks and job seekers with spotty work records by 3.8 weeks. Line 2 shows that average weekly earnings in the quarter following the return to work (for those whom we observed their earnings) was $260 for job seekers with strong work records and $180 for those with weak work records. Multiplying the number of weeks times the earnings per week produces a measure of the per-person increase in earnings, which as shown on line 3, is $1,872 for job seekers with strong work records and $684 for those with weak work records.

Line 4 of Table 3-10 shows the number of placements made according to official statistics to claimants and nonclaimants in 1998. If we make the conservative assumption that only claimants are in the steady employment group, the total increase in earnings to those individuals is $20,861,568 (11,144 x $1,872). The total increase in earnings to those with spotty work records is $23,965,992 (35,038 x $684). For both groups together the total increase in earnings is $44,827,560, which is about 1.8 times the cost of running the entire PLX system in 1998.

Table 3-10. Earnings Increases and the Benefit-Cost Ratio Stemming From Reductions in Weeks Unemployed

	Steady (1)	Spotty (2)	Both
1. Reduction in weeks unemployed	7.2	3.8	
2. Earnings per week	$260	$180	
3. Increase in earnings (line 1 x line 2)	$1,872	$684	
4. Number Placed in 1998	11,144	35,038	
5. Total Increase in earning (line 3 x line 4)	$20,861,568	$23,965,992	$44,827,560
6. Cost of running the PLXs in 1998			25,000,000
7. Benefit to Cost Ratio (line 5/line 6)			1.8

Because the basic estimates of the reduction in unemployment shown on line 1 are based on a relatively small and nonrepresentative sample we regard the above estimate as indicative of the true effect rather than definitive. However, it is clear that the estimates for the universe could be much smaller than those shown on line 1 and the total effect would still be far greater than the cost.

A major drawback in making the above calculation is having to wait about 9 months from when the survey is conducted to obtain the wage record data needed for this analysis. Because our new person-level file contains information about hourly pay and hours of work for the job openings at which individuals were placed, we could produce an alternative measure and see how it compares to the one based on quarterly earnings.

For job seekers with strong work records, we expect that both methods will produce similar results. For job seekers with spotty work records, the results using the two measures may be substantially different. The difference could stem from job seekers with spotty work records only wanting to work for limited periods. If this was the case, estimates of earnings gains based on quarterly earnings probably would much better reflect increases in earnings.

Although far beyond the scope of this analysis, once we are confident that we can measure reductions in unemployment, it would be worthwhile to directly estimate earnings gains in the short and long run. This is technically feasible for workers strongly attached to the labor force using powerful estimators developed to examine the costs of worker displacement. However, these estimators should be modified to take into account the large subsequent earnings gains, noted in previous research, stemming from returning to work sooner. Also, several years need to elapse before long-term effects can be estimated.

Estimating the earnings gains for job seekers with spotty work records also is important to determine if there is an adverse trade-off between quickly returning to work and subsequent long-term earnings growth. We do not anticipate that use of PLXs would produce an adverse trade-off because our crowding-out analysis suggests that most of the reduction in job search time stems from improving the information possessed by the searchers. Clearly, it would be useful to directly confirm this even if measuring the earnings effects for job seekers with little prior work experience is difficult.

3.11 Summary and Conclusions

The primary achievement of this chapter is demonstrating that use of a mail survey with telephone followup can provide unique information about the reasons referrals do not lead to placements, which permits identification of a group closely resembling a control group that would be generated from a random-assignment design. Thus, the procedure tested here has great potential for providing accurate measures of the effect on the duration of job search of interviewing for jobs listed in PLX job banks following reviewing job listings and obtaining other information.

Also of great importance, it should be possible to use the survey-based estimates to measure the bias in estimators derived from administrative data alone. Thus, it should be highly feasible to develop a low-cost system for accurately monitoring on-going operations, as long as PLXs can identify who is referred to jobs listed with PLXs and who is placed as a result of those referrals.

In addition, this study suggests that measuring the effect of direct placement services on job seekers with strong work records may be substantially improved by adding detailed data about the job orders to which these individuals are referred and by limiting comparisons between those placed to job seekers referred to the same job-listings. Key evidence suggesting this is the case comes from the fact that regression adjusting mean values does not make much difference. Indeed, the similarity between the means and regressions suggest that the results strongly resemble those derived from a random-assignment design.

In contrast, accurately measuring the effect of direct placement services on job seekers with spotty work records is likely to be much more difficult than for those with strong work records. First, the effects appear to be much smaller, which means large samples will be required to ensure results are statistically different from zero. Second, job seekers with spotty work records appear to be highly heterogeneous, and those differences are hard to narrow using administrative data alone.

Evidence suggesting this is the case comes from the fact that regression adjusting mean values makes a large difference. However, at this time we cannot determine how much of the differences are due to natural variation stemming from having small samples versus lacking precise information to identify job seekers who tried to obtain interviews but were unable to do so because the jobs or interview slots were already filled.

Clearly, future results could be greatly improved by obtaining data from a much larger, and far more representative sample of PLX users, and by securing information to be sure that we identify individuals denied interviews because jobs were filled.

Ideally, the sample of job seekers with strong work records should include about 750 individuals placed at jobs to which they were referred and 1,500 individuals referred but not placed individuals. The sample of job seekers with spotty work records should include about 1,250 individuals placed at jobs to which they were referred and 2,500 individuals referred but not placed.

Such a sample should be able to be secured through a mail survey with telephone followup at a cost of about $30 per complete or $180,000 for 6,000 completes. This is a substantial sum, but nowhere near the cost of using a pure random assignment.

There is no doubt that obtaining highly valuable survey information from a large representative sample is feasible and would greatly reduce uncertainty about the true value of PLX direct placement services. At the same time, this study suggests that there would still be some uncertainty about the value of placements as distinct from obtaining information that improves job search outcomes short of interviewing for jobs listed with PLXs. In particular, it is difficult to eliminate the conservative bias stemming from job seekers referred but not placed to one opening being placed subsequently.

However, this uncertainty is trivial compared to the uncertainty of not knowing the value of information that improves job search outcomes short of leading to placement or job interviews. Because for each placed job seeker there are roughly five job seekers who obtain PLX information who are not placed, even small positive effects of obtaining the information would have very large total benefits.

Unfortunately, we do not see how a survey could have an impact on this issue because it is not obvious that there is any naturally occurring group that is denied the opportunity to use PLX services. One possibility worth pursuing with administrative data is determining if there is a systematic relationship between PLX use and time required to get to local offices. If a measure of time to get to an office appeared to affect usage, this measure could be used as an instrumental variable to estimate the effect of PLX usage and not just the value after labor market information has been obtained.

Chapter 4

4. DETAILS OF OUR RESEARCH USING WASHINGTON STATE ADMINISTRATIVE DATA

In this chapter, we describe our analysis of an exceptionally large and detailed administrative database covering the use of public labor exchange (PLX) direct placement services by Unemployment Insurance (UI) claimants in Washington State from 1987 through mid-1995. For most years, the database is a 20 percent sample of the universe and, in total, includes 328,815 separate spells of unemployment. A spell is a period of benefit collection during which there is no gap in payments of more than 3 weeks. On average, each claimant has about 3.5 spells during the 8 1/2 years covered by the database.

The primary goal of this analysis is to measure the reductions in unemployment stemming from claimants' use of PLX direct placement services. We focus on UI claimants because the administrative data provide a great deal of information about each claimant's period of unemployment, work history, demographic characteristics, and the characteristics of his or her labor market. This information may be sufficient to greatly reduce, if not eliminate, selection and other forms of bias in our measures.

In addition, detailed data are available for claimants who obtain PLX referrals as well as a comparison group of UI claimants who do not obtain referrals (and usually obtain no PLX services). This permits obtaining information about the value of PLX referrals (and other informational services) that do not lead to a direct placement—a placement at a job to which a PLX user was referred.

One key advantage of using claimant data is that the data provide a highly accurate view of when claimants became unemployed, when they obtained PLX services, and, in most cases, when they returned to work. Controlling for the duration of unemployment at the point a referral is obtained is the single most important step to reducing selection bias. Indeed, much of the evidence suggesting PLX services are ineffective are seriously flawed because they fail to take into account the fact that many claimants do not even begin to use PLXs until well after apparently similar claimants have already returned to work.

In contrast, it is close to impossible to use administrative data alone to determine when PLX users who are not claimants first began to search for work and when they decided to give up searching for work. The problem is most severe for job seekers who have spotty work records and, therefore, do not qualify for UI benefits. Because these workers spend a lot of time out of the labor force, periods of job search and periods of joblessness do not coincide. Without accurate information about when individuals are searching for work, we would most likely overestimate the value of PLX services by comparing PLX users who are searching for work to nonusers, many of whom are not searching.

A different problem exists for individuals with strong work records who are not UI claimants. These individuals would qualify for UI benefits if they were unemployed, did not quit jobs, were not ill or otherwise unable to work, and actively searched for work. As a result, nonclaimants with strong work records are often still employed during their job search. For this group, the pattern of quarterly earnings tells us nothing about when search started, and since many employed searchers ultimately stay with current employers, we often would not know when job search stopped. Studying nonclaimants with strong work records who are not working faces us with a similar situation to that in studying individuals with spotty work records. We would be comparing PLX users, who are searching for work, with nonusers, many of whom are unable to work or uninterested in finding work.

Another advantage of confining our analysis to claimants is that we can examine reductions in UI expenditures as well as reductions in periods of job search. Reductions in UI trust fund expenditures are one of the most popular benefits of PLX services with legislators and business groups. This enthusiasm stems from savings in UI outlays being directly translated into reductions in a firm's UI tax rate. These reductions occur because UI experience rating systems base a firm's tax rate on the amount of benefits collected by workers the firm lays off. Thus, demonstrating high cost effectiveness with respect to saving UI payroll tax dollars would do a great deal to persuade key groups that labor exchange services are valuable to them.

Finally, this work is aimed at helping states develop a system to monitor ongoing performance. At present, there is exceptionally strong interest in putting in place high-quality performance measures. Measures of trust fund savings and increases in short-term earnings are the two value-added measures that stand the best chance of being put in place because (a) it is highly feasible to create reasonably accurate, low-cost measures of these two factors, and (b) these measures are attractive to a variety of groups.

Our interest in producing procedures for states to routinely employ has led us to use estimating techniques that can be applied using standard statistical packages and are reasonably easy to interpret. In particular, we want to use the fewest number of separate comparisons and mainly rely on ordinary least squares regressions. We have tested far more complicated alternatives, but have found that the more esoteric estimators do not materially change the results. Rather, they primarily serve to make it very difficult for nonexperts to interpret or replicate the results.

4.1 Design of the Analysis

Fundamentally, we want to answer the following two questions:

1. How many weeks sooner do directly placed claimants find jobs than would be the case if they obtained referrals but were not placed?
2. How many weeks sooner do referred but not placed claimants find jobs than would be the case if they did not use PLXs?

We strongly prefer addressing the above questions rather than simply determining how many weeks sooner do all referred claimants find jobs than would be the case if they did not use PLXs? As noted on pages 3-11 and 3-12, the difference in the returns to providing information versus directly placing a person at a job has important implications for how PLXs should be organized and how their effectiveness should be monitored. In particular, measurement problems would be much easier to overcome if the value of providing referrals and other information that helps PLX clients find jobs on their own was close to zero and, therefore, these effects could be omitted from total benefit estimates.

However, if, as appears to be the case, providing information that helps clients find jobs on their own is valuable, omitting those returns would substantially underestimate the total benefits, and could lead decisionmakers to take actions that reduce total benefits. Also, of considerable importance, measures of the effect of referrals not leading to placements can provide information about the potential bias in our estimators.

To answer question 2, we need to compare the duration of job search of claimants who are referred from the point of referral to the point the job seeker returns to work, to the duration of job search of a nonreferral claimant who is identical in every way to the referred person except having been referred. One way to make this comparison is to create a control group by using an experimental design—randomly denying access to PLX services to job seekers requesting aid. This is infeasible because PLXs are required to provide universal access. A second way is to find a naturally occurring group that resembles a control group because it was unable to secure key services. This procedure was used in the preceding chapter with the help of a mail survey, but cannot be done with administrative data alone.

A third way is to determine the effect of all pertinent factors on the duration of job search and then use these estimates to predict the duration of job search for those placed and those referred but not placed had those individuals not obtained PLX aid. The primary drawbacks of applying this technique are that (a) it is difficult to obtain all the variables needed to predict job search duration with absolute accuracy, and (b) without use of a random-assignment design or a natural experiment it is impossible to know if the duration equation includes all the pertinent variables. Nevertheless, the technique used here can substantially narrow the range of plausible answers. More specifically, we should be able to produce a lower-bound estimate that still suggests PLX services are highly cost effective.

Table 4-1 illustrates the sources of potential measurement problems. The table shows that among claimants who change jobs, those who are referred but not placed are considerably less likely to have returned to work by the end of the first full quarter of unemployment than those who are not referred. However, by the third full quarter referred but not placed claimants are considerably more likely to have returned to work than not referred claimants.

Table 4-1. The Probability of Returning to Work for Not Referred, Referred not Placed, and Placed Job Changers

Duration of unemployment	Not referred	Referred, not placed	Placed
1 quarter	49.0	41.7	63.5
2 quarters	67.3	66.8	86.2
3 quarters	78.2	86.2	92.4

This evidence suggests that some claimants are likely to quickly find new jobs without help from any public agency, while others are unlikely to find jobs, even if they receive help. Moreover, claimants who use PLX services are likely to need help more than those who do not use these services. Thus, failure to take into account the ability of claimants to find jobs on their own can lead to underestimates of the effectiveness of PLXs, especially in the first 4 to 6 months of unemployment.

However, after 4 months, when most claimants have found new jobs, many of those who are left may not be interested in returning to work, while others may have unusual difficulty locating a suitable job. Because those uninterested in finding jobs are unlikely to use PLXs, failure to take into account workers' interest in working can lead to overestimation of the long-term returns to use of PLXs.

"Selection bias" is the term applied to failure to adequately take into account factors that affect both the choice to obtain a government service and the outcome the program is supposed to improve. Selection bias most often leads to underestimation of the returns to effective government aid because those most in need of help would have the most negative outcomes without aid and also are those most likely to participate in government-run programs. What makes eliminating selection bias so difficult is that it is very hard to obtain sufficient information to explicitly control for factors influencing participation and outcomes. For example, under ordinary circumstances differences in access to information and motivation are extraordinarily difficult to explicitly measure.

One alternative to finding direct measures is to identify factors that are associated with the key causative factors but do not directly measure them. In this study, we use "the duration of joblessness at the point a referral is made" to reduce selection bias. Clearly, if some claimants have superior information that makes use of PLX services unnecessary, these individuals will have found new jobs at the point others turn to PLXs for aid. Other factors that we can easily measure such as age, education, continuity of employment (tenure), and earnings also can reduce selection bias because they are correlated with the adequacy of job seekers' information required to locate vacancies, as well as their attractiveness to employers once they locate suitable openings.

Another way to overcome selection bias is to limit the study to people who volunteer to participate. Random-assignment experiments gain their power to produce accurate results by denying services to volunteers, and thereby, totally excluding nonvolunteers from the analysis. One of the measures used in this study compares those placed to those referred but not placed. Because both groups have chosen to use PLX direct placement services, the comparison is free of bias due to selection by participants. However, it is still possible that those placed have attributes, such as skills in high demand or more determination to find work, that make them more likely to find work than those referred but not placed.

Therefore, it could be the case that the placed versus referred but not placed comparison overestimates the true effect. However, the information developed from the mail surveys suggests that those placed have poorer alternatives than those referred but not placed and, therefore, estimates based on comparing of those placed to those referred but not placed underestimates the value of the PLX services.

Moreover, because all referred claimants receive potentially useful information about the job market, the placed versus referred but not placed comparison excludes the value of that information.

Thus, it is close to a certainty that the comparisons between those placed and those referred but not placed produce lower bound estimates of the value of PLX direct placement services.[1]

4.2 Specification of the Model

We tested many different ways to estimate the effect of placements and referrals not leading to placements. Ultimately, we decided to employ the following model:

$$L(i,t) = a + b\,\overline{D} + c\,\overline{H} + d\,\overline{M} + r\,P(t) + s\,R(t) \quad i = 1,2;\ t = 1,2,\ldots,5 \tag{4.1}$$

where

L	=	length of unemployment in weeks from referral in period t to: (a) end of job search if i = 1, or (b) benefit exhaustion or 26th payment whichever comes first if i = 2;
$\overline{D}$	=	an array of demographic characteristics;
$\overline{H}$	=	an array of work and claim history characteristics;
$\overline{M}$	=	an array of labor market characteristics;
P	=	a dummy variable indicating a person is placed as a result of a referral in period t;
R	=	a dummy variable indicating a person is referred in period t, but not placed as a result of a referral through week 39; and (The omitted group are claimants who did not obtain placements, and rarely obtained any assistance from PLXs.)
t	=	period relative to start of claim spell over which measures of service receipt and subsequent duration are made: 1=week 1; 2=week 2-9; 3=week 10-13; 4=week 14-18; 5=week 19-26. (Lower case letters other than i and t denote parameter estimates.)

We use equation 4.1 to produce estimates of the effect of referrals made in five periods spanning the first 26 weeks of UI covered spells of unemployment. A key feature of this model is that we only include claimants unemployed and collecting UI benefits at the start of a period in that period's comparisons. Claimants who previously terminate spells of covered unemployment are dropped from the

[1] The bias measures in Chapter 3 compared results resembling those derived from a random-assignment design to results derived from a nonexperimental design similar, but not identical, to the one used here. There are three, potentially important, differences between the nonexperimental design used in Chapter 3 and the one used here: (1) the design used in Chapter 3 produced one estimate with job search duration at the point a referral was obtained as an independent variable, the one used here produces five separate estimates based on how long claimants were unemployed at the point referrals were made; (2) the mail survey discussed in Chapter 3 covered 1998, this study covers 1987-95; (3) the mail survey sample was limited to people referred to jobs to which other sample members were placed; that condition was not applied here. Because our nonexperimental results are quite similar to the nonexperimental results shown in Chapter 3, it is likely that the bias is in the same direction and has the same order of magnitude. However, the only way to be sure that the bias is measured accurately is to: (a) use the techniques described in Chapter 3 to obtain a much larger sample, and identify individuals who tried to obtain referrals but were unable to do so because the jobs or interview slots were filled; (b) use the techniques described in this chapter to produce nonexperimental results; and (c) compare the results similar to those based on a random-assignment design to those based on the nonexperimental design used here.

sample. As a result, all 328,815 spells of unemployment in our database are included in period 1, but only 98,111 of those spells lasted long enough to be included in period 5, which starts with the 19th week of unemployment. Appendix C provides the list of variables.

We tested a number of different specifications and determined (1) that separate equations produced superior results to simply including duration as an independent variable; and (2) use of the five periods selected tend to maximize differences across the grouping, while additional divisions would make very small differences. For example, we found that the estimates for the 2-9 week grouping were about the same as use of separate 2-4 and 5-9 week groupings.

Receipt of referrals and being placed is calculated independently for each period. Thus, it is possible for someone to be placed in more than one period, but this rarely happens. It is quite common, however, for someone to be referred but not placed in several periods. Again, after considerable experimentation, we determined that it made sense to omit from the referred but not placed category in one period, individuals who are placed within 39 weeks of the start of a UI spell. If we did not take this step, the referred not placed dummy variable would be picking up the effect of subsequent placements.[2]

One key reason for not examining referrals after the 26th week of an unemployment spell is that once benefit collection ends it becomes difficult to determine if claimants remain unemployed. Duration of unemployment, however, is a crucial variable to accurately measuring the effect of PLX services. Thus, we wanted to avoid introducing an additional source of uncertainty that could affect the accuracy of our results. The measurement problems are especially severe because many claimants take the best available jobs with or without PLX aid close to the point UI benefits are exhausted.[3]

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[2] An alternative way to estimate the effect of referrals is to use a hazard model, which summarizes differences in the probability of PLX-users and nonusers of ending an unemployment spell after 1,2,...,n weeks of unemployment. Thus, hazard models look ahead only one period. In contrast, our estimator uses a relatively long post-service time horizon, which eventually is truncated because some people do not end spells during the period observed. Hazard models deal with this truncation very well. However, figuring out what effect PLX services have on ending a spell is very difficult because the receipt of service in one period may affect the return to work beyond the next period. Also, it is difficult to introduce parameters into hazard models that take on different values as time passes. Perhaps the biggest drawback in the use of hazard models is that estimating these models is very complex as is translating the results from probabilities into durations of unemployment. Drs. Jacobson and Katz used hazard models and simpler estimators similar to equation 4.1 in earlier work. [reference] That analysis suggested that the differences between the two estimators were small, especially when truncation was not a major factor, and it was difficult to determine which estimator produced more accurate results. Thus, we doubt that hazard models would out-perform estimator 4.1 for looking at total unemployment duration which turns out to be of central importance to this study.

[3] Recently, Washington State added measures of hours worked each quarter to the administrative wage record file. This information should make it far easier to determine the precise duration of unemployment after claimants exhaust benefits.

A secondary reason for not extending the analysis beyond the point claimants end their claim spell is that we wanted to examine the benefits of reduced UI payments and earnings gains over a comparable period. Indeed, policymakers have shown much more interest in accurately measuring the reduction in UI payments than in measuring the increase in earnings. As we shall see, focusing solely on UI payment reductions is like looking only at "the tip of an iceberg," as most benefits are due to reductions in joblessness.

Also of considerable importance, the 26-week cutoff limits the effect of extended benefits, which were only in place from 1991 through 1994, on our measures of UI payment reductions. This step was taken to obtain comparable estimates over good economic times and bad. In addition, because extended benefit programs are fully financed from Federal revenue, not experience-rated state contributions, the measures we present reflect the savings to the state trust fund, which determines firms' experience-rated UI tax rate.

In order to look at reductions in benefit payments and increases in earnings, we use two different dependent variables with equation 4.1. The UI benefit measure includes the period from receipt of a referral to the point the claim spell ends: (a) with 4 or more weeks of no payments, (b) with benefit exhaustion, or (c) 26 weeks have elapsed, whichever comes first.

We estimate that by the 26th week about 70 percent of the spells have ended. Roughly 60 percent of the spells have ended with the claimant returning to work, about 5 percent end with benefit exhaustion, and 5 percent end with the claimants stopping benefit collection but not returning to work. Some claimants stop requesting payments because they do not meet the qualifying requirements of being able, available, and not refusing suitable work. Other claimants, usually those unemployed for long periods, do not request payments to leave some funds untouched much like a savings account. Of the 30 percent of spells that continue beyond 26 weeks, we estimate that they are roughly equally divided among returns to work before exhaustion, exhaustion, and stopping collection without returning to work or exhausting benefits.

The total unemployment duration measure includes the period of joblessness following receipt of a referral until the person finds work or seven quarters have elapsed. The estimate is derived from the pattern of quarterly earnings in our administrative database. If claimants earned four times the amount of their maximum weekly UI payment in the quarter following the start of a UI spell, they are assumed to have been unemployed 3 weeks. If earnings first reached four times the maximum payment in the second quarter, the person is assumed to have been unemployed 16 weeks (3+13), etc.

These parameters were set so that the duration of unemployment and the duration of UI payments were very similar for those who return to work within 26 weeks. However, we do not track weeks of unemployment beyond 7 quarters. Only about 10 percent of the claimants do not return to work covered by UI wage records. In most of these cases, workers have moved to another state or are working in an uncovered sector.

4.3 Results for All Years Together

In this section, we describe our results derived from using equation 4.1 for each of the five time periods with weeks of total unemployment and weeks of UI payments as dependent variables. Column 1 of the Table 4-2 shows the difference in the dependent variables between claimants referred but not placed versus those not referred. Column 2 shows differences between placed claimants versus those not referred. The tables show both the differences between the regression adjusted measures and measures based on the simple means.

4.3.1 Referral and Placement Effects on Unemployment Duration

Line 2, column 2, of Section A in Table 4-2 displays our estimate that, averaged over all five periods, a referral not leading to placements <u>reduces</u> unemployment by 2.149 weeks. Column 1 also shows that, on average, mean weeks of unemployment is .208 weeks <u>greater</u> for referred but not placed claimants than those not referred. Regression-adjusting unemployment measures using equation 4.1, therefore, leads to a substantial, 2.357 week (.208 - -2.149), decrease in unemployment over the simple mean difference between those referred but not placed and those not referred. This difference suggests that, much as we expect, claimants who are referred have characteristics associated with having more difficulty finding jobs than claimants who are not referred (most of whom receive no PLX services).

Table 4-2. The Effect of Referrals and Placements on Weeks of Unemployment Taking the Timing of Referrals into Account

A. Average Effects of Referrals and Placements Made over All Five Periods

	Weeks of Unemployment		
	Referred	Placed	Placed-Referral
1. Tabulation	0.208	-12.254	-12.462
2. Regression	-2.149	-9.867	-7.718
3. Difference	2.357	-2.387	-4.744

B. Effect of Referrals and Placements Made in Week 1 on Claimants Unemployed at Least 1 Week

	Weeks of Unemployment				Percent	
	Referred	Placed	Placed-Referral	Number of Claimants	Referred	Placed
4. Tabulation	0.854	-7.106	-7.960		2.28%	0.26%
5. Regression[a]	-2.029	-5.569	-3.540	328,815		
6. Difference	2.883	-1.537	-4.420			

C. Effect of Referrals and Placements Made in Weeks 2-9 on Claimants Unemployed at Least 2 Weeks

	Weeks of Unemployment				Percent	
	Referred	Placed	Placed-Referral	Number of Claimants	Referred	Placed
7. Tabulation	0.934	-9.789	-10.723		4.61%	1.60%
8. Regression[b]	-2.340	-8.427	-6.087	292,677		
9. Difference	3.274	-1.362	-4.636			

D. Effect of Referrals and Placements Made in Weeks 10-13 on Claimants Unemployed at Least 10 Weeks

	Weeks of Unemployment				Percent	
	Referred	Placed	Placed-Referral	Number of Claimants	Referred	Placed
10. Tabulation	-1.039	-15.428	-14.389		2.76%	0.77%
11. Regression[c]	-2.460	-12.388	-9.928	169,028		
12. Difference	1.421	-3.040	-4.461			

E. Effect of Referrals and Placements Made in Weeks 14-18 on Claimants Unemployed at Least 14 Weeks

	Weeks of Unemployment				Percent	
	Referred	Placed	Placed-Referral	Number of Claimants	Referred	Placed
13. Tabulation	-1.652	-15.647	-13.995		3.39%	0.92%
14. Regression[d]	-2.175	-11.867	-9.692	133,465		
15. Difference	0.523	-3.780	-4.303			

F. Effect of referrals and placements made in week 19-26 on claimants unemployed at least 19 weeks

	Weeks of Unemployment				Percent	
	Referred	Placed	Placed-Referral	Number of Claimants	Referred	Placed
16. Tabulation	-1.662	-16.631	-14.969		4.96%	1.48%
17 Regression[e]	-1.399	-12.187	-10.788	98,111		
18. Difference	-0.263	-4.444	-4.181			

NOTE: Referral and placement effect are measured relative to not being referred. In all cases, standard errors are tiny compared to the regression coefficient. All results are statistically significant at the 1% level.

		Standard Errors	
		Referred	Placed
a.	1	-0.319	-0.743
b.	2-9	-0.199	-0.330
c.	10-13	-0.358	-0.671
d.	14-18	-0.371	-0.701
e.	19-26	-0.371	-0.659

Across the five time periods, the effect of referrals not leading to placements shows modest variation. However, the most interesting pattern is that the difference between the means and regression-adjusted results narrow from the second through fourth period and finally reverses. This pattern suggests that, as the claimants who most easily find jobs return to work, the remainder of those referred but not placed and those not referred become more and more similar to each other.

Column 2 of Table 4-2 shows the coefficients and means that measure the difference of weeks of unemployment between placed claimants and those not referred. In Section A, we show our regression estimate that, averaged over all five time periods, placed claimants are unemployed 9.723 fewer weeks than similar nonreferred claimants. Using the mean differences to estimate duration reductions, we show that placed claimants are unemployed 12.254 fewer weeks than nonreferred claimants. Thus, use of equation 4.1 reduces the placement effect by 2.387 weeks (12.254 - 9.867). This difference implies that claimants who are placed have characteristics associated with having less difficulty finding jobs.

The placement effect increases sharply across the first three periods and remains about constant thereafter. The difference between means and regression-adjusted results increases over time. In contrast to the referral results, this pattern suggests that claimants who are placed early during their unemployment spell most closely resemble nonreferred claimants. However, as the duration of unemployment lengthens, those placed progressively come from the remaining claimants most likely to return to work on their own.

Finally, column 3 displays estimates of the effect of placements using those referred but not placed as the comparison group, instead of those not referred. These estimates are derived by subtracting the figures in column 1 from those in column 2. As noted earlier, this estimate inherently assumes that:

- The value of the information obtained about the job market by looking at job listings is of no value. What is valuable is the increased opportunity to interview for a job;
- Any positive effect of referrals not leading to placements relative to not being referred is due to selection bias; and
- Selection bias for those placed and those referred but not placed is identical.

The pattern across periods of the regression adjusted figures in column 3 are similar to those in column 2. The effects are about 2 weeks less in each case, but still substantial.

4.3.2 Referral and Placement Effects on Weeks of UI Payments

Table 4-3 uses the same format as Table 4-2 to show the effects of referrals and placements on weeks of UI payments, instead of weeks of unemployment. The pattern of the results is quite similar in the two tables. However, the effect on UI payments is about 40 percent of the effect on weeks of unemployment. The much smaller effect on weeks of UI collection stems from the UI payments included in these estimates lasting 26 weeks at most, while joblessness can persist long after benefits are exhausted.

On average, over all periods, referrals not leading to placements reduce the duration of UI payments by only .188 weeks. Placements reduce payments by 4.244 weeks. As was the case for weeks of unemployment, the difference between regression-adjusted and simple mean measures is positive for referrals not leading to placements, indicating that these claimants have characteristics associated with a lower probability of finding work on their own. The difference was negative for those placed, indicating that those claimants have characteristics associated with a higher probability of finding work on their own.

Across periods, the placement effect shows less variation on weeks of UI payments than weeks of unemployment. This is to be expected because of the relatively short maximum duration of payments. However, the effect of referrals and placements does not decline very much as the maximum duration of covered benefits approaches. This result suggests that as claimants who are most likely to return to work end their spells, those left are more and more likely to exhaust without PLX help. After about 13 weeks of joblessness, it is likely that the spell of unemployment will extend to 26 weeks, and most claimants will have had enough time to locate high-paying jobs. The jobs that are available, therefore, usually do not pay enough to make taking them more attractive than continuing to collect UI.

Equation 4.1 produces measures that suggest referrals not leading to placements slightly increases the duration of payments in some periods. Although it is theoretically possible that improved information would increase unemployment duration, we think this is an unlikely outcome. More likely, these results are due to equation 4.1 not fully capturing all the factors associated with some claimants using PLXs. However, that the increases in duration are tiny suggests that the measurement bias is small.

Table 4-3. The Effect of Referrals and Placements on UI Compensated Weeks of Unemployment Taking the Timing Referrals into Account

A. Average Effects of Referrals and Placements Made over All Five Periods

	Number of UI Payments		
	Referred	Placed	Placed-Referral
1. Tabulation	1.032	-5.145	-6.177
2. Regression	-0.188	-4.244	-4.056
3. Difference	1.220	-0.901	-2.121

B. Effect of Referrals and Placements Made in Week 1 on Claimants Unemployed at Least 1 Week

	Number of UI Payments				Percent	
	Referred	Placed	Placed-Referral	Number of	Referred	Placed
4. Tabulation	0.342	-4.043	-4.385	Claimants	2.28%	0.26%
5. Regression[a]	1.336	-3.039	-4.375	328,815		
6. Difference	-0.994	-1.004	-0.010			

C. Effect of Referrals and Placements Made in Weeks 2-9 on Claimants Unemployed at Least 2 Weeks

	Number of UI Payments				Percent	
	Referred	Placed	Placed-Referral	Number of	Referred	Placed
7. Tabulation	1.737	-5.294	-7.031	Claimants	4.61%	1.60%
8. Regression[b]	0.196	-4.512	-4.708	292,677		
9. Difference	1.541	-0.782	-2.323			

D. Effect of Referrals and Placements Made in Weeks 10-13 on Claimants Unemployed at Least 10 Weeks

	Number of UI Payments				Percent	
	Referred	Placed	Placed-Referral	Number of	Referred	Placed
10. Tabulation	0.298	-5.823	-6.121	Claimants	2.76%	0.77%
11. Regression[c]	-0.616	-4.811	-4.195	169,028		
12. Difference	0.318	-1.012	-1.330			

E. Effect of Referrals and Placements Made in Weeks 14-18 on Claimants Unemployed at Least 14 Weeks

	Number of UI Payments				Percent	
	Referred	Placed	Placed-Referral	Number of	Referred	Placed
13. Tabulation	0.506	-5.390	-5.896	Claimants	3.39%	0.92%
14. Regression[d]	-0.297	-4.249	-3.952	133,465		
15. Difference	0.209	-1.141	-1.350			

F. Effect of referrals and placements made in week 19-26 on claimants unemployed at least 19 weeks

	Number of UI Payments				Percent	
	Referred	Placed	Placed-Referral	Number of	Referred	Placed
1. Tabulation	0.941	-4.508	-5.449	Claimants	4.96%	1.48%
2. Regression[e]	0.373	-3.585	-3.958	98,111		
3. Difference	0.568	-0.923	-1.491			

NOTE: All results are statistically significant at the 1% level; * = significant at 5% level, + = not significant at 10% level, and † = significant at 10% level.

	Standard error	
	Referred	Placed
a. 1	(.179)	(.416)
b. 2-9	(.109)*	(.181)
c. 10-13	(.174)	(.327)
d. 14-18	(.174)+	(.328)
e. 19-26	(.170)†	(.302)

Indeed, comparing the tiny effect of referrals not leading to placements on weeks of UI payments to the far larger effect on weeks of unemployment suggests the following: obtaining information from PLXs that does not lead to direct placements primarily helps claimants to find jobs quickly at the point benefits are exhausted or are close to exhausted. This pattern is highly consistent with conceptual and empirical analyses of how UI benefits affect job search. These studies suggest UI benefits substantially raise the minimum acceptable wage of jobs, until benefits are close to exhaustion.

Finally, because the figures in column 1 are so close to zero, subtracting these estimates from those in column 2 makes little difference on the per-person effect shown in column 3. The primary impact of assuming a zero effect of information not leading to placements stems from greatly reducing the number of claimants helped by PLX direct placement services.

4.3.3 Total Benefits of Referrals and Placements

Obtaining estimates of the per-person effects of PLX direct placement assistance is a key component of a larger goal—determining the total benefits of these services to claimants. Thus, the next step in the analysis is multiplying the per-person estimates shown in Tables 4-2 and 4-3 times the number of individuals affected to estimate the total benefits.

Line 1 of column 1 of Table 4-4 shows that there were 47,555 incidents of claimants being placed at jobs (according to our database) from 1987 through mid-1995.[4] Line 2 shows that, on average, each placement reduced the duration of joblessness by 9.867 weeks. Line 3 shows that, in total, placed claimants were jobless by 469,225 fewer weeks (9.867 x 47,555).

[4] Because the same claimant can be placed during several different spells of unemployment and even more than once during different periods in a single spell, the unit of observation technically is the person-period. We use the term incident to indicate that it is possible for the same person to be placed multiple times.

Table 4-4. Four Measures of Total Benefits Stemming from Claimants' Decreased Joblessness

		Placed versus not referred (1)	Referred not placed versus not referred (2)	Placed versus referred not placed (3)	Referred versus not referred (4)
1	Incidents 1987 - mid 1995	47,555	160,945	47,555	208,500 (col 1 + col 2)
2	Reduction in joblessness each incident (weeks)	9.867	2.149	7.718 (col 1 - col 2)	1.000 (assumption)
3	Total reduction in weeks of joblessness (line 1 x line 2)	469,225	345,871	367,029	208,500
4	Average weekly earnings quarter after reemployment	$260	$260	$260	$260
5	Total earnings gain (line 3 x line 4)	$121,998,548	$89,926,409	$95,427,667	$54,210,000
6	Total gain placed + referred	$211,924,957 (Line 5, col 1 + line 5, col 2)	-	$95,427,667 (Line 5, col 1 + 0)	$149,637,667 (Line 5, col 3 + line 5, col 4)
7	Gain per year (line 6 / 8.5)	$24,932,348	-	$11,226,784	$17,604,431

Column 4 shows that the average weekly earnings of those placed in the first full quarter following their return to work was $260. Using this amount as the average weekly gain in earnings, we show in line 5 that the total increase in earnings is $121,998,548. We feel that this is a conservative measure of the total gain in earnings because returning to work sooner boosts both current and future earnings, but we ignore gains that accrue while the claimant is employed.[5]

Column 2 presents the same calculation as in column 1 for referrals not leading to placements. The total gain to those referred but not placed is $89,926,409, which is only 74 percent of the gain accruing to those placed. The gain is smaller, although there are 3.4 times more referrals not leading to placements than placements, because the per-incident reduction in weeks of joblessness is 4.6 times greater for placements than for referrals not leading to placements.

[5] In the future, we recommend adapting the technique developed by Drs. LaLonde, Sullivan, and Jacobson to directly estimate earnings gains using administrative data, rather than using this indirect method. However, for the new results to be accurate they need to explicitly take the decrease in joblessness into account. Because there is a strong negative association between reemployment earnings and unemployment duration, reducing the duration of unemployment is likely to substantially increase future earnings over what they otherwise would be. This is particularly true because many of the placements occur after claimants have been jobless for extended periods.

As shown on line 6 of column 1, adding the total gains from placements and referrals not leading to placements leads to a net gain of $211,924,957. This equals about $25 million a year, which is roughly the cost of running the entire Washington PLX.

Column 3 of Table 4-4 presents lower-bound estimates based on assuming that (a) the value of referrals not leading to placements is zero; (b) the positive value shown on line 2, column 2, measures selection bias; and (c) the selection bias for those placed is equal to the bias for those referred but not placed. Thus, the incidence of placement is the same in column 3 and column 1, the per-incident reduction in joblessness for those placed is 7.718 (9.867 - 2.149), and the reduction in joblessness for those placed is 0 (2.149 - 2.149).

The total lower-bound gain in earnings is $95,427,667, which is 45 percent of the gain shown in column 1, and roughly 45 percent of the cost of running the entire Washington PLX. However, we estimate that claimants receive less than 30 percent of all PLX services. Thus, these services have a benefit-cost ratio of about 1.5, which is quite respectable.

However, we believe that the per-incident estimate used here to produce the lower-bound estimate is highly conservative. This view stems from the mail survey results shown in Chapter 3 being about the same as those shown on line 2 of column 3, but (a) economic conditions during the period included in this study were considerably worse than in 1998 the period for which the mail survey results apply, and (b) evidence shown in the next section indicates that the per-person benefits are about 40 percent greater in periods of high unemployment than when unemployment is low.

Moreover, we believe that the per-incident value of referrals not leading to placements is greater than zero. Even if the reduction in joblessness is only 1 week, column 4 of Table 4-4 shows that the benefit from information not leading to placement would equal $54,210,000. When added to the benefit from being placed the total benefits equal $149,637,667, which is about 70 percent of the total cost of running the Washington PLXs.

Table 4-5 displays our results for reductions in benefit payments using the same format as Table 4-4. The incidence of service receipt shown on line 1 is also identical to that shown on line 1 of Table 4-4. However, the per-incident reduction in payments shown on line 2 is considerably less than the reduction in unemployment. In particular, the gain from referrals not leading to placements is only .188 weeks. As a result, the total reduction in weeks of benefit payments shown on line 3, column 1 is only 43 percent of the reduction in weeks of joblessness.

Table 4-5. Four Measures of Total Reduction of UI Payments

		Placed versus not referred (1)	Referred not placed versus not referred (2)	Placed versus referred not placed (3)	Referred versus not referred (4)
1	Incidents 1987 - mid 1995	47,555	160,945	47,555	208,500
2	Reduction in weeks of benefit payments each incident	4.244	0.188	4.056	0.090
3	Total reduction in weeks of payments (line 1 x line 2)	201,823	30,258	192,883	18,765
	Percent of weeks of unemployment	43.0%	8.7%	52.6%	9.0%
4	Average weekly payment	$116	$134	$116	$130
	Percent of earnings gain	44.6%	51.5%	44.6%	50.0%
5	Total reduction in payments (line 3 x line 4)	$23,411,517	$4,054,526	$22,374,437	$2,437,471
	Percent of earnings gain	19.2%	4.5%	23.4%	4.5%
6	Total reduction placed + referred	$27,466,043 (Line 5, col 1 + line 5, col 2)		$22,374,437 (Line 5, col 3 + 0)	$24,811,908 (Line 5, col 3 + line 5, col 4)
	Percent of earnings gain	13.0%		23.4%	16.6%
7	Reduction per year	$3,231,299		$2,632,287	$2,919,048

In addition, the average weekly payment shown on line 4 also is only about 45 percent of average earnings because benefits are set at about one-half of each claimant's weekly earnings and capped at about one-half the average weekly wage for all workers. Placed claimants have somewhat lower benefit payments than those who are referred but not placed because they have somewhat lower benefit entitlements. It also is worth noting that the measure used is the average payment per week of the UI covered spell. This amount is considerably smaller than the maximum weekly benefit payment because most spells include at least 1 week with no payments and often include a few weeks where payments are reduced due to having some earnings.

The total reduction in benefit payments is $23,411,517, which is only 19 percent of the earnings gain. Thus, about one-fifth of the placed claimants' gain in earnings is offset by a loss of UI benefits over the first 26 weeks of UI spells. If we add the reduction in benefit payments accruing to referred but not placed claimants shown in column 2 of Table 4-5, the total reduction increases by only 17.3 percent to $27,466,043. Because the per-incident referral effects are so small, the lower-bound estimate shown on line 5 is only 20 percent less than the estimate shown in column 3. Similarly, assuming that referrals not leading to placements have one-half of the measured effect (and one-half is due to selection bias) does not change the estimate of total benefits very much.

4.3.4 Sensitivity Tests

Because the above results appear to produce a plausible range of estimates, we took several steps to determine how sensitive the results are to changes in the way the data are organized and equation 4.1 is specified. Probably our most important test is examining the effect of excluding from our estimates the 11 percent of the spells that do not end with reemployment. This exclusion reduced the average affect of referrals not leading to placements and placements on total unemployment by about 10 percent.

The effect was large enough to raise concerns that many of those not referred who do not return to work may be employed in sectors uncovered by Washington wage record data. Uncovered sectors include all work in other states, self-employment, government employment, and small farm employment. However, the duration of benefit collection of not referred claimants who did not return to covered employment also was exceptionally long. Since any type of employment should terminate benefit collection, we concluded that most of these individuals were not employed. Thus, on balance, it appeared that including those not reemployed was reasonable because PLXs were particularly effective in helping claimants who otherwise would be unlikely to find work on their own.

We also estimated the effect of referrals and placements for claimants who changed jobs and separately for those who were recalled. Adding the results together produced results very similar to those when both groups were included in a single equation. Also, as expected, the effect of referrals and placements was much greater for employer-changers than recalled claimants. Even more importantly, the results supported the view that our estimates were not strongly affected by selection bias. In particular, the effect of referrals on recalled claimants was about zero, which seems reasonable for a group that mostly returns to work within 10 weeks.

We also examined how robust the results are with respect to changes in the specification of equation 4.1. Our basic finding was that the results change very little, not just when one or two variables were omitted but also when entire groups were omitted.

Basically, controlling for three characteristics—reemployment status (recall, job change, not reemployed), how long the claimant has been unemployed when a referral is made, and the year the unemployment spell begins—produces referral and placement results that are quite similar to when the entire array of variables are included. Adding industry, earnings, sex, and tenure improves the explanatory power of the unemployment duration estimates, but has little affect on the referral and placement estimates. Most likely, these added characteristics do not vary much across those placed, referred but not placed, and not referred, once the first three characteristics are taken into account.

That it is only necessary to control for a few characteristics makes it easy to use equation 4.1 for monitoring ongoing operations and replicate the Washington results in other states. However, the robustness tests also showed that the labor market variables we used were insufficient to eliminate the need to use dummy variables for each individual year in order to capture year-to-year differences in economic conditions and other factors. Thus, we doubt that our equations could be used to accurately project the returns to referrals and placements into periods where economic conditions are different from those observed.

This means that accurate estimates of current year PLX performance can be based on past years' performance, if labor market conditions and other factors are similar. But if there are major changes in labor market conditions, reasonably accurate results would have to wait until sufficient time elapses to follow up claimants who are placed and referred. Six quarters of followup certainly would be sufficient, but additional research is needed to determine if shorter periods would produce accurate results.

4.3.5 Other Evidence Bearing on the Effectiveness of PLX Services

When we started to accurately estimate the benefits of PLX referrals and placements, we knew that there were formidable technical problems that make this a very difficult task. Our major concerns were:

1. Dealing with negative selection bias at the start of claims periods where claimants with the poorest prospects of finding work are most likely to seek help from a PLX.

2. Dealing with positive selection bias that occurs after 8 or more weeks of unemployment when claimants using PLXs may be searching harder for work by a variety of means and are much more likely to accept job offers than apparently identical nonusers.

3. Accurately measuring the duration of joblessness after benefit collection ends.

Our primary concern was measuring the effect of referrals not leading to placements. If the true value of PLX referrals is zero, but claimants searching for work use PLXs as one of many different means to find work, the quicker return to work of those referred solely would be a result of search effort being correlated with the intensity of PLX use. Thus, it is possible that all of the referral effect is due to measurement bias.

However, it is much harder to explain how large placement effects could be due to measurement error, especially when referral effects are small. Indeed, it is likely that, if anything, measurement bias is greater for referrals not leading to placements than for those that lead to placements. Thus, we regard the evidence that referral effects are small as very important evidence that there is little bias in the placement measurements.

Although it seems self-evident that placements should have relatively large effects, there is additional evidence that it is PLX aid that causes this effect—that large proportions of claimants eventually placed previously spent a lot of time obtaining referrals. If placed claimants are simply searching harder using a variety of means, and PLX services are of little, if any, value, we would expect that most of these claimants would find jobs by those other means.

As shown in Table 4-6, almost two-thirds of the claimants placed after the tenth week of unemployment were previously referred to jobs by PLXs, and about 50 percent were referred in the previous 4 weeks. Similarly, the fact that about 45 percent of those referred but not placed in one period previously obtained referrals from PLXs suggests that this activity also is of considerable value. Otherwise, we would expect that claimants might accept referrals once, but not continue to obtain referrals.

Table 4-6. PLX Use Before Being Placed, Referred, and Being Unemployed During Weeks 1, 2 to 9, 10 to 13, 14 to 18, and 19 to 26 of UI Claim Spells

	Placed		Referred		All Groups	
	Number	Distribution	Number	Distribution	Number	Distribution
Week 19 to 26						
Referred week 14-18	656	45.3%	1,525	25.1%	5,348	5.5%
Referred before 14-18	266	18.4%	1,273	20.9%	7,923	8.1%
No prior referrals	526	36.3%	3,282	54.0%	84,840	86.5%
Total	1,448	100.0%	6,080	100.0%	98,111	100.0%
Week 14 to 18						
Referred week 10-13	601	49.0%	1,477	24.4%	5,994	4.5%
Referred before 10-13	242	19.7%	1,244	20.6%	8,600	6.4%
No prior referrals	383	31.2%	3,320	55.0%	118,871	89.1%
Total	1,226	100.0%	6,041	100.0%	133,465	100.0%
Week 10 to 13						
Referred week 2-9	842	65.1%	2,543	39.7%	12,975	7.7%
Referred before 2-9	27	2.1%	210	3.3%	1,927	1.1%
No prior referrals	425	32.8%	3,658	57.1%	154,126	91.2%
Total	1,294	100.0%	6,411	100.0%	169,028	100.0%
Week 2 to 9						
Referred week 1	1,342	28.7%	2,209	13.0%	7,184	2.5%
Referred before 1	–	0.0%	–	0.0%	–	0.0%
No prior referrals	3,339	71.3%	14,804	87.0%	285,493	97.5%
Total	4,681	100.0%	17,013	100.0%	292,677	100.0%
Week 1	862		7,015			320,938

NOTE: See Appendix sections C.2 and C.3 for regression estimates

Further, the tendency for claimants to persist with use of the PLX until they find work suggests that there is a group of claimants who would be unlikely to find work if they were not placed by the PLX. Indeed, part of the large reduction in total unemployment easily could stem from nonusers being so discouraged about finding work that they stop looking for work for long periods. In contrast, PLX referrals encourage users to continue to search for work.

However, because substantial numbers of claimants find jobs on their own in the same period they receive their first referral, we cannot rule out the possibility that some of the positive effect of referrals is due to the type of bias we are attempting to eliminate entirely. Also, some claimants may continue to use PLXs purely to satisfy work-search requirements. Thus, our bottom line is that assuming all of the positive effect of referrals is due to measurement bias produces conservative estimates of the true effect.

4.4 Results for Each Year

Given that the estimates based on equation 4.1 provide useful information about the benefits for all years together, it makes sense to extend the analysis to examine how benefits and costs vary by year. This is particularly important because (a) benefits are likely to vary substantially with differences in economic conditions, and (b) today's strong economic conditions differ radically from those of the 1991-92 recessions and their aftermath that heavily affect the results presented above.

Precisely the same steps used for examining all years together are used to examine each year individually. First, we estimate the per-person effect of placements and referrals not leading to placements. Second, we use estimates of the number of incidents of placements and referrals not leading to placements to estimate the total reduction in the number of weeks of unemployment and UI payments. Third, we multiply the total number of incidents times average earnings and average payments to estimate total benefits.

4.4.1 Per-Person Effects by Year

Table 4-7 (a and b) displays the same measures of the effects of referrals and placements on UI payments and weeks of unemployment presented in Tables 4-2 and 4-3, but for each year, rather than all years together. The year-by-year estimates were derived from introducing separate variables for referrals and placements in each year into the same specification used for producing all years together results. Separate equations were run for the same five periods relative to the start of the UI claim. The far more cumbersome alternative of also using separate equations for each year was tried but found to produce almost identical results.

Table 4-7a. Effects of Placements and Referrals on UI Payments for Each Year 1987 to 1995

Year	Washington unemployment rate	Effect on weeks of covered unemployment of Referral	Effect on weeks of covered unemployment of Placement	Deviation of placement effects from average	Effect of placement assuming referral effect measures bias	Deviation of adjusted placements effects from average
1987	7.6%	0.917	0.787	-118.5%	-0.783	-95.5%
1988	6.2%	0.561	-1.274	-70.0%	-1.835	-54.7%
1989	6.2%	0.588	-1.507	-64.5%	-2.095	-48.3%
1990	4.9%	-0.254	-3.529	16.9%	-3.275	-19.2%
1991	6.4%	-0.621	-5.404	27.3%	-4.783	18.0%
1992	7.6%	-0.588	-6.933	63.3%	-6.345	56.6%
1993	7.6%	0.229	-5.216	22.8%	-5.445	34.4%
1994	6.4%	-0.046	-3.173	-25.3%	-3.127	-22.8%
1995	6.4%	-0.197	-2.893	31.9%	-2.696	-33.5%
1996	6.5%					
1997	4.8%					
1998	4.8%					
Average		-0.194	-4.426	0.0%	-4.052	0.0%

Table 4-7b. Effects of Placements and Referrals on Weeks of Unemployment for Each Year 1987-1995

Year	Washington unemployment rate	Effect on weeks of covered unemployment of Referral	Effect on weeks of covered unemployment of Placement	Deviation of placement effects from average	Effects of placement assuming referral effect measures bias	Deviation of adjusted placements effects from average
1987	7.6%	-0.526	-6.625	31.9%	-6.099	-19.4%
1988	6.2%	0.760	-7.415	-23.8%	-8.174	8.0%
1989	6.2%	0.355	-6.393	-34.3%	-6.748	-10.8%
1990	4.9%	-1.558	-8.965	-7.8%	-7.407	-2.1%
1991	6.4%	-2.644	-11.572	19.0%	-8.927	18.0%
1992	7.6%	-1.855	-10.845	11.5%	-8.990	18.8%
1993	7.6%	-2.411	-10.249	5.4%	-7.838	3.6%
1994	6.4%	-1.630	-8.212	-15.6%	-6.582	-13.0%
1995	6.4%	-4.718	-11.217	15.3%	-6.498	-14.1%
1996	6.5%					
1997	4.8%					
1998	4.8%					
Average		-2.159	-9.725	0.0%	-7.657	0.0%

In reviewing these results, it is important to keep in mind that the standard errors of the estimates are much greater than when the results are not disaggregated. The standard errors are large relative to the small effects of referrals on covered unemployment but still very small relative to placement effects. Thus, some of the variation, particularly in referral effects, is due to samples for individual years being roughly one-tenth the size of the sample for all years together.

Also, the results for the earliest and latest years are affected by not having long strings of pre- or post-unemployment wage records. The 1987 and 1995 results are strongly affected by most claimants not having the wage records needed to determine recall and job change. In addition, the late 1994 and 1995 results are affected by the short followup period preventing accurate estimation of the duration of unemployment. When claimants did not return to work we assumed that they were unemployed for 83 weeks. This assumption is responsible for the exceptionally large estimated effect of 1995 referrals on total unemployment.

The year-by-year results are in keeping with expectations, once we recognize the measurement problems with the earliest and latest years. The effects of referrals and placement on UI payments and unemployment are greatest during 1991-1993, years strongly affected by recessions. In that period placement effects are about 30 percent above average on weeks of UI payments and about 12 percent above average on total weeks of unemployment.

The effect on benefit collection duration is more than twice that on unemployment duration because UI payments were extended by a series of Federal programs in the aftermath of the 1991-92 recessions. The extended benefit programs extended collection from about 26 weeks to a minimum of 52 weeks and, in some cases, as many as 121 weeks. Thus, extended benefits (1) reduced the probability that spells ended with UI exhaustion, and (2) decreased incentives for claimants to take the best jobs that were available, rather than remain jobless.

Thus, it is hardly surprising that the effects of placements on covered unemployment were so large during the period extended benefit programs were in place. Also, it seems reasonable that the effects would fall from a peak of 6.9 weeks to below 3.0 weeks as economic conditions improved and the extended benefit programs were terminated. Importantly, 1990 economic conditions appear to have been similar to conditions in 1997-98. This suggests that current benefits are much lower than those during recessions but still substantial.

It also makes sense that as economic conditions improve, the decline in benefit payments and unemployment duration due to placements are comparable in terms of weeks. But the decline as a percentage of average is far lower for weeks of unemployment than weeks of payments. This result stems from (a) PLX placement services strongly affecting claimants who otherwise would be unemployed for substantial periods without PLX aid in both good times and bad, and (b) the measure of benefit payments being heavily influenced by extended benefit programs that only are in effect during bad periods. Thus, while improvement in economic conditions affects those with the greatest difficulty finding work, the period over which those improvements can be observed is far shorter in the absence of extended benefit programs.

Referral effects show similar patterns to those for placements, but in several cases referrals appear to increase, rather than reduce, collection of benefits. Although it is unlikely that referrals make claimants worse off, obtaining small positive estimates is not especially surprising because most of the effects are close to zero, and it is difficult to control for the poor prospects of finding work that usually prompts use of PLXs in the first place.

4.4.2 Number of Claimants Referred and Placed Each Year

Table 4-8 provides year-by-year information about the number of claimants in our sample, the number referred and placed claimants in our sample, and the ratio of claimants in our database to the number of claimants reported in official statistics.

The ratio of the number of claimants in our database (multiplied times five to reflect the sampling ratio) relative to the number of UI first pays is presented in the box on the bottom right of Table 4-8. These figures show that our sample is complete from 1990 through 1994. We expect that our sample will be greater than first payments because each claimant enters the sample each time he or she establishes a new claim, while first pays reflect the establishment of a new benefit year that can only occur once in a calendar year. The pattern of differences is consistent with the effect of worsening employment prospects and triggering of extended benefit programs on the number of separate spells of unemployment.

Table 4-8. Number and Distribution of Referrals, Placements, and Claimants; Comparison of Claimants in the Database to UI First Payments for Each Year 1987 to 1995

Year	Number of: Referrals (not leading to placements)	Placements	Claimants (spell starts)	Referrals as a percent of claimants	Placements as a percent of claimants
1987	182	175	20,058	0.9%	0.9%
1988	514	364	18,034	2.9%	2.0%
1989	1,272	637	19,943	6.4%	3.2%
1990	3,775	1,274	32,329	11.7%	3.9%
1991	5,709	1,588	50,318	11.3%	3.2%
1992	5,874	1,553	53,968	10.9%	2.9%
1993	6,047	1,501	57,441	10.5%	2.6%
1994	5,771	1,527	52,172	11.1%	2.9%
1995	3,026	892	24,553	12.4%	3.6%
Total	32,190	9,511	328,815		
Average				9.8%	2.9%

Year	Distribution of: Referrals (not leading to placements)	Placements	Claimants (spell starts)	Claimants in the database as a percent of first payments	Placements as a percent all Referrals
1987	0.6%	1.8%	6.1%	66.2%	49.0%
1988	1.6%	3.8%	5.5%	56.8%	41.5%
1989	4.0%	6.7%	6.1%	67.7%	33.4%
1990	11.7%	13.4%	9.8%	89.3%	25.2%
1991	17.7%	16.7%	15.3%	100.7%	21.8%
1992	18.2%	16.3%	16.4%	107.8%	20.9%
1993	18.8%	15.8%	17.5%	114.1%	19.9%
1994	17.9%	16.1%	15.9%	102.8%	20.9%
1995	9.5%	9.4%	7.5%	61.7%	22.7%
Total	100.0%	100.0%	100.0%		
Average				91.8%	22.8%

The 1995 shortfall also is in keeping with expectations because we excluded from this analysis claimants who had not exhausted their benefit entitlement by the end of 1995. However, the shortfalls for 1987-89 suggest that our sample is not complete for those years. Discussions with Washington State officials suggest that the most likely reason for these shortfalls is that some data were not archived when a new management information system was installed during this period.

The 1987-88 data problems clearly extend to referrals and placements. In those years referrals, and to a somewhat lesser extent placements, as a percentage of claimants are disproportionately lower than in other years. For example, 1987 claimants constitute 6.1 percent of all claimants, but 1987 referrals constitute on 0.6 percent of all referrals, and 1987 placements are 1.8 percent of all placements.

We decided to include the 1987 and 1988 data in the study because there are so few placements and referrals in these years that they have a very small effect on the total results. At the same time, their inclusion points up the problems with going back over a long period to retrieve data, as well as the problem with drawing inferences from data covering the first year of a series.

In contrast, referral and placement data for 1995 appear to be proportional to the fraction of claimants. This suggests that adjusting the 1995 service data to compensate for the shortfall would provide an accurate measure of total benefits. However, we decided not to adjust the 1995 estimates because the per-person effect measures are inaccurate due to the followup period being constrained. A key message from this evidence is that it might take states a few years from the point they begin to assemble data needed to monitor PLX operations to the point they can be confident results are reasonably reliable. This puts a premium on starting as quickly as possible.

4.4.3 Total Benefits and Costs by Year

Tables 4-9a and 4-9b display the benefits from referrals and placements reducing UI payments and total unemployment, respectively. The computations were made precisely as they were for all-years-together in Table 4-9c. We multiplied the average effect measured in weeks times the number of instances times the per week decrease in UI payments or increase in earnings. We also used the same measures of weekly UI payments and weekly earnings across all years because the differences once inflation was taken into account were small.

Table 4-9a. Total Yearly Gains from Referrals and Placements Reducing UI Payments

Year	Reduction in UI payments Due to: Referral	Placement	Both	Distribution of reductions	Reduction assuming referral effect measures bias	Distribution of reductions
1987	$118,362	$79,916	$198,278	-0.7%	-$18,605	0.1%
1988	$193,330	-$268,863	-$75,324	0.3%	-$387,383	1.7%
1989	$501,039	-$556,697	-$55,659	0.2%	-$773,906	3.5%
1990	-$641,683	-$2,607,369	-$3,249,052	11.8%	-$2,419,901	10.8%
1991	-$2,376,251	-$4,977,062	-$7,353,313	26.6%	-$4,404,878	19.7%
1992	-$2,314,734	-$6,245,097	-$8,559,831	31.0%	-$5,715,322	25.6%
1993	$925,872	-$4,540,970	-$3,615,099	13.1%	-$4,739,921	21.2%
1994	-$178,815	-$2,810,088	-$2,988,903	10.8%	-$2,769,129	12.4%
1995	$402,860	-$1,496,761	-$1,899,621	6.9%	-$1,394,634	6.2%
Total	-$4,175,742	-$23,422,992	-$27,598,734	100.0%	-$22,354,940	100.0%

Table 4-9b. Total Yearly Earning Gains from Referrals and Placements

Year	Increase in earnings due to: Referral	Placement	Both	Distribution of increases	Increase assuming referral effect measures bias	Distribution of increases
1987	$124,392	$1,507,137	$1,631,529	0.8%	$1,387,529	1.5%
1988	-$507,509	$3,508,567	$3,001,058	1.4%	$3,867,970	4.1%
1989	-$586,983	$5,294,414	$4,707,431	2.2%	$5,588,367	6.0%
1990	$7,646,484	$14,847,554	$22,494,038	10.7%	$12,266,992	13.1%
1991	$19,626,494	$23,888,459	$43,514,953	20.7%	$18,429,206	19.7%
1992	$14,162,863	$21,894,789	$36,057,653	17.1%	$18,150,335	19.4%
1993	$18,955,583	$19,999,246	$38,954,829	18.5%	$15,294,047	16.3%
1994	$12,229,407	$16,301,093	$28,530,500	13.5%	$13,065,206	14.0%
1995	$18,684,265	$13,006,677	$31,690,942	15.0%	$7,535,120	8.1%
Total	$90,334,997	$120,247,937	$210,582,933	100.0%	$93,557,159	100.0%

Table 4-9c. Total Yearly Costs and Benefit-Cost Ratios

Year	PLX Cost	Benefits from referrals and placements as a percent of total cost	Benefits assuming referrals measure bias as percent of: Total cost	Cost of serving claimants (Estimated at 35% of total)
1987	$25,403,602	6.4%	5.5%	15.6%
1988	$25,571,371	11.7%	15.1%	43.2%
1989	$25,573,263	18.4%	21.9%	62.4%
1990	$26,615,821	84.5%	46.1%	131.7%
1991	$26,045,997	167.1%	70.8%	202.2%
1992	$26,344,969	136.9%	68.9%	196.8%
1993	$25,538,436	152.5%	59.9%	171.1%
1994	$26,090,959	109.4%	50.1%	143.1%
1995	$24,319,544	130.3%	31.0%	88.5%
Total	$231,493,961	91.0%	40.4%	115.5%
1990-94	$130,626,182	129.8%	59.1%	168.9%

The pattern of UI payments reductions across years is similar to the pattern in the number of referrals and placements. However, differences between good times and bad are further accentuated because per referral and per placement reductions also strongly fluctuated over the business cycle. (Also, keep in mind that the estimates for 1987, 1988, and 1995 substantially underestimate the true effects, and therefore, are of limited value.)

Total reductions were above $8.5 million in 1992 and almost as great, $7.3 million, in 1991. In years with much stronger economic conditions and no extended benefit programs, active reductions were much smaller. In 1990, the study year with the best economic conditions, reductions were $2.6 million, which is much lower than in 1991-92 but still substantial. Reductions were only a bit greater $2.8 million in 1994. These differences provide further evidence of how much larger payment savings are in periods when extended benefit programs are active.

Interestingly, assuming that the referral effect purely captures measurement bias does not change the results very much because referrals usually have small effects and, in some cases, appear to lengthen covered unemployment. Comparing the distributions of benefits using the two methods clearly shows that assuming that referral effects only reflect measurement bias increases estimates for years with small benefits and decreases estimates for years with large estimates. Because it is reasonable to expect that benefits are proportionally larger when extended benefits programs are active, we feel that at least some of the measured effect of referrals is not due to measurement bias. Thus, we remain confident that assuming that referral effects are due to bias produces highly conservative estimates of the true benefits.

Comparisons between Tables 4-9a and 4-9b show that (a) estimates of earnings gains are many times larger than estimates of UI payment reductions, (b) earnings increases from referrals contribute importantly to the overall gains, and (c) the differences across years are considerably less for earnings gains than reductions in UI payments.

Earnings gains are greatest, $43.5 million, in 1991, but almost as large in 1992 and 1993, $36.1 and $39.0 million, respectively. The gain in 1990, the year with the best economic conditions, is $22.5 million. This is roughly one-half the gain in 1991, the peak year. Most of the decline stems from a one-third reduction in the number of claimants, but some of it stems from a substantial decline in the per-person effect of referrals and placements.

Assuming that referral effects measure bias reduces the estimates of earnings gains by about 45 percent on average. However, the gains are still large. Also, the conservative assumption produces a much more reasonable estimate for 1995, a year when many claimants beginning spells were omitted.

This is an interesting result because the estimated referral effect for 1995 is exceptionally large and it is the only year where results may be strongly biased by not having a long enough followup period (and having to make an assumption about how long claimants were jobless). Thus, it is reasonable to believe that the bias correction may only make sense for this year alone. Additional evidence that bias is confined to 1995 earnings reduction estimates comes from the facts that (a) referral effects on covered unemployment are not especially large for 1995, and (b) covered duration is known with precision.

Finally, the first column of Table 4-9c shows the total budget for Washington PLXs in each year. These estimates include Wagner-Peyser funds, state claimant placement program funds, and veteran program funds, which account for two-thirds, one-sixteenth, and one-sixth of the budget, respectively.

Unfortunately, it is very difficult to determine how much of the funds were spent on direct placement services for claimants (during the period they were collecting benefits). However, we know that budgets do not fluctuate much from year to year. This suggests that much of the cost of direct placement services is the relatively fixed cost of maintaining the computer matching system and providing staff to assist PLX-users.

A ballpark estimate is that roughly 35 percent of the PLX budget goes to providing placement services to the claimants during the first 26 weeks they collected benefits. This figure is based mainly on our estimate that nonclaimants make up about 60 percent of PLX users, but also takes into account the fact that claimants often receive services other than those leading to direct placements. In particular, substantial amounts of staff time go to conducting workshops, providing counseling, and making referrals to alternative services.

Clearly, the gains in earnings accruing to claimants are very impressive even when measured against total PLX outlays. Over all years the benefits equal 91 percent of costs, and lower-bound estimates indicate that the benefits equal at least 40 percent of the costs. These are remarkably large numbers given that the true incidence of service receipt is severely underestimated for 1987 and 1988, and somewhat underestimated for 1989.

When we restrict the estimates to the years for which we are most confident they are accurate, 1990-94, the gains are truly remarkable. Including the value of referrals produces estimates that benefits equal 130 percent of costs for that period. Even assuming that referral estimates represent bias, benefits amount to about 60 percent of costs.

Admittedly, most of the period covered was in recession, when finding jobs was particularly difficult and extended benefit programs further reduced incentives to find work. However, the results were unaffected by extended benefits in 2 of the 5 years, 1990 and 1994, and economic conditions were good in 1990 and improving in 1994. Also, the true incidence of aid given to claimants most likely is understated for 1990.

While results for 1987-89 are much smaller than for later years, we are certain that the incidence of referrals and placements is so severely underestimated that the 1987-88 figures are meaningless. Comparisons with official statistics suggest that the incidence of claims is underestimated by at least 50 percent for 1989. While taking this difference into account does not bring the results up to 1990 levels, it brings them close enough to suggest that some other data problem is the source of the smaller estimates for 1989.

We, therefore, conclude that even in relatively good economic times the benefits to claimants are about 100 percent of the costs assuming our estimates are unbiased. At a minimum, benefits are 50 percent of costs, which is still considerably greater than outlays to produce the benefits.

4.5 Summary and Conclusions

This chapter used an exceptionally large administrative database covering an unusually long period to determine:

- How well these data measure the effect of PLX placements and referrals on UI claimants' job search and benefit collection.
- Whether the techniques used here can and should be used on an ongoing basis to provide meaningful feedback to program operators and policymakers.

The analysis presented in this chapter provides highly positive assessments of both issues. In particular, the analysis suggests that:

1. On average, placements reduce the duration of unemployment by 7.7 weeks relative to being referred but not placed. This nonexperimental estimate of the per-incident effect of placements relative to obtaining referrals are similar to those generated from use of a survey. While several steps should be taken to increase our confidence that these results reflect those from a natural experiment, we feel that the bias in these results is small enough for the similarity to support the view that the non-experimental estimates are reasonably close to the true values.

2. We produced plausible nonexperimental estimates of the per-incident effect of obtaining information in the course of being referred relative to not being referred, and usually not obtaining any PLX help, even though we have no experimental evidence to serve as a benchmark. On average, our procedures produce estimates that referrals reduce the duration of unemployment by 2.1 weeks relative to not being referred. Even if measurement error accounts for half of this figure, the contribution to total benefits is large.

3. Estimates of the total benefits hinge on multiplying the per-incident effect times the number of incidents and average weekly post-unemployment earnings.

    a. The administrative data provided accurate estimates of the incidence of being placed and being referred once the state employment security agency decided to use these data for in-depth analysis. On average, PLXs refer about 25,000 claimants per year, of whom about 7,000 are placed.

    b. Our procedure for estimating post-unemployment earnings is reasonably accurate, but could be greatly improved by applying techniques used with these administrative data in estimating the earnings losses due to job loss. Post-unemployment earnings of claimants average about $260 per week.

4. Estimates of the year-by-year variation in the pre-incident effect of placements and referrals as well as the total benefits derived from direct placement services are much in keeping with expectations. Total benefits are between 30 and 40 percent greater in the trough of a business cycle than near its peak.

    a. About one-third of the peak-to-trough reductions are due to reductions in the per-incident effect of placements and referrals as economic conditions improve. Placements, in particular, are more potent when good jobs are more difficult to find and extended benefit programs considerably lengthen the period over which UI can be collected.

    b. About two-thirds of the reductions are due to there being fewer claimants in total during prosperous periods.

5. The cost of running the entire PLX system in Washington is easily derived from budget figures. Those costs average about $25 million per year and vary very little

from year to year. Sixty percent of the funds come from the Federal Wagner-Peyser allocation, 15 percent from Federal Veterans funding, and the bulk of the remainder from state funds earmarked to assist claimants. It is not nearly as easy to determine how much of the total goes to providing direct placement services, nor how much of those funds goes to aiding claimants. We use a ball park estimate of 35 percent for the portion spent on direct placement services going to claimants included in the benefit calculations.

6. We produce the following three different benefit-cost estimates:

   a. The lowest estimate assumes that the value of the information obtained in getting referrals is zero. Thus, this measure only includes the value of placements relative to being referred but not placed. This produces a benefit-cost ratio of 1.2 (using the 35 percent estimate of the portion of costs going to direct placement services and including 1987-89, and 1995 years for which the incidence of placements and referrals is substantially underestimated).

   b. A less conservative estimate assumes that half of the measured referral effect is due to measurement error. This assumption produces a benefit-cost ratio of 1.9.

   c. The least conservative estimate accepts the referral effect measure as being accurate. This assumption produces a benefit-cost ratio of 2.8.

   Because the per-incident cost of placements and referrals is very low, we feel that even the least conservative estimate is plausible. Key reasons for this view are that: (1) receipt of UI payments greatly restricts the types of jobs claimants are likely to accept before exhausting benefits, (2) some claimants would have great difficulty finding suitable work on their own after they have been jobless for several months and (3) we included 1987-89 and 1995 years for which the incidence of placements and referrals is substantially underestimated. Thus, we think it is likely that placements and referrals will be particularly potent for claimants.

   The trend in unemployment reductions stemming from placements and referrals obtained after successively longer periods of being unemployed strongly supports the view that, as unemployment duration lengthens, claimants have more and more difficulty finding jobs on their own. The per-incident effects just about double from between the 9th and 19th week of unemployment spells.

7. Separate estimates suggest that UI payment reductions equal about 22 percent of the gain in earnings. Payment reductions are lower because weekly payments are about one-half of weekly earnings and because payments end after about 30 weeks, on average. Thus, except during periods covered by extended benefits, reductions in UI payments are modest relative to the cost of providing direct placement services to claimants. Nevertheless, employers benefit both from the reduction in UI payments, which usually will reduce their UI tax burden, and from claimants' increased earnings. By lowering the cost of job loss, placement services reduce "the risk premium" workers would require to take jobs that are likely to end with permanent layoffs.

Overall, we strongly feel that program operators, policymakers, and concerned citizens of all types would be much better off using the type of information described above in assessing the performance of PLXs than the types of data that have been used to date. Second, we do not think it would be difficult to produce these measures on an ongoing basis.

Of primary importance, comparing benefits to costs is a much better way to judge both overall performance and determine ways to improve performance of any program. In particular, it focuses attention on program effects rather than outcomes that often largely have nothing at all to do with program effectiveness. Also, use of the more sophisticated value-added measures that require use of comparison groups for one program might lead to improving measures and the conceptual framework for viewing other employment and training programs.

Finally, in our view presenting three benefit-cost estimates is not unduly confusing, but rather honestly reflects how difficult it is to overcome key measurement problems. Also of considerable importance, adoption of the measurement system used here would produce the straightforward measures that have been used in the past and improvements on those measures. For example, we could determine the placement and referral rates as a function of how long claimants were jobless prior to receiving this aid, and further determine how many referred but not placed claimants were recalled to former jobs. These measures should help put the contributions of PLXs into a far more relevant context than can use of the current ETA-9002 measures.

Because we feel that the range of estimates of placement and referral effects provides a reasonably accurate view of benefits going to claimants, it is our view that the biggest improvements in PLX measures would come from expanding the above measures to include:

a. Claimants who have stopped collecting benefits;

b. Other unemployed job seekers with strong work records;

c. Unemployed job seekers with spotty work records; and

d. Job seekers searching while employed.

The primary impediment to measuring placement effects for these groups with administrative data is not knowing for sure when these individuals began to search for work, and if they failed to find new jobs, when they stopped searching. Because claimants are supposed to be able, available, and have not refused suitable jobs, UI payment data provide a reasonably accurate measure of periods of job search. In this study we did not even examine referral and placement effects after claimants stopped collecting benefits because we were concerned these measures would be biased due to the difficulty in determining whether claimants are searching for work after they stop collecting benefits.

We think that it should not be too difficult to at least determine the duration of job search to make reasonably accurate estimates of the effect of being placed relative to being referred but not placed among members of all four groups. Indeed, the one-time use of a mail survey with telephone followup would provide a benchmark for developing unemployment duration measures using administrative data alone, and adjusting nonexperimental estimates to reflect those generated from a natural experiment.

What would be extremely difficult to do is develop job search duration measures for individuals who could serve as a comparison group of nonreferred individuals who have spotty work records or search while employed. Fortunately, even if we omitted these estimates, we would be able to produce lower-bound estimates for all groups, and less conservative estimates for the vast majority of referred individuals. An important, but far less critical, improvement is more accurately translating reductions in weeks unemployed into increases in earnings, as already noted in point 4a above.

In short, while we consider our estimates to be far from perfect, they dramatically improve on the measures that are currently in use as well as those advocated to replace existing PLX measures. Indeed, in our view the primary threat to greatly improving PLX measurement is instituting self-service computerized systems to review PLX job orders that provide contact information without requesting that users register with PLXs and identify which job orders they are likely to pursue.

Chapter 5

5. DETAILS OF OUR RESEARCH USING OREGON ADMINISTRATIVE DATA

In this chapter, we describe our analysis of the effect of PLX referrals and placements on Oregon unemployed insurance (UI) claimants. We do this using a database developed by the Oregon Employment Department (OED) to our specifications. One goal of this work was to see if the large Washington benefits were typical of results in other states. A second goal was to see if the methods used for the analysis could be implemented by a state employment security agency.

Because the design of this work is nearly identical to that just discussed in Chapter 4, we do not need to go into detail about why we limited the study to claimants or explain the conceptual framework for the analysis. Instead, we describe the process by which we developed the analytic database. We then describe our results derived from use of a model that differs only slightly from the one discussed in Chapter 4. Finally, we summarize what we learned about the effectiveness of placements and referrals, and how states can develop similar databases for measuring public labor exchange (PLX) effectiveness.

5.1 Database Development

This study was designed to replicate the Washington administrative data analysis as closely as possible. However, rather than Westat working with raw wage, claim, and PLX files on our own computer, as was the case with the Washington analysis, Westat provided detailed descriptions of the data processing steps to the OED, who then performed all the data processing. Westat worked with OED officials to check whether the database was properly created and to correct problems. Ultimately, we specified the analytic model and analyzed the results, but we did not have copies of the person-level files used to run the regressions described below.

The data processing tasks consisted of the following three elements:

1. Creating an analytic file from the raw files;
2. Ensuring that the analytic file was sound; and
3. Estimating the effect of placements and referrals on claimants' job search.

One element did not smoothly follow completion of the preceding element. Some problems were only spotted at the point estimates were produced and inconsistencies became apparent. Thus, some programs that we thought were sound had to be refined. Even at the end of the project, it still appeared that the database did not capture all the services received by claimants.

Overcoming the problems we encountered was part of the normal process of using administrative data for in-depth analysis. Similar problems were encountered in our use of data from Washington and other states. However, given sufficient time and resources it has always been possible to ultimately produce a sound analytic database.

Constraints imposed by not being able to validate the models we used with experimental evidence are far more important impediments to accurately estimating PLX effectiveness than are problems in creating analytic files. Indeed, we refined our analytic models using the Washington data concurrently with developing the Oregon database. In some cases, it was only after we refined our models that some of the problems with the data became apparent.

It is also worth noting that prior to the start of our joint project OED had made a major investment in creating a data warehouse. Thus, the basic "raw" data files needed for this work were available about 2 years ago when our collaboration began. The preliminary steps conducted by OED on its own included:

1. Developing a common format for each type of file across years;
2. Providing excellent documentation for the contents of each file; and
3. Storing the data in a way that made it easy to process.

Having this start saved 3 to 5 months of hard work and reduced expenditures by as much as $300,000.

In order to assist OED in reformatting the data to produce an analytic file, we provided what we called "The Cookbook"—a large loose-leaf folder with the precise details of every processing step we carried out using the Washington data. The new database was created by adapting the information we provided to the OED's computer environment.

Much of this work was carried out by OED staffers who had completed or almost completed doctorates in economics from Oregon State University. Having staff with extensive experience with structuring data for analysis and estimating models using statistical packages was a major asset to this project and substantially reduced the time and effort needed to produce the results.

After the database was created, we ran tests to determine whether the data were sound. Our initial comparisons used a simplified model that suggested that some of the crucial variables were not properly constructed. We then made minor adjustments to the programs to create variables that appeared to be precisely correct. However, as we got deeper into the analysis, it became clear that we had not caught all the errors. We, therefore, conducted far more detailed tests of the accuracy of the variables. This led to making extensive modifications to the basic programs.

Finally, we applied even more sophisticated models and produced what appeared to be reasonable estimates of the effects of referrals and placements on claimants' duration of joblessness and collection of UI benefits. However, after converting the per-person effects to total effects, we determined that the number of referrals and placements in the database were only about one-half of the number reported in published tables. We were able to accurately estimate total effects, but could not determine why the database was incomplete.

The next section summarizes the key lessons we learned about how to make the database development process go smoothly.

5.1.1 Lessons for Developing Analytic Files

There are two separate issues in creating sound analytic files:

1. Ensuring that each variable for each person is properly created; and
2. Ensuring that the database includes the right number of individuals in total and the right number of individuals receiving various types of services.

The cookbook we provided had the information needed to properly create each variable because it included: (a) a description of each variable, (b) the computer code used to produce the variable, and (c) a detailed printout of how the raw data looked and how the transformed data looked after the computer program was run. Unfortunately, OED was not able to simply run the programs we created for use with Washington data. Instead, they wrote their own programs using a different computer language.

In such cases, it is imperative to very carefully check how the programs work by looking at the variables in the input and output files.

Initially, we did not request detailed formatted data dumps, but eventually we had to produce these dumps in order to resolve problems with the data. There is no question that we should have requested and reviewed the detailed dumps right from the start.

Even if we did not spot problems immediately, having these files would have made it far easier to resolve issues when they arose. A key problem is that many complex steps had to be completed in order to produce the analytic files, and without a complete set of data dumps at each stage, it was difficult to determine at what step something might have gone wrong. Indeed, by the time a question surfaced it was common for the person doing the programming to have left OED or be unable to recall precisely what was done.

Although we believe that our checks ultimately led to properly transforming the variables in the raw files, we were unable to determine why there were only about one-half of the number of referrals and placements in our database as there were in published statistics. Indeed, we believe that the shortfalls were in the raw files and not due to errors in the way the data were processed. However, this is not certain. Clearly, we should have made the comparisons between aggregate statistics and tabulations of the raw files and continued to create counts at each stage in the processing to make sure that data continued to be complete.

One reason that we did not make the detailed comparisons using Oregon data initially is that we knew at the outset that (a) Oregon's labor force was about 60 percent the size of Washington's; and (b) the number of claimants, referrals, and placements in the Oregon database were roughly 60 percent of those in Washington. Unfortunately, what we did not know was that the Oregon PLX job banks included a far higher proportion of all job vacancies than did the Washington PLX job banks and that Oregon assists a far higher proportion of claimants than does Washington.

A second reason for not making these comparisons is that we assumed that some of the checks were carried out in the course of creating the data warehouse. However, in retrospect it is obvious that it is a mistake to take any key assumption for granted. We can not emphasize too strongly that it is essential to assemble relevant published statistics at the outset and determine at each stage that tabulations of the person-level files match those published statistics.

5.2 The Per-Incident Effect of Referrals and Placements

The estimating equation used with the Oregon data was almost identical to that used with the Washington data. The only important difference is that the Oregon estimates lacked the final refinement in the Washington model of restricting our measure of the effect of referrals not leading to placements to claimants who were not placed at any time within 39 weeks of the start of a given unemployment spell. (See Appendix section D.1 for the list of variables.)

The restriction was made because otherwise much of the positive effect attributed to referrals not leading to placements, in reality, would be due to subsequent placements. Fortunately, this difference did not have much effect on the measures of total benefits because most Oregon referrals and placements occurred before the tenth week of unemployment, and most of those claimants returned to work in that period. Thus, subsequent placements were rare.

Panel A of Table 5-1 presents the key Oregon results. These results cover only one calendar year, 1995. On average, a referral not leading to a placement reduces the duration of claimants' unemployment by about 1.1 week, and a placement reduces unemployment by about 5.8 weeks. Subtracting the referral effect from the placement effect produces a 4.6-week estimate for the reduction in unemployment following a placement relative to viewing listings and receiving staff assistance but not being placed.

As noted in earlier chapters, there is some uncertainty about whether the 1.1-week reduction in unemployment associated with referrals not leading to placements accurately measures the value of obtaining information that helps claimants decide to take jobs they find on their own and accept offers made as a result of referrals. It is possible that at least some of this reduction is due to measurement error. Thus, we later display estimates of total benefits with the referral effect included and the effect excluded.

Placement effects (relative to not being referred) increase markedly after the ninth week of unemployment and then increase markedly again after the 18th week. This pattern, coupled with the fact that almost three-quarters of all claimants return to work by the tenth week of their unemployment spell, suggests that Oregon referral and placement effects are smaller than those in Washington because most claimants who are aided were likely to return to work relatively quickly even without receiving those services.

Table 5-1. Effects of Referrals and Placements on Weeks of Unemployment and UI Payments Sample Size and Percent Referred and Placed, for Five Periods and Average for All Periods Together, Oregon 1995.

Panel A. Weeks of Unemployment

Weeks when services were delivered	Effect on weeks of unemployment of: Referral not leading to placement A	Placement B	Placement relative to being referred C	Number of spells D	Percent of spells with: Referral not leading to placement E	Placement F	Placements as a % of referrals G
1. 1	-2.292 ***	-5.499 ***	-3.207	138,280	2.9%	0.4%	11.8%
2. 2-9	-0.171	-5.223 ***	-5.052	115,625	12.0%	2.7%	21.0%
3. 10-13	-2.781 ***	-6.943 ***	-4.162	59,286	4.6%	0.8%	16.6%
4. 14-18	-2.550 ***	-5.981 ***	-3.431	45,535	4.2%	0.7%	16.0%
5. 19-26	-1.293 ***	-9.236 ***	-7.943	33,192	5.5%	1.0%	16.6%
6. Average	-1.131 ***	-5.751 ***	-4.620	78,384	5.9%	1.1%	16.4%

Panel B. Weeks of UI Payments

Weeks when services were delivered	Effect on weeks of UI payments of: Referral not leading to placement A	Placement B	Placement relative to being referred C	Number of spells D	Percent of spells with: Referral not leading to placement E	Placement F	Placements as a % of referrals G
1. 1	-0.397 ***	-2.452 ***	-2.055	138,280	2.9%	0.4%	11.8%
2. 2-9	1.328 ***	-2.284 ***	-3.612	115,625	12.0%	2.7%	21.0%
3. 10-13	-0.099	-2.915 ***	-2.816	59,286	4.6%	0.8%	16.6%
4. 14-18	-0.073	-1.439 ***	-1.366	45,535	4.2%	0.7%	16.0%
5. 19-26	-0.074	-2.086 ***	-2.012	33,192	5.5%	1.0%	16.6%
6. Average	0.665 ***	-2.295 ***	-2.961	78,384	5.9%	1.1%	16.4%

Note 1: *** = significant at the .01 level.

Note 2: See Appendix D sections D.2 and D.3 for regression estimates.

In contrast, most Washington claimants helped by direct placement services would have been unemployed considerably longer than other claimants had they not received aid. Part of the difference is that the Oregon results cover only 1995, while the Washington results apply to 1987-95, years that included a major recession and an extensive period where extended benefit programs were active.

There also are several possible additional reasons for these differences. Oregon may have given claimants incentives to search harder and more effectively for work over their entire spell of unemployment than Washington by (a) enforcing the UI work test more stringently, (b) requiring most claimants to register in person with the PLXs, (c) making receipt of more PLX services mandatory, and (d) more readily providing additional voluntary supportive than Washington.

Panel B of Table 5-1 displays estimates of the reduction of weeks of UI payments. Placements are associated with a 2.3-week reduction in the number of weeks UI payments are made. However, referrals not leading to placements are associated with a 0.7 more weeks of payments. Thus, placements relative to viewing listings (rather than not receiving referrals) lead to a 3.0-week decrease in the number of weeks with payments.

The small increase in payments due to referrals is primarily a result of a 1.3-week gain in the period including the second through ninth week of unemployment. About 55 percent of all referrals are made in this period, and Oregon claimants might slightly delay taking new jobs in order to review listings or comply with registration requirements. Alternatively, the small positive effect might be due to measurement error.

5.3 Estimating Total Benefits

To estimate total benefits, we need to multiply estimates of the per-incident effect of referrals and placements times the number of incidences. The administrative data provide estimates of both the per-incident effect and number of incidences. Chapters 3 and 4 detail why we believe that our procedures for estimating the effect of placements (relative to viewing listings and receiving staff assistance) are reasonably accurate, and why it is reasonable to use two alternative measures of the effect of viewing listings and receiving staff assistance relative to not receiving referrals (and usually not receiving any PLX aid).

Chapter 4 also describes the procedures we used to ensure that the measure of incidence of referrals and placements is accurate. First, we compared the number of spells of unemployment in the Washington database for each year 1987 through 1995 to the number of first payments reported in official UI statistics. Second, we compared the referral and placement rates across the years.

Our findings were that the Washington data were incomplete for the first 3 years and for the last year. The problems with the early years were traced to not retaining all the data when the computer systems were updated. However, a deeper reason is that interest in creating longitudinal files for analysis from the administrative database did not begin until 1992. Thus, there was little reason to be concerned about retention of the 1987-89 data beyond a 3-year period required to ensure UI claims were being properly made. In contrast, the data for the last year in the series were incomplete simply because the data were not available for all of 1995 when the database was created.

Because the Washington files included data covering all unemployment spells, referrals, and placements for the 1990-1994 period, we focused our estimates on this period and felt that it was not necessary to estimate the incidence of these events for the other years. However, we only had estimates of the per-incident effects in Oregon for a single year, 1995. Thus, to accurately estimate total benefits it was necessary to determine if the incidence of unemployment, referrals, and placements was accurately reported in the administrative data, and if it was not accurate, use official statistics to produce accurate estimates.

The next subsection describes (a) how we used published statistics to estimate the actual incidence of unemployment, referrals, and placements in both Washington and Oregon for 1995; (b) how these statistics compared to analogous figures derived from the person-level administrative files, and (c) how we used the Washington comparisons to adjust the Oregon figures to accurately measure the incidence of referrals and placements. We then used these figures to produce estimates of total benefits in the following subsection.

5.3.1 Measuring the Incidence of Referrals and Placements

Table 5-2 displays key measures of PLX usage and services provided to claimants in Washington and Oregon covering 1995. Because the published data cover program years stretching from July of 1 year to June of the next year, we averaged the figures for program years 1994 and 1995 to obtain measure covering calendar year 1995.[1] Line 1 of panel A of Table 5-2 presents the total number of job seekers registering with PLXs in each state. Line 2 displays the number of claimants who registered, and lines 3 and 4 display the number of claimants who were referred and placed, respectively. (In this case referrals include those who were, and were not, placed.)

The key findings based on panel A are that the official statistics for Washington and Oregon are quite similar even though Oregon's labor force is only 60 percent of the size of Washington's. The proportion of registrants who are claimants is slightly lower in Oregon than Washington. The proportion of claimants who are referred is slightly higher, and proportion of claimants who are placed, given they are reviewed, also is slightly higher.

Panel B of Table 5-2 presents statistics on the number of unemployment spells, as well as the number of referrals and placements made to claimants recorded in the person-level administrative databases. The Washington and Oregon figures are for 1995, but the Washington figures had to be adjusted for the shortfall due to the data covering only a portion of that year. We multiplied the partial-year numbers by 1.67 to make the adjustment. This figure was used because we assumed that the ratio of claimants in the person-level file in 1994 to first payments observed in 1994 would also prevail in 1995.

The variables in panel B are not identical to those in panel A. Claimants who register with PLXs reported in panel A could be considerably less than the number of spells of unemployment because (a) not all claimants register with PLXs, and (b) some claimants have more than one spell of unemployment in a calendar year.

[1] The statistics for program years 94 and 95 were quite similar. Referrals and placements as a percent of claimants registered with PLXs were almost identical. However, there were about 15 percent fewer claimants in program year 95 than 94. We, therefore, doubt that averaging the figures for the 2 years has much, if any, affect on the comparisons.

Table 5-2. Claimants and Claimants Referred and Placed Measured with Published Statistics and Tabulations of Person-level Files, Oregon and Washington 1995.

Panel A. Measures Derived from Published Statistics for Program Years 94 and 95

		Oregon		Washington		Oregon Washington (col A / col C)
		Number A	Percent of applicants B	Number C	Percent of applicants D	E
1	Applicants	410,832		420,882		97.6%
2	Claimants	200,897	48.9%	223,312	53.1%	90.0%
			Percent of Claimants		Percent of Claimants	
3	Referred	54,605	27.2%	54,517	24.4%	100.2%
4	Placed	13,579	6.8%	11,524	5.2%	117.8%
5	Placed/Referred	24.9%		21.1%		117.6%

Panel B. Measures Derived from Tabulations of Person-Level Files

		Oregon		Washington		Oregon/ Washington
		Number A	Percent of panel A measures B	Number C	Percent of panel A measures D	E
6	Claimants	132,280	65.8%	232,701	104.2%	56.8%
7	Referrals	25,865	47.4%	34,648	63.6%	74.7%
8	Placements	4,726	34.8%	7,533	65.4%	62.7%
9	Placed/Referred	18.3%		21.7%		

Panel C. Adjusted Oregon Measures

		Oregon		
		Number A	Percent of applicants B	Adjustment factor (panel B col D / col B) C
10	Claimants	209,344	104.2%	1.58
11	Referrals	34,704	63.6%	1.34
12	Placements	8,876	65.4%	1.88
13	Placed/Referred	25.6%		

Referrals and placements to claimants in panel A could be greater than those reported in panel B because panel B only includes those made while benefits are being received within the first 26 weeks of an unemployment spell. It is likely that many claimants use PLX services and accept jobs just after benefit collection ends because either benefits are exhausted or claimants stop collecting to leave some funds available for subsequent unemployment. On the other hand, the statistics in panel B allow for multiple referrals, while those in panel A are supposed to reflect the number of different people referred.

Because the statistics in the two panels are not identical, it is instructive to compare the two different statistics for Washington where we are reasonably confident the database is complete. Thus, shortfalls in referrals and placements should reflect the effect of the restrictions imposed on the database, not an inherent difference in the population being observed. The Washington comparisons show that the number of spells is 4.2 percent greater than the number of claimants registering, suggesting that most claimants register with PLXs. However, both the number of referrals and the number of placements in the database are only about 65 percent of those reported in official publications. This suggests that substantial use of PLXs is made by claimants at or near the point benefits are exhausted and are, therefore, not included in our measures.

Panel B also shows that all the Oregon statistics in panel B are considerably less than analogous statistics in panel A and, even more importantly, are considerably less than the same statistics in panel B for Washington. We conclude from these comparisons that it is highly likely that the Oregon administrative data do not cover all claimants and all PLX services provided to the claimants included.

In particular, it is hard to see how the number of spells of unemployment included in administrative file could be smaller than the number of claimants registering with Oregon PLXs. In contrast, the number of referrals and placements in panel B easily could be lower than the number in panel A. However, the shortfalls should be proportional to those in Washington.

As noted earlier, the same pattern of shortfalls emerged in the first several years of the Washington administrative data used in Chapter 4. There were substantial shortfalls in the number of spells of unemployment and even more substantial shortfalls in the number of referrals and placements. We, therefore, decided to adjust the Oregon figures so the proportion of spells, referrals, and placements in the administrative files relative to those reported in official publications would be equal to those in Washington.

Column C of panel C in Table 5-2 shows the adjustment factors for each variable needed to equalize the proportions in Oregon with those in Washington. By far the largest adjustment, an 88 percent increase, was made to the number of placements. The smallest adjustment, a 34 percent increase, was made to the number of referrals. This adjustment was smallest because the Washington data excluded many cases where multiple referrals were made when ultimately the claimant was placed, but this adjustment was not made with the Oregon data. As a result, the adjustments reduce a potential source of overestimation of the total benefits.

Finally, Table 5-3 displays some published statistics for 1998 that help explain why Oregon, whose labor force was only 60 percent of Washington's, was able to refer and place about the same number of claimants as Washington. Unfortunately, we were unable to secure comparable data for 1995. However, we believe that they reflect differences that also prevailed in 1995.

First, lines 5 and 6 display the proportion of all referrals and all placements that were made to claimants in the two states. In Washington, only about 25 percent of referrals and placements were made to claimants, compared to about 38 percent in Oregon. Although both states have special programs to aid claimants, Oregon's program has much greater funding in total and even greater funding per claimant. Thus, far more of PLXs resources are targeted on claimants in Oregon than Washington.

Second, line 7 shows the number of job openings listed with PLXs. Despite Oregon having only about 60 percent of Washington employment, Oregon's PLXs were able to secure about 4.3 percent more openings. Clearly, the more openings that are available the more openings can be filled. However, line 11 shows that the proportion of openings filled was higher in Washington than Oregon. This seems to be a natural outcome of Washington having more applicants per opening than Oregon in 1998. Line 12 shows that the average wage of job openings listed with PLXs in Oregon were a bit higher than those in Washington. This, too, is evidence that Oregon PLXs were able to obtain a broader range of jobs than Washington PLXs.

Most likely, Oregon PLXs were able to obtain a greater number and range of job orders because they used some of their greater funding to put more effort into securing more listings.

Table 5-3. Selected Statistics Describing Direct Placement Services and Job Order Characteristics in Washington and Oregon from July 1997 through June1998 and Labor Force Characteristics for 1990.

	Oregon A	Washington B	Oregon / Washington (col A / col B) C
A. Referral and Placement Characteristics			
Total Referrals			
1a. Number	197,848	171,299	115.5%
1b. % of applicants	46.4%	26.3%	
Total Placements			
2a. Number	44,098	46,182	95.5%
2b. % of applicants	10.3%	7.1%	
Claimant Referrals			
3a. Number	75,255	44,870	167.7%
3b. % of claimant applicants	36.1%	10.8%	
Claimant Placements			
4a. Number	17,342	11,144	155.6%
4b. % of claimant applicants	8.3%	2.7%	
5. Percent of all referrals made to claimants	38.0%	26.2%	
6. Percent of all placements made to claimants	39.3%	24.1%	
B. Job Order Characteristics			
7. Openings	152,304	145,994	104.3%
8. Orders	92,803	65,185	142.4%
9. Openings per order	1.64	2.24	
10. Openings filled	54,150	60,225	89.9%
11. % of openings filled	35.6%	41.3%	
12. Average wage of listings	$8.89	$8.58	103.6%
13. Average wage of placements	$8.18	$8.56	95.6%
C. Labor Force Characteristics			
14. Population (millions)	2.854	4.888	58.4%
15. Employment (millions)	1.327	2.275	58.3%
16. Employment / Population	46.5%	46.5%	

Although not shown in Table 5-3, the recall rate among claimants (not necessarily registrants) derived from the person-level files suggests that Oregon was able to place more claimants because a lower proportion was recalled, and therefore, its claimants were considerably more likely to need to find a new job.

5.3.2 Estimates of Total Benefits

In Table 5-4, the adjusted estimates of the number of referrals and placements in Oregon shown in Table 5-3 are multiplied by the per-incident effects of referrals and placements shown in Table 5-1 to estimate the total reduction in the number weeks of unemployment resulting from direct placement services.

Table 5-4 displays comparable results for Oregon and Washington using two different estimates of the total effects. The estimates presented in columns A and C use incident estimates derived from the person-level files. The estimates presented in columns B and D use incident estimates based on published statistics.

The Washington results shown in columns C and D use 1995 estimates of the number of referrals and placements, but 1994 estimates of the per-incident effects. The estimate of the effect of placements (relative to obtaining referrals) derived by subtracting the referral effect from the placement effect is almost the same in 1994 and 1995. However, the 1995 referral effect is overestimated because we were unable to track the return to work over a sufficiently long period. That is why we use the 1994 estimates.

Two different measures of the benefits also are presented. One only includes the effect of placements relative to obtaining a referral. These estimates are analogous to those described in Chapter 3 and verified as accurate using a natural experiment. The other adds to the above estimate the benefits from the “referral effect”-- the effect of obtaining a referral and staff assistance both on those referred but not placed and those placed. The accuracy of the referral effect could not be verified using any type of experimental design. Thus, some of the positive effect could be due to self-selection or other forms of measurement error.

Table 5-4. Estimates of Total Benefits Due to 1995 Referrals and Placements of Claimants in Oregon and Washington

	Oregon incidents based on		Washington incidents based on	
	Person-level file A	Published statistics B	Person-level file C	Published statistics D
1. Referral incidents	34,704	54,605	34,648	54,517
2. Placement incidents	8,876	13,579	7,533	11,524
Reductions in weeks of unemployment:				
3. Per-referral	1.1	1.1	1.6	1.6
4. Per-placement	4.6	4.6	6.6	6.6
Total reduction in weeks of unemployment due to:				
5. Referrals (line 1 x line 3)	38,175	60,066	55,437	87,227
6. Placements (line 2 x line 4)	40,829	62,463	49,718	76,058
7. Average weekly earnings of claimants aided	$247	$247	$260	$260
Total earnings increase due to:				
8. Referrals (line 7 x line 5)	$9,429,225	$14,836,302	$14,413,574	$22,679,072
9. Placements (line 7 x line 6)	$10,084,763	$15,428,361	$12,926,563	$19,775,184
10. Total earnings increase due to referrals and placements	$19,513,988	$30,264,663	$27,340,137	$42,454,256
11. PLX Budget in 1995	$26,000,000	$26,000,000	$24,300,000	$24,300,000
Earnings increase as a percent of budget:				
12. Due to placements	38.8%	51.0%	53.2%	81.4%
13. Due to referrals and placements	75.1%	116.0%	112.5%	174.7%
14. Percent of budget spent on claimants	38.0%	38.0%	25.0%	25.0%
Benefit-Cost Ratio				
15. Due to placements	1.02	1.56	1.26	2.13
16. Due to referrals and placements	1.98	3.06	2.62	4.50

Notes: In this table the per-incident effect of referrals is relative to no referrals. Thus, the measure includes the positive effects of obtaining information from viewing listings and receiving staff assistance that helps claimants more quickly find work on their own or accept jobs to which they were referred.

The per-incident effect of placements is measured relative to acquiring the information needed to obtain a referral and obtaining a referral that does not lead to a placement.

The incident of referrals include referrals not leading to placements and leading to placements. The percentage of budget spent on claimants (line 14) is based on the fraction of all referrals made to claimants.

Lines 1 and 2 of Table 5-4 show the number of referrals (both those leading and not leading to placements) and number of placements. Lines 3 and 4 show the per-incident effect of referrals (relative to no referrals) and placements (relative to being referred). Lines 5 and 6 show the total reduction in weeks of unemployment derived by multiplying the incidents times the per-incident effect.

In order to translate the total number of weeks into an increase in earnings, we multiply the numbers on lines 5 and 6 by $247 for Oregon and $260 for Washington. The Washington earnings numbers were derived by examining the post-unemployment earnings of referred and placed claimants. We did not have comparable data for Oregon, so we adjusted the Washington earnings using the ratio of average hourly wages of placements in the two states.

Line 9 shows the increase in earnings due to placements (relative to being referred). This is the lower bound estimate of the benefits. Line 10 shows the sum of the referral and placement effects. Line 11 shows the cost of running all PLXs in 1995, and lines 12 and 13 show the percentage of total costs covered by the benefits accruing to claimants alone with referral effects excluded and included, respectively.

In both Oregon and Washington, there are slightly greater total reductions in weeks of unemployment due to referrals than due to placements. This is because the number of claimants referred is about five times greater than the number placed, but the per-incident effect of placements is only four times the effect of referrals.

However, the total reduction in weeks of unemployment is about 45 percent greater in Washington than Oregon. This is the case because the number of incidents is about the same in both states, but the per-incident effect is about 45 percent greater in Washington.

Using the lower-bound estimate of total benefits for Oregon shows that the benefits accruing to claimants while they are collecting benefits up to the 26th week of their unemployment spell is equal to 38.8 percent of total costs. However, published statistics suggest that only 65 percent of claimants' referrals and placements are included in the column A estimates.

If we use the published statistics to estimate the incidence of referrals and placements, benefits going to claimants, the lower-bound estimate of claimant benefits increases to 51.0 percent of costs. This is substantially greater than the estimate presented in Table 5-3 that 38 percent of the costs of PLX services go to helping claimants.[2]

Naturally, if we include the referral effect the benefits even further exceed the portion of all PLX costs estimated to go toward claimants. Indeed, the estimate of total effects in column B, which includes the total number of placements is over 4 million dollars more than the cost of running the entire PLX.

While the Oregon results are quite impressive, the Washington results are even larger. The minimum estimate suggests that benefits derived from placements to claimants cover twice the cost of this activity, while the maximum estimates suggest that the total benefits going to claimants is about 75 percent greater than the entire cost of running the Washington PLXs.

5.3.3 Why Total Benefits Differ between Oregon and Washington

It would be of great value to improving the performance of PLXs to know why the results differ between Oregon and Washington. Unfortunately, at this time we can only speculate on why the differences exist, but their existence suggests that it would be highly fruitful to examine the following plausible hypotheses:

1. Oregon has implemented procedures that make it more likely that claimants will intensively search for new jobs, or if they are not willing and able to search intensively, they stop collecting benefits.

2. Oregon has used its resources to attempt to directly aid a far larger fraction of claimants. As a result, a higher proportion of claimants who are likely to return to work on their own receive services.

3. The characteristics of Oregon's workers and/or labor markets are such that claimants, in general, are more likely to find work on their own.

The first hypothesis suggests that the comparison group has been strongly affected by policies implemented by its PLX and UI systems that give claimants incentives to assiduously search for

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[2] If we assume that the placement effects for claimants after they stop collecting benefits equals the effect observed for period 5 covering weeks 19 through 26, the average reduction for the 35 percent of claimants omitted from our sample would be 7.9 weeks, not the 4.6 weeks shown in Table 5-1. Thus, not using separate estimates for the omitted groups very likely leads to the estimates in column B and D being even more conservative than those in columns A and C.

work or end benefit collection. If this is the case, the policies lower the value of direct placement services relative to those in Washington. However, a fuller analysis of the benefits and costs of PLX activities would indicate that a lot of benefits of these activities are omitted from this analysis. Thus, the total benefits of all PLX (and UI) system activities could be far greater in Oregon than Washington.

The second hypothesis was mentioned in Section 5.3.1 as a possible explanation for why the per-incident effects are lower in Oregon than Washington. Pursuing this hypothesis could be of particular value, since it suggests that the same benefits could be attained by improving the targeting of services to claimants. If additional analysis could identify groups of claimants whose benefits from direct placement services do not cover the costs, it would be possible to raise the total benefits by providing less services to those groups. For example, instead of requiring all claimants to register in person and review listings, it might be possible to limit this requirement to those claimants most likely to be affected by this policy.

The third hypothesis suggests that factors outside the control of PLX and UI policies account for the differences. While there are some substantial differences in the characteristics of claimants in the two states, it is likely that most of these differences have been taken into account in producing the estimates.

The best way to test all three hypotheses would be to combine the databases for both states, add variables that better reflect policy differences in availability of services and mandatory requirements to take certain action, and then reestimate the effects taking those differences into account. Use of cross-state differences is particularly important because it is very difficult to use data from one state alone to assess the effect of policies uniformly applied to claimants in that one state.

Because of Oregon’s more stringent confidentiality restrictions, we could not carry out this analysis at Westat as part of this study. However, we hope to do so as part of on-going work by transferring the Washington data to the Oregon Employment Department.

5.4 Summary and Conclusions

The Oregon study addressed two questions:

- Could a state employment security agency replicate the Washington analysis we carried out using its own staff and computers with modest help from us?
- Are the results for a second state as large as those for Washington?

We determined that the answer to the first question is definitely yes. However, any state attempting to develop the person-level database needed to replicate the analysis should recognize that this is a very demanding task that requires exceptional attention to detail. Also, any state should assume that it is likely to take several years before it can be certain that the following two goals are achieved:

1. The variables required for analysis are properly created.
2. The data coverage of the population is complete.

To reach goal 1, it is essential to directly compare the input and output files at each stage in the data preparation process. A key suggestion is to structure the data preparation work assuming that some problem will be spotted with each person's work longer after that person is not available for consultation and would have forgotten the details of work. In short, it is imperative to create documentation that would allow an independent review of each person's work, without any additional aid from that person.

To reach goal 2, it is essential to compare published statistics to analogous statistics derived from tabulations of the person-level files at the outset and all through the data preparation processing. We cannot explain why person-level data that once were used to produce aggregate statistics cannot easily be reassembled, but this has been our experience with every state where we have tried to perform this task. Also, we have found it to be difficult, if not impossible, to find missing data. However, once it becomes clear that the person-level files will be used for research, arrangements can be made to properly preserve current files.

Thus, one especially clear-cut conclusion is that the sooner a state starts to assemble person-level files for research purposes, the sooner problems with available data will be spotted, and the sooner those problems can be resolved.

With respect to the second question regarding the relative size of the effect, it appears that the benefits of direct placement services are smaller both per-incident and in total in Oregon than in Washington. Placements relative to obtaining a referral and staff reduce unemployment by 4.6 weeks in Oregon, compared to 6.6 weeks in Washington. The lower-bound estimate for the total benefits is $10 million in Oregon, compared to $12.9 million in Washington. Moreover, we estimate that both states spend about the same amount on their PLXs, but Oregon spends about 38 percent of the total PLX budget to aid claimants, compared to Washington's 25 percent.

In Oregon, all four estimates of benefits more than cover the costs of providing services to claimants. The largest estimate suggest that benefits going to claimants more than cover the costs of running the entire PLX.

In Washington, all four estimates far exceed the estimate of costs of providing services to claimants. The lowest estimate suggests benefits are twice the costs to serve claimants, and highest suggests benefits going to claimants are 75 percent greater than the cost of running the entire PLX.

Importantly, the estimates of the benefits of direct placement services in Oregon could be considerably lower than benefits in Washington because Oregon has implemented other policies that dramatically reduce the time claimants who are not directly helped by PLXs remain unemployed. Thus, because those benefits are outside of this study, the full value of public services provided to claimants is dramatically underestimated.

However, even if this is the case, Oregon might be able to dramatically increase benefits of direct placement services by improving their targeting. We suspect that the benefits in Washington are particularly high because many claimants voluntarily seek help usually long after most claimants have already become reemployed. In contrast, Oregon benefits might be relatively low because claimants are encouraged (or required) to obtain aid far closer to the start of the unemployment spell. Thus, additional analysis might suggest ways to reduce the amount of services received by claimants who are likely to return to work quickly on their own.

We hope to be able to continue our work with the OED to answer the highly provocative questions that are unresolved in this, our initial collaboration.

Chapter 6

6. CROWDING-OUT EFFECTS OF THE PUBLIC LABOR EXCHANGE IN WASHINGTON STATE[1]

6.1 Introduction

Improvements in public labor exchanges (PLXs) are intended to reduce the duration of unemployment for jobless workers by improving the information available to those workers and by increasing the rate at which they receive job offers. Improvements in the PLXs can be thought of as enhancements to the technology of job search that lead to increases in the rate at which jobless workers become reemployed. However, a potential unintended consequence of increasing the reemployment rate of one group of workers (in this case, users of the PLXs) is that the job prospects of other groups of workers may worsen. The reason is that improvements in the technology of job search for one group of workers may have the indirect effect of "crowding out" other groups of workers. For example, if the search technology available to PLXs registrants is better than the search technology available to other workers, then PLX registrants may beat other workers to job vacancies and fill those vacancies. As a result, job vacancies that would normally be available to other workers are no longer available, and the other workers don't receive job offers that they would otherwise receive. This crowding-out effect could offset part (or all) of the benefits of improving the services provided by the PLXs.

To investigate the crowding-out effects of the PLXs, we build on previous work on the crowding-out effects of reemployment bonus programs and wage subsidies (Davidson and Woodbury 1993, 1996). In particular, we develop and apply an equilibrium search and matching model of the labor market in which heterogeneous unemployed workers are assumed to search randomly across firms for a vacancy, and firms with vacancies randomly select workers from the pool of applications they receive. Each unemployed worker chooses a search effort (the number of firms to contact) in an effort to maximize expected lifetime utility. Increasing search efforts raises the probability of reemployment but is also costly. We characterize the steady-state equilibrium generated by the market in such an environment and then examine how that equilibrium would change in the absence of the referral and placement activities of PLXs. Comparing the two equilibria provides insight into the impacts of the PLXs on labor market outcomes.

[1] This chapter was written by Drs. Carl Davidson and Stephen A. Woodbury at Michigan State University.

In Section 6.2, we develop a general equilibrium model in which different groups of workers flow through different labor market states (employment and unemployment). The rates of transition between different labor market states depend, in part, on the search technology available to workers, and the model allows for interactions between different groups of workers. Section 6.3 describes how the model is calibrated using labor market data from Washington State and recent evidence on the impacts of PLXs in Washington State that have been produced by Westat, Inc. (Jacobson 1999). Calibrating the model grounds the model in observed behavioral effects and, as much as possible, forces the model to look like the actual labor markets that are of interest. We then solve the model without the PLXs and compare the two solutions to obtain simulated impacts of the PLXs on workers who do not use the PLXs.

Section 6.4 describes the results of the simulations. In the model, the referral and placement activities of the PLXs have two main effects. The first is a gross employment effect – that is, the PLXs reduce unemployment and increases employment of PLX users (workers who are referred or placed by the PLXs). The second is the crowding-out effect, which occurs if some (or all) of the improvement experienced by PLXs users comes at the expense of workers who do not use PLXs (workers who are not referred or placed by PLXs). The simulations suggest that PLXs increase the overall level of employment in Washington State and reduce the unemployment rate by a small amount (0.08 percentage points). The simulations also suggest a small crowding-out effect of PLXs: nonusers of PLXs experience unemployment spells that are longer by 0.061 week (or 0.4 percent) as a result of the referral and placement activities of PLXs. The net result of these two effects is that about one-quarter of the improvement in labor market outcomes for which PLXs are responsible is offset by crowding out of PLX nonusers. It follows that the crowding-out effects of PLX referral and placement activities are small in relation to the benefits.

6.2 Description of the Model

6.2.1 Overview

To examine the extent to which users of the PLXs might crowd out other workers, we employ a model of the labor market in which workers must search for reemployment after losing their jobs. The basic setup of the model is patterned after the labor market for workers covered by the

Unemployment Insurance (UI) program in Washington State during 1987 through 1995. In the model, there are three classes of workers:

- UI claimants who are neither referred nor placed by PLXs, although they may be registered with the PLXs (we refer to these as PLX nonusers);
- UI claimants who are referred to at least one job opening by PLXs but are not placed; and
- UI claimants who are referred and placed by PLXs.

In what follows, the subscript j can take three values: 0 always refers to the group of nonusers; the subscript 1 always refers to those referred but not placed; and the subscript 2 refers to those referred and placed by the service.

Note that workers who do not claim UI benefits are not considered in this model. In principle, they could be included. However, the data available for this study (see Jacobson 1999 and Table 6-1) are specific to UI claimants only. Although we focus on UI claimants in this chapter, we conjecture that including nonclaimants would not materially change the basic findings. We find that the referral and placement activities of PLXs result in little crowding out of PLX nonusers by PLX users. Adding nonclaimants to the model would diffuse the small crowding-out effects over more workers and would result in even smaller losses per PLX nonuser.

While unemployed, workers of type j (j = 0, 1, and 2) collect unemployment benefits (x_j) and search for reemployment with search effort (p_j) chosen to maximize expected lifetime income (VU_j). In each period, the worker's probability of reemployment (or match rate, m_j) depends on his or her own search effort (p_j), the search effort of all other unemployed workers, and the vacancy rate. Since the probability of reemployment is assumed constant throughout the spell of unemployment, the expected duration of unemployment for a worker of type j is ($1/m_j$) periods.

While employed, each worker of type j is paid a wage (w_j) that is negotiated at the time of initial employment. In addition, in each period, each employed worker faces a risk of losing his or her job; the probability of job separation is denoted by s. This implies that the expected duration of a job is (1/s) periods. Thus, workers cycle between employment and unemployment with each job lasting (on average) 1/s periods and each spell of unemployment lasting (on average) $1/m_j$ periods.

Firms post vacancies as long as the expected profit from doing so is positive. Each firm attempts to fill each of its vacancies separately. We use F to denote the total number of jobs available in the economy at any point in time (full employment), J to denote the number of these jobs that are filled, and V to denote the number that are vacant. In each period, there is a constant probability of filling a vacancy and a constant probability that any filled job will break up. Thus, job openings cycle between being filled or vacant.

There are three key assumptions of the model. First, workers search optimally for reemployment. Second, firms create vacancies up to the point where the last vacancy generates an expected profit of zero. Third, the wage is negotiated by the firm and the worker when the worker is hired. The impact of PLXs is captured by assuming that PLXs make it easier for users (groups 1 and 2) to find reemployment. The model is calibrated using labor market data from Washington State and recent estimates of the impact of PLXs obtained by Jacobson (1999). The crowding-out effects are approximated by solving the calibrated model to see how PLXs affect (a) expected duration of unemployment on nonusers and (b) the number of jobs held by nonusers.

6.2.2 Workers

We begin with a description of worker behavior. Since search effort is chosen to maximize expected lifetime income, we must first define the expected lifetime income for each employed worker of type j (VWj) and the expected lifetime income for each <u>unemployed</u> worker of type j (VU_j). These values are given by

$$VW_j = w_j + [sVU_j + (1\text{-}s)VW_j]/(1 + r) \text{ for } j = 0, 1, 2 \tag{6.1}$$

$$VU_j = x_j - c_j p_j^{\beta} + [m_j VW_j + (1-m_j)VU_j]/(1 + r) \text{ for } j = 0, 1, 2 \tag{6.2}$$

In equation 6.1, the expected lifetime income for a type j employed worker consists of two components. The first component is current income, which is given by the wage w_j. The second component is expected future income, which is a weighted average of what the worker earns if he or she loses a job (VU_j) and what the worker earns if he or she keeps a job (VW_j). Note that the weights are equal to the probability of each event occurring (s is the probability of losing a job) and that expected future income is discounted, with r representing the interest rate.

In equation 6.2, the expected lifetime income for a type j unemployed worker consists of two components. The first component is current income, which is given by unemployment benefits (x_j) less search costs ($c_j p_j^{\beta}$). [Recall that p_j denotes the worker's search effort. Additional search effort is assumed to reduce a worker's income at an increasing rate, so p_j is raised to the power β (>1), which is the elasticity of search cost with respect to search effort (search costs are convex in search effort). The term p_j^{β} is then scaled by the multiplicative factor c_j, which we refer to as the search cost parameter.]

The second component of expected lifetime income is expected future income, which is a weighted average of what the worker earns if he or she finds a job (VW_j) and what the worker earns if he or she remains unemployed (VU_j). Once again, the weights are equal to the probability of each event occurring (the probability of finding a job is m_j) and expected future income is discounted.

Each unemployed worker chooses search effort to maximize expected lifetime income. Thus, for each worker p_j is chosen to maximize equation 6.2; or

$$p_j = \arg\max VU_j \text{ for } j = 0, 1, 2 \qquad (6.3)$$

Equations 6.1, 6.2, and 6.3 define optimal search effort. Our next goal is to show how search effort determines the reemployment probabilities. For each worker, we interpret search effort as the number of firms contacted in an effort to find a job. Once the worker contacts a firm, the probability of getting a job depends on (a) whether the job is vacant and (b) if the job is vacant, being chosen over all other job applicants. Since there are V vacancies and F total job openings, the probability that any given opening is vacant at any point in time is V/F. In Davidson and Woodbury (1993), we showed that in this type of model the probability that any given worker will be chosen over all other applicants to fill a vacancy is given by $(1/\lambda)[1 - e^{-\lambda}]$ where λ is the total number of job applicants per job opening. Thus, equation 6.4 shows how search effort can be used to determine the reemployment probabilities

$$m_j = p_j(V/F)(1/\lambda)[1 - e^{-\lambda}] \text{ for } j = 0, 1, 2 \qquad (6.4)$$

where

$$\lambda = (p_0U_0 + p_1U_1 + p_2U_2)/F \qquad (6.5)$$

In equation 6.5, U_j is the number of type j workers who are unemployed in equilibrium, and F denotes the total number of job openings (filled and vacant).

6.2.3 Discussion

At this point, it is useful to summarize the model with the aid of Figure 6-1, which shows the stocks of each type of unemployed worker (U_0, U_1, and U_2) and their flows through the labor market and into jobs. In each period, we assume that unemployed workers choose search effort (p_j) to maximize expected lifetime income. The optimal level of search effort for each type j worker is defined by equations 6.1, 6.2, and 6.3. These workers then compete with each other for the jobs that are available in the labor market. The levels of search effort determine the number of applications filed at each firm, and each firm is assumed to choose randomly from the pool of applicants to fill vacancies. With these assumptions, equations 6.4 and 6.5 show how the levels of search effort can be translated into reemployment probabilities (m_j) for each type of worker. These reemployment probabilities then determine the number of type j job seekers who find new jobs each period.

6.2.4 Firms

We assume that firms create vacancies as long as the expected profit from doing so is non-negative. Let π_v denote the expected lifetime profit for a firm with a vacancy and let π_j denote the expected lifetime profit for a firm with a job opening that is filled by a type j worker. Furthermore, let q_j denote the per period probability of filling a vacancy with a type j worker and let R_j denote the per period revenue generated by a type j worker. Then π_v and π_j are given by

$$\pi_v = -K + [q_0\pi_0 + q_1\pi_1 + q_2\pi_2 + (1-q_0-q_1-q_2)\ \pi_v]/(1 + r) \tag{6.6}$$

In equation 6.6, K represents the cost of maintaining the job opening when it is vacant. Note that π_v consists of two components. The first component, -K, is the current period profit associated with having a vacancy. The second component measures expected future profit, which is the weighted average of the profit earned by filling the vacancy with a worker of each type. The weights are the probabilities of filling the vacancy with each type of worker, and this second component is discounted at the interest rate to take into account that it is earned in the future.

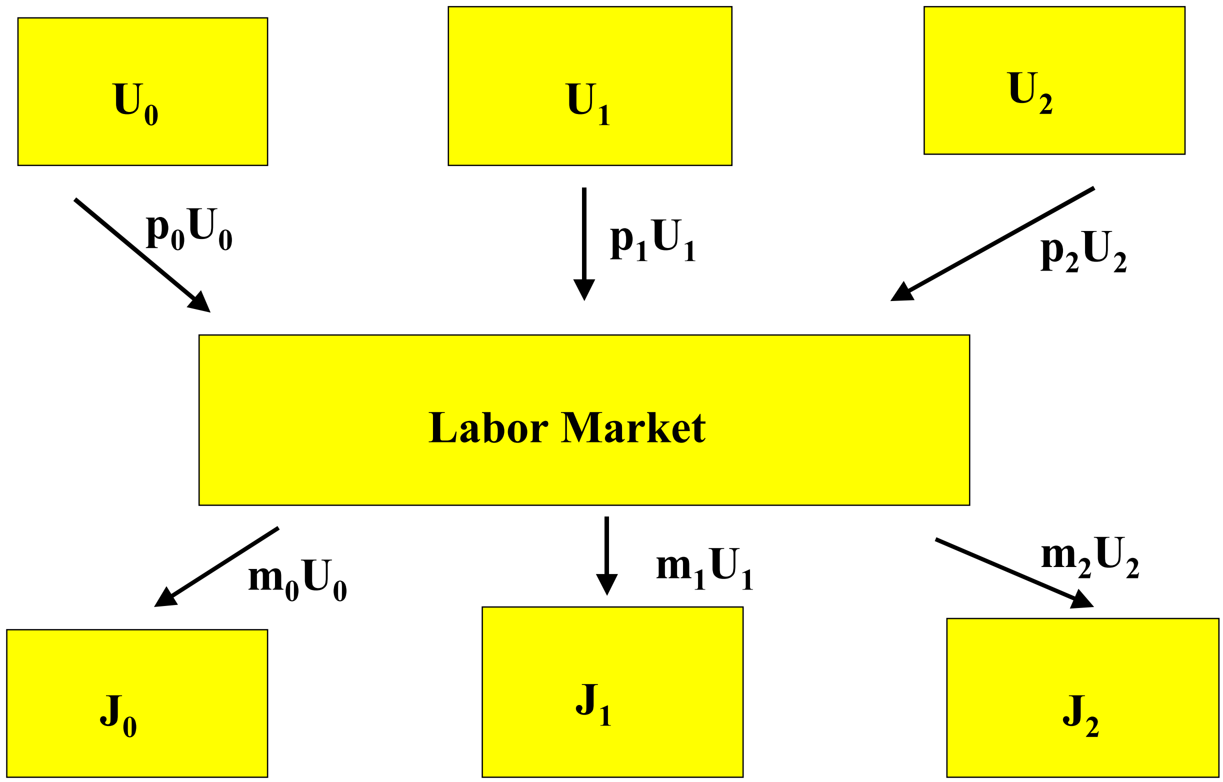

Figure 6-1. The labor market

The expected lifetime profit for a firm with a job opening filled with a type j worker (π_j) is calculated in a similar manner. The first term on the right-hand side of equation 6.7 captures current profit, which is revenue (R_j) net of the wage (w_j). The second term captures expected future profit, which is the weighted average of (a) what the firm can expect to earn in the future if the job lasts another period and (b) what the firm can expect to earn in the future if the job breaks up at the end of the current period.

$$\pi_j = R_j - w_j + [s\pi_v + (1\text{-}s)\,\pi_j]/(1 + r) \text{ for } j = 0, 1, 2 \qquad (6.7)$$

Since we have assumed that firms create vacancies until the expected profit earned by the firm creating the last vacancy is zero, we know that in equilibrium

$$\pi_v = 0 \qquad (6.8)$$

We complete our description of firm behavior by explaining how the equilibrium wages are determined for each type of worker. We assume that once the firm and worker meet, they negotiate the wage by making offers and counteroffers until an agreement is reached. As is well known by now, such negotiations lead to an outcome that is described by the Nash Cooperative Bargaining Solution, which splits the surplus generated by the job evenly between the firm and worker. For a worker of type j, the surplus created by the job is $VW_j – VU_j$ since this measures the increase in expected lifetime income from accepting the job. For the firm, the surplus created by the job is $\pi_j - \pi_v$ since this measures the increase in expected lifetime profits from filling the vacancy with a type j worker. Thus, if w_j splits the surplus generated by the job evenly, it must satisfy

$$\pi_j - \pi_v = VW_j - VU_j \text{ for } j = 0, 1, 2 \tag{6.9}$$

6.2.5 Equilibrium

We begin our description of equilibrium by introducing some accounting identities that allow us to keep track of employment, unemployment, and their composition. In each period, a worker must either be employed or unemployed. Thus, if we let L_j denote the total number of type j workers in the economy and use J_j and U_j to denote the number that are employed and unemployed, respectively, then we must have

$$L_j = J_j + U_j \text{ for } j = 0, 1, 2 \tag{6.10}$$

We use J to denote total employment and U to denote total unemployment. It follows that

$$J = J_0 + J_1 + J_2 \tag{6.11}$$

$$U = U_0 + U_1 + U_2 \tag{6.12}$$

Finally, each job opening must be either filled or vacant. Thus, we must also have

$$F = J + V \tag{6.13}$$

where F denotes the total number of job openings and V denotes the total number of vacancies.

In equilibrium, the flow into each state of employment must equal the flow out of that state. For example, in each period m_jU_j unemployed type j workers find new jobs. It follows that m_jU_j is the flow into state J_j. At the same time, sJ_j type j employed workers lose their jobs. Thus, sJ_j is the flow out of state J_j. In equilibrium, these flows must be equal so that employment of type j workers remains constant. Therefore, it is necessary that

$$m_j U_j = sJ_j \text{ for } j = 0, 1, 2 \tag{6.14}$$

The employment dynamics for each type of worker are depicted in Figure 6-2, which shows the flows of workers into and out of each labor market state. Equation 6.14 guarantees that these flows will be equal so that the employment and unemployment levels for each type of worker will not change over time in equilibrium.

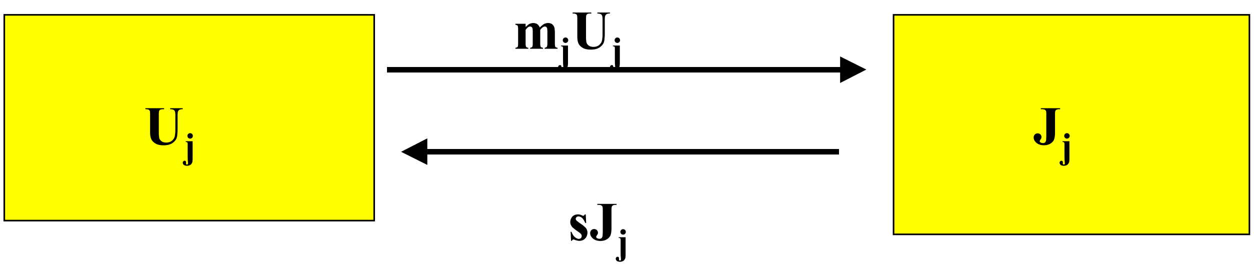

Figure 6-2. Employment dynamics for type j workers

A final equilibrium condition allows us to calculate the firm's probability of filling its vacancy with a type j worker (q_j). Since each job consists of one worker and one vacancy, the number of vacancies filled by a type j worker in each period (q_jV) must equal the total number of type j workers who find employment in that period (m_jU_j). Thus, we must have

$$q_j V = m_j U_j \text{ for } j = 0, 1, 2 \tag{6.15}$$

Equations 6.14 and 6.15 together imply that the number of firms and vacancies will remain constant over time.

This completes the description of the model. The key parameters of the model are s (the separation rate), r (the interest rate), c_j and β (the search cost parameter and the elasticity of search cost with respect to search effort), x_j (unemployment benefits), R_j (the revenue generated by a type j worker), K (the cost of maintaining each vacancy), and L_j (the number of type j workers in the economy). The endogenous variables are VW_j, VU_j, p_j, m_j, λ, w_j, π_v, π_j, F, J_j, U_j, J, U, V, and q_j. Note that the model consists of 33 equations in 33 unknowns. There are 16 parameters.

6.2.6 Why Use an Equilibrium Search and Matching Model?

Before explaining how the model can be used to estimate the displacement effect of PLXs, it is useful to discuss why we have chosen to work with such a model. If there were full employment, displacement would not be an issue—each worker who wanted to work would be able to find a job and there would be no concern about one worker displacing another. Of course, in an economy of full employment, programs such as the PLX would not be necessary because every worker would be able to find a job without government assistance.

However, full employment does not prevail, even in the current period of prosperity. Standard economic models have often been criticized for assuming full employment, and economists have worked hard to replace the standard textbook models of supply and demand with more realistic models of the labor of the labor market. In these models, the informational asymmetries and transaction costs (or trade frictions) that keep the market from clearing are taken into account so that there is an "equilibrium" rate of unemployment. The search model used in this report follows in this tradition. It emphasizes the time and effort needed for unemployed workers and firms with vacancies to find each other. These trade frictions result in an equilibrium rate of unemployment and provide some justification for government intervention aimed at either lowering unemployment or reducing the time it takes for unemployed workers to find new jobs. Accordingly, the search framework provides an ideal setting for investigating the impact of government programs such as PLXs.

Three additional features of the model make it particularly useful for estimating the crowding-out effects of PLXs. First, the key parameters of the search model are the turnover rates; that is, the separation rate and the reemployment probabilities. Research exists that suggests values of separation rate. The duration of unemployment, which is linked directly to the reemployment probability, can be observed directly. As a result, the model can be calibrated using existing data. (This is discussed further in the next section.) Once calibrated, the model looks remarkably similar to the actual labor market in Washington State.

Second, the key parameters of the model can be altered to simulate different types of labor markets. For example, when the separation rate is increased, the model's equilibrium rate of unemployment rises. Thus, by increasing the separation rate and examining the impact of this parameter change on crowding out, we can gauge the impact of a program such as the PLXs at different stages in the business cycle.

Finally, by modeling job search explicitly, we are able to show how PLXs directly affect the problem faced by unemployed workers. For those who use PLXs, the cost of contacting a given number of firms is lowered. In terms of the model, for users, PLXs lower c_j, the cost-of-search parameter. For nonusers, PLXs make it harder to find employment because λ, the average number of applications filed per firm, rises while the number of contacts they make does not change. Since users are contacting more firms, the reemployment probability for all nonusers falls. (In terms of the model, m_0 falls if p_0 remains constant while λ increases.)

6.3 Implementing the Model

Several of the key parameters of the model are observable. For example, we know that in Washington State there were roughly 282,000 UI claimants who were neither referred nor placed by PLXs during the period in question. In addition, roughly 34,000 UI claimants were referred but not placed by PLXs, and roughly 12,600 UI claimants were referred and placed by PLXs. Thus, we measure workers in thousands and set $L_0 = 282$, $L_1 = 34$ and $L_2 = 12.6$. We also have data on the unemployment benefits collected by these workers, the wages they earned, and their average durations of unemployment (DU_j). Measuring time in weeks, we have $w_0 = 365$, $w_1 = 304$, $w_2 = 252$, $x_0 = 188$, $x_1 = 181$, $x_2 = 158$, $DU_0 = 13.9$, $DU_1 = 17.9$ and $DU_2 = 12.3$. These data are summarized in Table 6-1. Although the wages and expected durations of unemployment are not parameters of the model, we use these values to infer some of the underlying parameters that cannot be observed.

For the separation rate, we turn to the literature on labor market turnover. Estimates of this parameter can be found in Clark and Summers (1982), Ehrenberg (1980), and Murphy and Topel (1987). We set $s = .005$ in our reference case and then vary s to make sure that our results are not sensitive to this assumption. For the interest rate, we set $r = .004$. We know from previous work with models similar to this one that the results are largely insensitive to changes in the assumed value of r.

Table 6-1. Descriptive Statistics of Public Labor Exchange (PLXs) Registrants, and Estimated Impacts of the PLXs, Washington State, 1987-1995

	All	Nonusers	Referred/ not placed	Referred/ placed
Weekly wage before unemployment ($)	355	365	304	252
Weekly UI benefit amount ($)	186	188	181	158
UI replacement rate	0.52	0.52	0.60	0.63
Spell duration (weeks)				
Insured unemployment	14.3	13.9	17.9	12.3
Total unemployment	20.6	20.7	23.2	13.1
PLXs impact (weeks)				
Insured unemployment	--	--	-0.56	-4.25
Total unemployment	--	--	-2.43	-12.59
Number of individuals	328,815	282,240	33,990	12,585

Source: Jacobson (1999).
Note: PLXs nonusers are PLXs registrants who are neither referred nor placed by PLXs.

The search cost parameters are the next consideration. In Davidson and Woodbury (1996), we calibrated a variation of this model for Washington State in an effort to explain the results of the Washington Reemployment Bonus experiment, which was conducted during 1988-1989 (Spiegelman, O'Leary, and Kline 1992). We found that in order to explain the effects that were observed in the bonus experiment, β would have to be between 1.5 and 1.7. Thus for the reference case we set $\beta = 1.6$, but we solve the model and obtain estimates of crowding out for $\beta = 1.5$ and 1.7 as well.

Finally, solving the model requires values of c_j, R_j, and K. To obtain estimates of these values, we use the data on wages and average durations of unemployment mentioned above. We also add one other equation to the model. From Abraham (1983), we know that the number of job vacancies per unemployed workers (V/U) is typically close to 1/2 (that is, there is usually one job vacancy for every 2 unemployed workers), although that ratio varies between 1/3 and 1 over the business cycle. If we add this equation to the model and set w_j and DU_j equal to the values above, we can solve the model for the missing parameters (c_j, R_j and K). This gives us values of the parameters that make the model consistent with Jacobson's (1999) estimates of the impact of PLXs in Washington State.

Once the model is calibrated, we are in position to estimate the crowding-out effects of Employment Service referrals and placements. Jacobson (1999) estimates that a PLX referral reduces the average duration of unemployment by .56 weeks and that a referral leading to a placement reduces the average duration of unemployment by 4.25 weeks. In the model, the activities of PLXs (that is, referrals and placements) change the search cost parameters (c_1 and c_2) by making it easier (or less costly) for workers who receive a referral to make job contacts. Thus, we re-solve the model for the new values of c_1 and c_2 that make the model's results consistent with Jacobson's estimated impacts of PLX referrals and placements in Washington State. The model's solution yields estimates of J_0 and m_0 (the number of jobs and the probability of reemployment for PLX nonusers) that allow us to measure the impact of PLX referrals and placements on the employment level and average unemployment duration of workers who do not receive PLX referrals (the nonusers).

To summarize, we start with a model that has 33 equations in 33 unknowns and 16 parameters. We obtain estimates of the parameters needed to implement the model from various sources: the separation rate and the interest rate come from the published literature; unemployment benefits come from Jacobson's study of PLXs in Washington State (Jacobson 1999); and the elasticity of search costs with respect to search effort (β) is taken from our previous work (Davidson and Woodbury 1993, 1996).

However, several parameters cannot be observed: the search cost parameters (c_0, c_1, and c_2); the revenue generated by each type of worker (R_0, R_2, and R_2); and the cost of maintaining a vacancy (K). We use data from Jacobson (1999) to infer values for these missing parameters. We do so in two steps. First, we take values for six of the endogenous variables (wages - w_0, w_1, w_2 - and the average duration of unemployment - DU_0, DU_1, DU_2) from Jacobson (1999) and take an estimate of U/V from the literature (Abraham 1983). Adding these seven values to the model allows us to solve for the seven missing parameters and gives us a model that is then consistent with the data and findings in Jacobson (1999).

The second step is to infer how the program alters the cost of search for each group of users. We do this by using Jacobson's estimate of the impact of PLXs on the average unemployment durations of PLXs users. Accordingly, we re-solve the model with all of the parameters held fixed (including the values for c_0, R_0, R_2, R_2 and K that were solved for in stage 1). This yields values of c_1 and c_2 that would lead to the reductions in DU_1 and DU_2 that were observed in Jacobson's study. In other words, we model the program by assuming that PLXs cause c_1 and c_2 to fall, and then we check to see how much they must fall to completely explain the observed outcome.

6.4 Results

In the model described in the previous section, the referral and placement activities of PLXs have two main effects. First, they improve the labor market outcomes of PLX users, reducing their unemployment durations and increasing their employment. This can be thought of as a gross employment effect of PLXs. Second, some (or possibly all) of the improvement experienced by PLX users may come at the expense of workers who do not use PLXs. This is the crowding-out effect that motivates this report. The simulations address the following questions. How large is the displacement effect in relation to the gross employment effect of PLXs? Does the gross employment effect outweigh the displacement effect or, alternatively, is there a one-for-one trade-off between improvements for PLX users and crowding out of nonusers? Do PLX referral and placement activities lead to an increase in overall steady-state employment (and a decrease in overall unemployment)?

Tables 6-2 and 6-3 display results of the simulations. Table 6-2 shows the results of nine simulations using three different values of the separation rate (s = .004, .005, and .006) and three different values of the elasticity of search cost with respect to search effort (β = 1.5, 1.6, and 1.7). Table 6-3 shows the results of another nine simulations using the same three values of the separation rate (s = .004, .005, and .006) but this time varying the value of the number of vacancies to unemployed workers (V/U = 1/3, 1/2, and 1). Varying the ratio of vacancies to unemployed workers is a way to checking how the crowding-out effects of the PLXs might vary over the business cycle.

6.4.1 Reference Case

Consider first the reference case, which is in the middle column of Table 6-2. The reference case uses parameter values that we believe to be the most likely to obtain in fact: a separation rate (or weekly probability of separating from a job) of .005, an elasticity of search cost with respect to search effort (β) of 1.6, and two unemployed workers for every job vacancy (V/U = 1/2).

Table 6-2. Simulated Impacts of PLXs on Unemployment Duration, Employment, and the Total Unemployment Rate, Washington State, for Various Separation rates (s) and Elasticities of Search Effort (β)

Separation rate (s):	0.004			0.005			0.006		
Elasticity of search effort (β):	1.5	1.6	1.7	1.5	1.6	1.7	1.5	1.6	1.7
Change in unemployment duration (in weeks) of workers:									
who do not use PLXs	.141	.059	.073	.003	.061	.062	.061	.061	.061
referred by PLXs	-0.56	-0.56	-0.56	-0.56	-0.56	-0.56	-0.56	-0.56	-0.56
placed by PLXs	-4.25	-4.25	-4.25	-4.25	-4.25	-4.25	-4.25	-4.25	-4.25
Change in employment of workers:									
who do not use PLXs	-147	-61	-76	-3	-76	-76	-88	-88	-88
referred by PLXs	66	66	66	80	80	80	93	93	93
placed by PLXs	191	191	191	233	233	233	272	272	272
Change in total employment	110	196	181	310	237	237	277	277	277
Change in total Unemployment rate	-.04	-.06	-.06	-.09	-.07	-.07	-.09	-.09	-.09

Notes: Vacancies per unemployed worker (V/U) are set to 1/2 in all the above simulations. Other parameters of the model are set to the values listed in the text. PLXs nonusers are PLXs registrants who are neither referred nor placed by PLXs. See the results section for discussion.

Table 6-3. Simulated Impacts of PLXs on Unemployment Duration, Employment, and the Total Unemployment Rate, Washington State for Various Separation Rates (s) and Vacancies per Unemployed Worker (V/U)

Separation rate (s):	0.004			0.005			0.006		
Vacancies per unemployed worker (V/U):	1/3	1/2	1	1/3	1/2	1	1/3	1/2	1
Change in unemployment duration (in weeks) of workers:									
who do not use PLXs	.064	0.59	.059	.063	.061	.181	.127	.061	.058
referred by PLXs	-0.56	-0.56	-0.56	-0.56	-0.56	-0.56	-0.56	-0.56	-0.56
placed by PLXs	-4.25	-4.25	-4.25	-4.25	-4.25	-4.25	-4.25	-4.25	-4.25
Change in employment of workers:									
who do not use PLXs	-64	-61	-60	-78	-76	-224	-182	-88	-84
referred by PLXs	66	66	66	80	80	80	93	93	93
placed by PLXs	191	191	191	233	233	233	272	272	272
Change in total employment	193	196	197	235	237	89	182	277	281
Change in total unemployment rate	-.06	-.06	-.06	-.07	-.08	-.03	-.06	-.09	-.09

Notes: The elasticity of search cost (β) is set to 1.6 in all the above simulations. Other parameters of the model are set to the values listed in the text. PLXs nonusers are PLXs registrants who are neither referred nor placed by PLXs. See the results section for discussion.

In the reference case, the referral and placement activities of PLXs increase the expected unemployment duration of UI claimants who do not use PLXs by .061 week. (See the row of Table 6-3 labeled “Change in unemployment duration of workers who do not use PLXs.”) This is clearly a small increase. It implies that the average UI claimant who does not use a PLX loses about 2.5 hours of work (and the wages associated with those hours of work), as a result of the referral and placement activities of the PLX. (This calculation is based on a 40-hour work week: 40 hours/week x .061 week = 2.44 hours.) Also, it implies that the average UI claimant who does not use a PLX experiences a 0.4 percent increase in the average duration of his or her unemployment spell. (This calculation is based on an average unemployment duration of 13.9 weeks for a UI claimant who did not use a PLX: 13.9 weeks x 0.61 = .0044.)

The next two rows (“Change in the unemployment duration of workers referred by PLXs and placed by PLXs”) indicate that UI claimants referred to a job opening (but not placed) see their spell of unemployment shortened by .56 week and that UI claimants referred and placed in a job see their spell of unemployment shortened by 4.25 weeks. These do not change from simulation to simulation; they come from Jacobson’s recent estimates of the impact of PLX activities.

The referral and placement activities of the PLXs imply a reduction in employment of PLX nonusers (the crowding-out effect) and an increase in employment of UI claimants who use the PLXs (the gross employment effect). These changes are shown in the columns labeled “Change in employment of workers who do not use PLXs, referred by PLXs, and placed by PLXs.” UI claimants who do not use PLXs experience an employment loss of 76 workers. UI claimants who are referred by PLXs but not placed experience an employment increase of 80 workers. UI claimants who are referred and placed by PLXs experience an employment increase of 233 workers. Altogether, these changes imply that PLX activities increase overall employment by 237. (Note that this equals the sum of the increases in employment of PLX users minus the reduction in employment of nonusers: 80 + 233 - 76 = 237.)

The above result is central because it implies that the gross employment effect of PLX activities greatly outweighs the crowding-out effect. This occurs because PLX activities represent an improvement in the technology of search that causes more job vacancies to be filled at any given time than would otherwise be the case. Because of PLXs, the labor market operates closer to full employment than otherwise.

In the reference case, the crowding-out effect is less than one-quarter of the gross employment effect [76 / (80 + 233) = 0.24]. Because the gross employment effect outweighs the crowding-out effect, the total unemployment rate falls as a result of PLX referral and placement activities. This can be seen in the bottom row of Table 6-2. In the reference case, the unemployment rate falls by 0.08 percentage points. Although small, this small change reemphasizes that the crowding-out effect is small in relation to the increase in total employment for which PLX activity is responsible.

6.4.2 Variation in the Separation Rate (s) and Ratio of Vacancies to Unemployed (V/U)

The discussion so far has focused solely on the reference case. If some of the key parameters differed from those we have used in the reference case, would the results differ substantially? The various columns shown in Tables 6-2 and 6-3 address this issue. They suggest that, although the results can change with variations in the separation rate (s), the elasticity of search cost with respect to search effort (β), and the ratio of vacancies to unemployed workers (V/U), the implied changes do not alter our basic conclusion that the referral and placement activities of PLXs have relatively small crowding-out effects.

First, consider differences in crowding out that are observed with variations in the separation rate (s). The results in Tables 6-2 and 6-3 suggest that the relationship between crowding out and s depends on both the elasticity of search cost with respect to search effort (β) and on the ratio of vacancies to unemployment (V/U). When β is set to 1.5, the relationship between crowding out and s is nonmonotonic: crowding out falls as s increases from .004 to .005, but increases as s increases from .005 to .006. When β is set to 1.6, crowding out increases with increases in s. When β is set to 1.7, crowding out is essentially invariant to changes in s. (See Table 6-2 for these results.)

When V/U is set to 1/3 or 1/2, crowding out tends to increase with increases in s. However, when V/U is set to 1, the relationship between crowding out and s is nonmonotonic: crowding out increases dramatically as s increases from .004 to .005, but decreases as s increases from .005 to .006. (See Table 6-3 for these results.)

It seems clear that the relationship between crowding out and the rate at which jobs break up (s) depends on other features of the labor market. In general, however, the impact of PLX referrals and placements varies with the separation rate (s) in a nonmonotonic manner. The reason is that an increase in s has two opposing effects. On one hand, when s rises, the unemployment rate increases. With more unemployment, it is harder to find a job since there are more unemployed workers competing for fewer jobs. It follows that the PLX should have a greater impact when s (which moves up and down with the unemployment rate) is higher. On the other hand, when s is higher, jobs do not last as long, and as a result, more jobs open up in each period. With more jobs opening up, the PLXs may have a smaller effect. The conclusion is that an increase in s (or the unemployment rate) may either increase of decrease the effects of the PLXs (and, in turn, the crowding-out effects of the PLXs). The outcome depends on which of the two opposing forces described dominates.

Next, consider differences in crowding out that occur with variations in the elasticity of search cost with respect to search effort (β). Again, the relationship depends on the value of another parameter, in this case s. When s is set to .004, the relationship between crowding out and β is nonmonotonic: crowding out falls as β increases from 1.5 to 1.6, but increases as s increases from .005 to .006. When s is set to .005 or .006, crowding out tends to increase as β increases. (See Table 6-2 for these results.)

Finally, consider differences in crowding out that occur with variations in the ratio of vacancies to search effort (V/U). Once again, the relationship depends on the value of s. When s is set to .004 or .005, crowding out tends to increase as V/U increases. But when s is set to .006, crowding out increases as V/U increases from 1/3 to 1/2, then falls as V/U increases from .005 to .006. (See Table 6-3 for these results.)

Although the above results seem somewhat arbitrary, the extreme cases do make sense. For example, consider the case in Table 6-2 where s = .004 and β = 1.5. In this case, crowding out is relatively high: crowding out increases the unemployment duration of PLX nonusers by .141 week (about 3/4 of a day) and reduces the employment of PLX nonusers by 147 workers. These crowding-out effects are about twice those in the reference case and can be explained as follows. When β is relatively low (as in this case), additional search effort is less costly, and workers respond more strongly to the assistance that is provided by PLXs. When s is relatively low (as again is the case here), turnover in the labor market is lower, and vacancies appear less frequently. Hence, when a PLX user gets to a vacancy (and becomes reemployed) before a nonuser, the loss of the vacancy is more costly to a PLX nonuser. It makes sense, then, that this case should be one in which crowding out is relatively high.

Despite the variation in crowding out that is evident in Tables 6-2 and 6-3, it is clear that in every case shown, the gross employment effect of PLXs substantially outweighs the crowding-out effect.

In the reference case and 12 other cases, the ratio of the crowding-out effect to the gross employment effect is very close to one-quarter. [For example, in the reference case, 76 / (80 + 233) = 0.24.] This implies that employment of PLX users increases by 4 for every PLX nonuser who is crowded out. In three cases, the ratio of the crowding-out effect to the gross employment effect is one-half or larger, but it is never greater than 0.72 (in the case where s = .005 and V/U = 1 in Table 6-3.) In one case, the ratio is only .01, implying that employment of PLXs users increases by 100 for every PLX nonuser who is crowded out.

6.5 Summary and Discussion

The results displayed in Tables 6-2 and 6-3 suggest strongly that the crowding-out effects of PLX referral and placement activities are small both absolutely and relative to the increases in employment that result from PLXs activities.

Twelve of the 17 simulations shown in Tables 6-2 and 6-3 give results that are very close to those in the reference case. (There are only 17 simulations shown in total because the reference case shown twice, in the middle column in both Tables 6-2 and 6-3.) Only three simulations suggest larger crowding-out effects than the reference case, and one suggests smaller crowding-out effects.

Even in the three cases shown in Tables 6-2 and 6-3 where crowding out is larger than in the reference case, it would be difficult to argue that the crowding-out effects of PLX referral and placement activities are large. Consider the simulation in which crowding out is greatest: In Table 6-3, when s = .005 and V/U = 1, the unemployment duration of PLX nonusers increases by .181 week (slightly under 1 day) and the employment of PLX users falls by 224 workers. The implication is that the average UI claimant who does not use the PLX loses nearly a day of work and experiences a 1.3 percent increase in the duration of his or her unemployment spell as a result of the referral and placement activities of PLXs. In this case, it is still true that the increase in employment of PLX users exceeds the crowding out of nonusers (employment of PLX users increases by roughly 1.3 for each PLX nonuser who is crowded out).

The crowding out of UI claimants who do not use PLX referral and placement services needs to be weighed against the gross employment effect of PLXs. The gross employment effect has two kinds of benefits. First are the benefits to users of PLXs, who reduce the duration of their unemployment spells and increase their employment. Second are the overall economic improvements that result from PLX activities, as evidenced by the increase in total employment and the reduction in the unemployment rate shown in Tables 6-2 and 6-3. These benefits accrue to employers as well as to workers, since employers are able to operate closer to full employment and expand output.

We conclude that, although the activities of PLXs do lead to some crowding out of workers who do not use PLXs, those crowding-out effects are quite small both absolutely and relative to the gains of PLXs users. The reason that the crowding-out effect is so small seems clear: as can be seen in Table 6-1, the number of UI claimants who use PLXs by getting a referral is small in relation to the total number of claimants. Over the 1987-1995 period, about 10 percent of all UI claimants (33,990 out of 328,815) received a PLX referral that did not lead to a job placement, and another 4 percent (12,858 out of 328,815) received a referral that did lead to a job placement. Accordingly, although significant proportions of the UI claimant population did make use of PLXs, it appears that these proportions are not large enough to result in substantial crowding out of UI claimants who choose not to make use of PLXs.

If use of PLXs were to expand, it seems likely that its crowding-out effects would be greater. If so, a judgment might need to be made as to whether 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 labor market that result from PLX activities (that is, an increase in total employment, a reduced unemployment rate, and greater output). The costs are the crowding-out effects (that is, longer unemployment spells for PLX nonusers) that are the main concern of this report. Given that PLX users tend to be workers whose wages are relatively low and whose unemployment spells are relatively long, the crowding out of PLX nonusers by PLX users (if it were truly significant) could well be considered a justifiable transfer of resources from workers who are relatively well off to workers who are worse off. However, at the existing level of usage, the crowding-out impact of PLX activities is quite small and seems unlikely to pose a serious policy problem.

Chapter 7

7. SUMMARY AND CONCLUSIONS

This report draws upon four separate studies to address two key questions:

1. What are the benefits of public labor exchange (PLX) direct placement services given to job seekers, and how do those benefits compare to the costs of providing the services?

2. How can we best apply the lessons from our research to provide meaningful ongoing feedback about the value of PLX services to monitor and improve performance?

In this chapter we summarize our discussion of the conceptual framework for the analyses of the above questions, what we have learned about how to produce accurate measures, and the results of the four studies. We do not focus on the details of the analysis. The executive summary provides on overview of those details and the main body of the report presents detailed descriptions of what we did and why we did it.

7.1 Study 1: Information from the Washington State Mail Survey

To address question 1, we conducted a study using a mail survey that was able to identify a group of job seekers who viewed listings but were unable to secure interviews to jobs of interest. To the extent the interviews were not secured because the jobs (or interview slots) were already filled, this group resembled a control group from a random-assignment (experimental) design where employers were asked to randomly tell job seekers that they could not secure interviews.

More specifically, our goal was to use as a comparison group those job seekers referred to "stale" listings—listings where the jobs or interview slots were already filled. While not all job seekers we identified as having tried but failed to obtain interviews were referred to stale listings, information obtained from PLX staff suggest that this was the case for the vast majority of those job seekers. Staff universally agreed that lags in removing listings are unavoidable and commonly lead to referrals being made after employers have filled the job or interview slots. Thus, with some refinements, surveys have the potential to identify a natural experiment that experts agree can provide unbiased estimates of placement effects.

Our pilot use of the mail survey in Washington State also provided additional information that was unavailable from administrative data but crucial for our analysis. The single most important piece of information was the duration of job search from the time referrals were made until the referred individuals were placed by PLX referrals, found jobs on their own, or stopped looking. This is the dependent variable we used in our analysis, and is difficult to measure accurately with administrative data for job seekers other than unemployment insurance (UI) claimants.

In addition, knowing when job search begins and ends is crucial to holding constant "elapsed duration"—how long job seekers were unemployed when they were referred and matching them to placed job seekers who were unemployed for a comparable period. Elapsed duration is a key control variable in our nonexperimental estimates, second in importance only to knowing if the job seeker returned to a former employer.

The three central findings derived from the mail survey are:

1. One-third of all referred job seekers in our sample were unable to secure interviews for jobs to which they were referred.

2. Placed job seekers with strong work records in the survey sample returned to work 7.2 weeks sooner than they would have had they viewed listings and received a referral but were unable to secure interviews.

3. Placed job seekers with spotty work records in the survey sample returned to work 3.4 weeks sooner than they would have had they viewed listings and received a referral but were unable to secure interviews.

The first finding, coupled with staff information about the prevalence of lags in removing listings, suggests that there were plenty of PLX referred job seekers who were highly similar to control group members generated from a random-assignment design. The second and third findings provide the best evidence to date about the per-incident effect of PLX placements. However, a larger and more representative survey would be needed to provide definitive estimates of placement effects for the universe of PLX users.

Thus, at this point, we concluded that the mail survey demonstrates a way to obtain accurate estimates of the benefits stemming from being placed as a result of obtaining a PLX referral relative to obtaining a referral, but not being placed. However, this conclusion was far from obvious at the outset because we did not know that the incidence of inability to secure an interview would be sufficient to estimate the value of placements.

As a result of this uncertainty we did not use telephone followup to secure a large, representative sample. Instead, we used several low cost techniques to increase the mail response rate. These efforts only had limited success. Thus, we would recommend that telephone followup be used in future surveys. This is likely to cost about $30 per completed survey, and about 6,000 completes would be needed for a highly accurate analysis.

Use of telephone followup could overcome two major limitations and two minor limitations of the pilot study reported here. The major limitations are having a small sample and a sample that may not be representative of the universe. The two minor limitations are not knowing if a few employers refused to grant interviews because they felt the job seekers were not qualified, and not knowing if job seekers were able to secure interviews to other listings after the initial failure to obtain an interview.

We see no way to overcome one major limitation in use of the mail survey. That is, the survey cannot identify job seekers who closely resemble members of the control group needed to measure the value of information obtained about the job market in the course of being referred by viewing listings and talking to PLX staff.

If we were to use a random-assignment design, we would randomly deny access to PLX job banks to some of the job seekers who sought this service. This could be done if PLXs were run like health clubs where you needed a membership card to enter offices and needed a special password to view computerized listings at other sites. Thus, some job seekers wanting PLX help could be told that there simply are no new memberships available. In reality, however, it is not possible to oversubscribe PLXs because they are required to provide universal access and have sufficient equipment to accommodate huge numbers of job seekers.

In the absence of experimental evidence, we used the nonexperimental approach discussed in the next two sections to estimate a range of plausible values for the effect of obtaining information about the job market that could help job seekers find and accept jobs more quickly with or without being placed by a PLX referral.

7.2 Study 2: Information from Washington State Administrative Data

Study 2 used an exceptionally large sample of administrative data to examine the effect of (1) placements (relative to securing referrals) and (2) referrals not leading to placements (relative to not being referred and usually not obtaining any PLX services). The first comparison measures the same PLX effect as was measured with the mail survey, but for a different period. The second comparison provides an estimate of the value of the information acquired in the course of being referred.

The accuracy of these nonexperimental measures depends on adequately capturing the effect of factors that influence job search outcomes that are distributed differently between the targets (service receivers) and their comparison groups. To measure the placement effects, the targets are those placed and the comparisons those referred but not placed. To measure the information effect of referrals, the targets are those referred but not placed and the comparisons those not referred.

Because administrative data provide sufficient detail about key factors for UI claimants alone, we confined Study 2 to that one group. In particular, administrative data provide excellent measures of the time it takes claimants to find new jobs and stop collecting benefits, which are the dependent variables in the analysis. Also we can measure how long claimants were unemployed at the point they receive referrals and find comparisons who were unemployed for the same period. This is a key control variable.

Taking elapsed duration into account is extremely important because claimants who are able to find work quickly on their own tend to have job search resources that PLX clients lack and other characteristics that affect reemployment prospects that differ from PLX users, but are close to impossible to measure directly. Thus, by confining the comparisons to claimants who receive PLX services versus other claimants <u>unemployed for a similar period at the point services were delivered</u> we implicitly hold constant many factors that affect the remaining job search durations that are extremely difficult to measure directly.

To take advantage of this important insight, we separately estimated the value of referrals and placements made during five periods: week 1, weeks 2-9, weeks 10-13, weeks 14-18, and weeks 19-26. Once we took this step, we determined that the difference in the duration of unemployment (measured using the simple means versus adjusted values using regressions) between claimants who are referred versus those who are not referred become more and more similar as their duration of unemployment lengthens. Moreover, separate comparisons among job changers versus recalled claimants showed similarly small differences between simple means and regression adjusted values for unemployment durations.

The increasing similarity between targets and comparisons suggests that by making five separate estimates and controlling for recall we were able to substantially reduce, if not completely eliminate, self-selection bias. It is this bias that makes it so difficult to estimate the value of program services in the absence of a random assignment design. Importantly, using referred but not placed claimants as the comparison group in estimating placement effects also limits self-selection bias because both targets and comparisons have chosen to use PLXs.

The three key findings derived from use of the Washington administrative data were that:

1. Placed claimants returned to work 7.7 weeks sooner than they would have had they viewed listings and received a referral but were not placed.
2. Referred but not placed claimants returned to work 2.1 weeks sooner than they would have had the not received a referral.
3. The reduction in the duration of unemployment due to placements and referrals is about two and a half times greater than the reduction in the number of UI payments.

Until we improve the survey-based estimates, we cannot be sure that we have unbiased estimates to serve as an appropriate benchmark for these nonexperimental results. However, we feel that the study 1 estimates are close enough to the true values to note that the difference between the above placement effect estimates and the mail survey results could easily be due to differences in economic conditions during the periods covered. The administrative data estimates cover 1987-95, which was heavily affected by recessions in the early 1990s. In contrast, the mail survey covered the first half of 1998, which was a much more prosperous period.

Because our research suggests that it is possible to create an appropriate benchmark for estimates of the placement effect, the central problem with the use of the administrative data is that we lack experimental evidence to serve as a benchmark for assessing the accuracy of the referral effect estimates. In this report we produce a range of estimates based on three assumptions: (1) the 2.1-week estimate is free of measurement bias, (2) the true effect is 1.0 weeks because about half of the estimated effect is due to measurement bias, and (3) the effect is 0 weeks because all of the 2.1-week estimate is due to measurement bias.

In the future we plan to thoroughly assess how our 2.1-week estimate compares with experimental evidence on the effect of job search assistance directed at claimants. A quick review suggests that the treatments studied with random-assignment designs have only about half of the effect we measure. However, it is possible that those treatments were mandatory, rather than voluntary, and delivered over a short period close to the start of UI spells. In contrast, PLX referrals often occurred over a long period and started when claimants were especially anxious to return to work. We, therefore, believe that the experimental studies examined treatments that are likely to be considerably less potent obtaining PLX referrals. We also plan to test an instrumental variable approach based on obtaining information about the difficulty in getting to local offices to view listings.

7.3 Study 3: Information from Oregon Administrative Data

Study 3 also used an exceptionally large sample of administrative data to examine the effect on claimants of placements (relative to securing referrals) and referrals not leading to placements (relative to not being referred and usually not obtaining any PLX services). However, this analysis was confined to a single year, 1995, and all the data processing was performed by the Oregon Employment Department (OED) to our specifications. In contrast, Westat used its own computers and staff to assemble and analyze the Washington data.

The two key analytic findings derived from use of the Oregon administrative data were that:

1. Placed claimants returned to work 4.6 weeks sooner than they would have had they viewed listings and received a referral but had not been placed.

2. Referred but not placed claimants returned to work 1.1 weeks sooner than they would have had they not received a referral.

The Oregon results were produced using estimation procedures that were nearly identical to those used to produce the Washington results. Thus, the considerably smaller effects observed in Oregon can only be due to differences in the characteristics of claimants, their labor markets, and procedures used by PLXs. We have not reached definitive conclusions about why the results differ for the two states, but our leading hypotheses are that much of the difference stems from (a) Oregon referring and placing more claimants than Washington despite having only 60 percent of Washington's job vacancies and (b) Oregon subjecting claimants to more stringent work-search requirements.

In order to refer and place more claimants it is necessary for PLXs to have more listings and to reach more claimants before they become reemployed. One key difference between the two states is that Oregon referred and placed more claimants within the first 10 weeks of their unemployment spell. However, the effect of referrals and placement is considerably greater when made after claimants have been unemployed for 10 weeks. This is because PLX aid delivered to claimants unemployed for 10 weeks or more goes to job seekers who are unlikely to be recalled and unlikely to find jobs on their own for a considerable period, while claimants unemployed less than 10 weeks are much more likely to end spells of unemployment quickly without PLX aid either from being recalled or finding a new job on their own.

Perhaps of even greater importance, Oregon may impose conditions for continued benefit collection that substantially reduce the duration of unemployment of claimants in the comparison groups. For example, almost all claimants could be required to register with PLXs in person, and could be encouraged to look at PLX job listings. Those who fail to find promising listings may be further questioned by staff about their job search strategies. Also, many other PLX services may be provided to claimants. As a result, Oregon claimants may search for work harder and more effectively on their own or simply stop collecting benefits.

In short, it easily could be the case that the referral and placement effects are smaller in Oregon than in Washington because the value of PLX services and procedures <u>not included</u> in the study is high. Additional data, including use of large surveys for benchmarking, should be collected and analyzed for several states to test the above hypotheses. This evidence could be exceptionally valuable because it could suggest ways to dramatically increase the value of PLX services in each state.

7.4 Estimation of Benefit-Cost Ratios

The central analytic goal of this research was to estimate the <u>per-incident</u> effects of referrals and placements, which were just described. These estimates are an essential component in comparing total benefits to total costs. Program evaluators agree that the best single indicator of program effectiveness is a demonstration that the benefits outstrip the costs.

We estimated total benefits by multiplying the per-incident estimates of reductions in weeks of unemployment times the number of incidents, and then multiplying that product by average post-unemployment weekly earnings. For all three studies, we presented total benefits estimates including only the value of placements relative to being referred but not placed. For the two claimant studies using administrative data, we also produced two additional total benefits estimates. One included all of the estimated value of information obtained in the course of being referred; the second, half of the estimated value of the information.

Post-unemployment weekly earnings were estimated by dividing average earnings in the first full quarter following the return to work by 13. Measures of the incidence of referrals and placements were a natural outgrowth of use of both the Washington and Oregon administrative files. However, a key conclusion of our analysis is that the administrative data tend to be incomplete for the first few years following the start of its use for in-depth analysis. Thus, we used published statistics to (1) determine whether the person-level files covered the relevant universe, and (2) estimate referrals and placements when person-level data were unavailable or inadequate.

Total cost estimates are derived from PLX budgets that in both Washington and Oregon include Federal Wagner-Peyser allocations, Federal veteran program allocations, and state appropriations to assist claimants. Determining how much of the total costs went to providing direct placement services to the populations for which we estimated total benefits was a complex procedure.

We made the simplifying assumption that all PLX funds are spent on activities that directly affect direct placement services. Those activities include developing the computer systems to allow job seekers to view listings, working with employers to list and update job orders, and working with job seekers to accurately describe their qualifications and interests as well as find appropriate matches. The major services that go beyond providing the job banks needed to make referrals are help from staff who conduct job search workshops and develop individualized job search plans. Because we lacked the time and resources to accurately estimate the cost of those services, our cost estimates tend to overstate the actual costs of the services we measure, by perhaps 20 percent or more.

The mail survey included all types of job seekers referred by PLXs. Thus, we simply compared estimates of total benefits to total costs. For the two claimant studies, we estimated the percentage of referrals that went to claimants and pro-rated total expenditures accordingly. Table 7-1 shows the minimum and maximum estimated value of the benefit-cost ratios for each study.

Table 7-1. Benefit Cost Ratio

	Benefit cost ratios	
	Minimum	Maximum
All types of PLX users:		
Study 1: Washington Mail Survey for first half of 1998	1.7	—
Claimants alone:		
Study 2: Washington Administrative Study for 1987-95	1.2	2.8
Study 3: Oregon Administrative Study for 1995		
Oregon	1.6	3.1
Washington	2.1	4.5

Note: Study 3 compared Oregon results to those for Washington in 1995 and used published Washington data on the number of claimants who were placed and referred. In contrast, study 2 results only include placements and referrals made to claimants up to the 26th week of unemployment and exclude placements made after benefit collection ends. The difference in the number of referrals and placements largely explains why the study 3 benefit-cost estimates for Washington are so much larger than the study 2 estimates.

We regard these estimates as first-approximations of the true values rather than definitive estimates because the basic per-person effects are not estimated with precision, and the study 1 results are based on a nonrepresentative sample. In addition, these estimates do not include crowding out effects that are discussed in the next section, which reduce the benefit-cost ratios by about 20 percent. However, we believe that the true benefit-cost ratio in each of the three studies is considerably greater than the minimum estimate shown above because the value of obtaining information in the course of being referred is most likely considerably greater than crowding out and other effects that would reduce the ratios. Thus, although we cannot provide a single definitive estimate, we conclude that PLX direct placement services we studied were highly cost effective in both states.

Moreover, as noted above, we exclude from our estimates of benefits the value of additional services provided by PLXs, but the cost of providing those services was included. In particular, we believe that benefit-cost ratios would be much greater in Oregon had we measured the value of the full range of services including procedures designed to ensure that claimants are searching hard and effectively. Similarly, the Washington benefit estimates in study 2 omit the considerable value of referrals and placements delivered after claimants have stopped collecting benefits, but the cost of providing those services is included. Indeed, the 4.5 maximum benefit-cost ratio for Washington from study 3 is greater than the 2.8 estimate from study 2 because the study 3 estimate includes all placements and referrals made to claimants.

7.5 Study 4: Crowding-Out Effects

Study 4 examined the extent to which the positive effect of referrals and placements made to claimants adversely affected job seekers who were not referred. Only recently have analysts recognized the importance of estimating the negative side effects of employment and training programs. Importantly, this omission affects random-assignment studies as well as those using nonexperimental methods.

Our evidence is derived from a simulation model that traces the source of reductions in the duration of unemployment experienced by claimants referred by PLXs to (a) decreases in the amount of time it takes employers to fill vacancies, as opposed to (b) increases in the amount of time nonreferred claimants take to find new jobs. Society as a whole benefits from PLX direct placement services filling vacancies more quickly in terms of increases in the amount of goods and services and decreases in their price. However, gains to one group that come at the expense of losses to other groups merely redistribute resources without affecting the overall efficiency of the economy.

The analysis conducted by Professors Davidson and Woodbury suggests that 80 percent of the gains from direct placement services is derived from increases in overall labor market efficiency, and 20 percent is derived from crowding out nonreferred claimants who otherwise would have found jobs more quickly. This work suggests that the benefit-cost ratios described above should be reduced by 20 percent. However, the negative effects only amount to about 2.5 hours of lost work per person affected.

7.6 Summary of the Strengths and Weaknesses of our Estimation Techniques

Our most important overall conclusion is that our pilot use of a survey demonstrated a means to create highly accurate measures of the per-incident effect of placements relative to obtaining referrals but not securing interviews. This technique hinges on identifying a comparison group of referred job seekers who tried to obtain interviews after the jobs listed or their interview slots were already filled. These job seekers resemble a control group derived from a random-assignment design.

Also of great importance, surveys provide the information needed to estimate placement effects for all types of job seekers using PLX direct placement services. Studies 2 and 3 were limited to claimants largely because we could not easily determine (1) how long nonclaimants had searched for jobs prior to obtaining PLX referrals, and (2) how long nonclaimants who were not placed searched for jobs after obtaining referrals.

What we cannot do with a survey is find a comparison group that resembles a control group for measuring the value of the information obtained in viewing listings and obtaining information from PLX staff. This is a serious shortcoming because:

- Four out of five referred job seekers are not placed at jobs to which they are referred; and
- Nonexperimental evidence suggests that the effect of acquiring information in the course of being referred reduces the duration of job search by 1 week or more.

Thus, including the value of information acquired in the course of being referred is likely to increase measures of total benefits by at least 50 percent. However, without an accurate benchmark, we have to present a range of plausible estimates, including an estimate that assumes the referral effect is zero, which certainly is an underestimate.

Our second most important conclusion is that once we execute a survey that produces accurate placement estimates, those estimates can serve as a benchmark for developing nonexperimental procedures to estimates placement effects. Even though the survey estimates are not definitive, we are highly encouraged by the nonexperimental placement estimates being quite similar to the mail survey results. We believe that similarity stems from both estimates being reasonably close to the true value. We hold this view in large measure because the placement effect estimates are based on comparisons limited to job seekers referred to jobs by PLXs and thus, self-selection bias is automatically limited. Also, we feel that our estimating equations were able to account for most of the key factors that affect unemployment duration; in particular, we know if claimants are recalled and how long they were unemployed at the point they were referred.

The bottom line is that one-time results from a large, representative survey can validate nonexperimental procedures for estimating placement effects, which could then be used for ongoing monitoring. Use of a survey also should be able to provide a means to validate techniques to create key measures of job search duration for nonclaimants with administrative data.

Thus, the techniques developed and tested in this report have the potential to produce:

- Accurate point estimates of placement effects (relative to being referred) for all job seekers through use of a survey; and

- Accurate point estimates of placement effects for claimants using administrative data (once they are validated with the results of a survey).

With some additional work it also should be possible to develop procedures to produce:

- Accurate point estimates of placement effects for most, if not all, nonclaimant groups through use of administrative data validated with the results of a survey.

The nonexperimental techniques developed in this report also have the potential to produce:

- A plausible range of estimates for referral effects (relative to not being referred) for UI claimants—estimates that reflect the value of the information obtained in the course of being referred, which helps claimants to find and accept jobs more quickly on their own, as well as from job leads obtained from PLX referrals.

What we lack is a way to obtain a plausible range of estimates of the referral effects for nonclaimants. To solve this problem, we need to compare job search outcomes of referred but not placed individuals to job seekers who have not been referred and usually obtained no PLX services. However, it is difficult to identify a suitable comparison group, determine how long they have been searching, and how they differ from referred job seekers.

We also lack a way to determine the value of other services delivered by PLXs, such as group job search workshops and individualized counseling, as well as procedures used to monitor the work search of claimants (and other groups required to search for work as a condition of receiving transfer payments). Not having information about these activities leads to overestimation of total costs going to direct placement services. But far more important, excluding the effects of these activities may have led to substantial underestimation of the value of PLXs, especially in Oregon where 40 percent or more of PLX resources go to working with claimants.

Developing estimation techniques to deal with the deficits just noted should be possible, but it would take a lot of work to assemble the relevant data. Data would need to be pooled across states (with different procedures for working with claimants), and much larger and more representative mail surveys need to be created for use with relatively easily assembled administrative data.

7.7 Monitoring and Improving Ongoing PLX Operations

Up to this point we have discussed only the first of the two key questions: What are the benefits and costs of PLX direct placement services? This discussion culminated with the summary of the strengths and weaknesses of the analytic techniques used in the preceding section. This section turns to the second key question: How can meaningful ongoing feedback about the value of PLX services be provided to monitor and improve performance?

While there are important gaps in our knowledge, it is our view that having the information that can be obtained from the techniques used here would represent an enormous improvement over the ETA-9002 measures currently used to monitor PLX performance as well as the measures suggested for future use under the Workforce Investment Act (WIA). Also of great importance, it would not be difficult to produce the measures described in this report, given that most of that information would be assembled anyway to produce the WIA measures.

The primary reasons for advocating use of the measures developed here are that (1) these measures give a reasonably accurate, although not a perfect, view of the value of PLX services, and (2) the alternative measures largely reflect the influence of factors outside of PLXs control rather than the value of those services.

More specifically, we contend that use of highly imperfect measures played a decisive role in allowing policymakers to reach the erroneous conclusion that training under JTPA was far more cost effective than PLX placement services. Further, we believe that experimental evidence used to accurately compare the cost effectiveness of alternative treatments was of great importance in the change from train-first to work-first approaches across all types of employment and training programs.

In addition, not only do the 9002 and WIA measures make it impossible to judge the overall value of PLX services or the relative value of PLX services to those of other employment and training programs, but it is hard to see how a PLX operator could use those measures to improve operations. In sharp contrast, it is obvious to us that use of the information presented in this report suggests several ways to improve services and ongoing monitoring using these techniques could measure the effect of those changes.

Four policy-oriented suggestions that follow from the analysis in this report are listed below. The first three apply to claimants alone, and the fourth to all PLX clients.

- Evidence that increases in claimants' earnings are about five times greater than reductions in benefit payments suggests that both job seekers and employers would benefit more by focusing PLX activities on maximization of earnings gains rather than benefit reductions.[1]

- Evidence that the per-incident placements and referrals effects increase with claimants' unemployment duration suggests that it would be worthwhile to test the cost-effectiveness of serving more claimants with long unemployment durations and fewer with short durations.

- Evidence that placement effects account for more than half of all benefits going to claimants suggests that efforts to secure many relevant listings and help claimants find suitable listings are high payoff activities that probably should take precedence over activities that are more focused on providing information to claimants about the characteristics of available jobs or how to independently search for work.

- Our evidence that placement effects are considerably greater for job seekers with substantial work records than with spotty records suggests that it is appropriate to focus a lot of resources on claimants and other job seekers with strong work records.[2]

Of particular importance, it is hard to see how relevant evidence on any of the above issues could be derived from the 9002 or WIA measures, even if those measures were disaggregated to make the distinctions used in this report. Indeed, steps designed to maximize either the ETA-9002 measure of the number of placements or the WIA measure of the number of PLX clients who entered employment would be quite likely to reduce net benefits.

The major flaw in the ETA-9002 measure is that it treats all placements as if they are equal in value. However, this is not the case. Thus, because PLX budgets have been stagnant, measures that increase placements in total easily could reduce the number of the most valuable placements. This is particularly likely because our research suggests that the most valuable placements are those going to

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[1] Reductions in UI payments usually lead directly to reductions in a firm's UI tax rates. This is a benefit that is obvious to employer groups. Reducing the cost of unemployment to claimants most clearly benefits claimants. However, part of the cost of unemployment imposed on firms is in the form of higher wages. Thus, measures that reduce the costs of being unemployed also tend to lower wages. While this effect is less obvious, there is evidence demonstrating the existence of "wage premiums" resulting from differences in the risk of being unemployed [Classen 1976].

[2] If we were able to estimate the value of referral effects for nonclaimants we could test the important hypothesis that providing information that helps job seekers find jobs on their own is more valuable for PLX clients with spotty work records, while placements are relatively more important for claimants and others with strong work records.

claimants with long durations of unemployment, but it probably would be far easier to increase the number of placements going to other PLX clients.

The major flaw in the WIA measure is that it fails to differentiate between benefits stemming from PLX actions versus outcomes that would otherwise occur. If PLXs focused on maximizing the entered employment rate, they would want to provide services to those most likely to find work on their own, instead of helping those they can help the most who tend to be those who otherwise would be unemployed the longest without PLX help. Because it is clear that there is substantial, but predictable, variation in how long PLX clients would otherwise be jobless, it is a virtual certainty that maximization of the entered employment rate would reduce cost effectiveness.

Of central importance, there are two prerequisites for creating measures that would provide the guidance needed to improve cost effectiveness. One is that benefits need to be measured relative to what otherwise would have happened through use of comparison or control groups. The second is that benefits need to be related to costs. The primary WIA and 9002 measures satisfy neither condition. However, one WIA measure, "cost per entered employment," satisfies the second condition, but only for programs as a whole.

The following example helps explain why satisfying both prerequisites is important. According to our conceptual framework, it would be worthwhile to reduce the total number of placements by transferring the resources required to place 100 fewer short-duration claimants to place only 50 long-duration claimants as long as the benefits of placing each long-duration claimant were more than twice that of placing each short-duration claimant. In sharp contrast, maximizing placements would always lead program operators to focus efforts on the easiest to place regardless of the relative benefits and costs.

It is possible for the simple measures to be highly correlated with cost-effectiveness measures. For this to occur, however, the simple measures must be correlated with measures that use a comparison group and benefits need to be proportional to costs. As noted above, our research suggests that in both cases the needed relationship is not simply absent, but in the opposite direction.

In summary, we conclude that it would be highly desirable to put in place a system that could generate the types of measures we produced in this report. It would also be possible to use the techniques developed in this report to do so. Not only would program operators and policymakers directly benefit from having improved information, but we believe that their decisions would be greatly improved by having a far better conceptual framework for viewing their options.

7.8 Routinely Producing the Measures Described in this Report

The last section discussed the merits of implementing a system to produce the measures developed for this report. Because the benefits of having the measures appear quite high, it is reasonable to discuss their costs. We have learned a great deal about what is required to produce an operational system because the Oregon Employment Department (OED) used its own resources to assemble the data and create the estimates used in study 3.

Overall, that experience was very positive, but the process required careful attention to detail and was time consuming. As discussed in detail in Chapter 5, the process started by Westat providing OED with a large loose leaf folder describing the data processing steps, computer programs, and illustrations of the input and output files of each step we followed to produce the Washington results presented in study 2. OED had already created a data warehouse that provided basic files using well defined, common formats, across years. Thus, the work for this project involved transforming the basic files into files suitable for analysis.

With perfect hindsight, we recommend that the following steps be taken by any state creating a database suitable to produce the measures discussed in this report:

- Obtain published statistics that describe the populations included in the database and the services they received.
- Compare tabulations of the raw administrative files to the published statistics. To the extent possible, determine the source of all major differences, and correct problems that are spotted.
- At each stage in the data processing, produce formatted dumps of the input and output files, as well as counts of the number of observations. Review the dumps to make sure that every variable is being transformed properly. Correct any errors that are detected and keep the final dumps for future reference.

- Produce tabulations of the analysis file to ensure that:
  - The sample size is consistent with the initial tabulations;
  - Service receipt is consistent with the initial tabulations; and
  - The means and standard deviations for each variable are consistent with tabulations produced at the outset, reasonable estimates from published sources, and results of this and similar studies.
- Use the documentation maintained during the process of creating the analytic file to determine the source of any anomalies, and correct any mistakes that are uncovered during the analysis phase of the project.

In our own work with OED we made only some of the required checks and did not maintain complete documentation of each step. As a result, at the point we began analysis of the file we were (a) surprised to learn from published sources that we had highly faulty expectations of how the Oregon data should look based on the incorrect assumption that they would resemble Washington results, and (b) had great difficulty figuring out why the Oregon database did not look the way Oregon published statistics told us it should look.

Another important lesson from our work with OED and similar groups in other states is that it is likely to require 1 year or more to produce a satisfactory database. Also, the most difficult problem to resolve is to determine why tabulations of person-level files fail to reproduce published statistics. States should not be surprised to learn that data archiving procedures have been inadequate. In some cases it will be possible to retrieve the needed retrospective data, but in other cases the best that can be done is to ensure that the needed data are properly archived in the future.

In short, states should act as if some important problem with the data processing will go undetected, the programming errors causing the problem will be subtle and difficult to trace, and the people who created the crucial programs will either not be available at the point the problem is detected or not be able to remember what they did. Thus, states should make every effort to detect problems early, as well as be able to easily determine the source of problems whenever they are detected. To do this requires making careful comparisons with published statistics and maintaining excellent documentation throughout the entire process. Also, states should assume the process will be time consuming, and might require implementing new procedures for archiving key files.

7.9 Concluding Remarks

Our twin goals in producing this report were to satisfy a panel of technical experts that we have developed techniques that have the potential to produce sound estimates, and provide state employment security agencies with the means they could use to produce useful measures on an ongoing basis. We feel that we have met these goals reasonably well, but the true test of the value of this work is whether the measures used here are widely adopted at the state and Federal level.

Despite progress in reaching the goals cited above, we feel the work is not nearly done. This study strongly suggests that the ultimate goal of a performance measurement system should be to assess the value of all services for all groups served. At this point, figuring out ways to avoid omitting services or groups is at least as important as fully implementing the techniques described here for measuring staff-assisted placement effects for all clients and staff-assisted referral effects for claimants. Dealing with these omissions is essential for providing program operators and policy makers the information they need to improve services.

We believe that much progress can be made in extending our analysis. However, the primary challenge we see to developing a comprehensive measurement system is tracking the receipt of referrals and placements made to all PLX clients as job banks become more highly automated. Recently a state-Federal panel recommended confining most PLX measures to those services delivered by staff, and omitting those obtained without staff intervention. The analysis presented here strongly suggests that this recommendation would not produce an accurate view of the value of PLX services overall or provide guidance on how to improve services.

The central problem is that states with highly effective automated systems would not get credit for that work, but would be judged on the value of services delivered by staff to the clients who are hardest to help. We believe that this would recreate the existing situation where the expensive training programs use measures that give them far more credit than is their due, and the inexpensive labor exchange programs use measures that make them appear to be much less effective than the expensive programs.

We feel that it is fitting to conclude this report by noting that much of the work we have done will be in vain if PLXs fail to track delivery of all services to different client groups. Moreover, there is no need to let this happen. Oregon has adopted a unique system that requests clients identify themselves at the point they want to obtain automated contact information to refer themselves to promising listings.

The strengths and weaknesses of the Oregon system should be examined. If the voluntary identification process is not overly burdensome to clients and produces a representative sample of referred clients, it should be universally adopted. Otherwise, it will continue to be impossible to make fair comparisons across major employment and training programs and determine what steps would most improve the value of their services.

Chapter 8

8. THE EXPERT PANEL'S REVIEW

8.1 Introduction

An important element of this project was to obtain the views of technical experts regarding the accuracy of the methods used, completeness of the topics covered, validity of the results, and usefulness of the methods for monitoring ongoing PLX operations.

Given the controversies generated by past attempts to rigorously measure the strengths and weaknesses of employment and training programs, we felt that it would be of great value for ETA to know which elements of the work are acceptable to experts and which are of questionable value. The expert panel's review also is particularly timely because it provides advice on how Westat's ongoing PLX benefit-cost analysis should be conducted.

The next section presents a brief description of the experts' credentials. We then present the two letters Westat sent to the expert panel describing what we asked them to do. This is followed by the summary comments of the four panel members. Next, we describe areas of agreement and disagreement. Some information for this section is drawn from written comments about specific points in the text, which were not designed for publication here, as well as from informal discussions. We conclude by discussing the panel's suggestions for next steps.

8.2 Description of Panel Members

The panel members were selected because they are thoroughly familiar with the underlying issues in program evaluation and are exceptionally well qualified to critique the best econometric work in the field. Indeed, these individuals have carried out some of the best work themselves, and are knowledgeable about the work that has been, and is being, conducted at the major research universities.

Dr. Jeff Smith of the University of Western Ontario has published several important papers on the use of experimental and nonexperimental methods in estimating the impact of employment and training programs on participants, and is currently examining the effectiveness of UI profiling in

Kentucky. He also has worked closely with Dr. Jim Heckman of the University of Chicago on the National JTPA Study, which evaluated Title II programs.

Dr. Burt Barnow of Johns Hopkins University has published important papers about a variety of evaluation topics, especially the relative merits of output measures versus value-added measures as performance indicators. Dr. Barnow also served as ETA's Director of Research and Evaluation from 1979 to 1984. While in that position he reviewed and helped guide the only large-scale prior study of PLX effectiveness as well as a variety of other program evaluations including the National Study of JTPA Title II.

Dr. Dan Sullivan of the Federal Reserve Bank of Chicago has published important papers on the econometric evaluation of employment and training programs, and has worked with Dr. Card, then at Princeton University, on several highly relevant evaluation studies. Dr. Sullivan also published a highly regarded study (with Drs. Jacobson and LaLonde) on the earnings losses of dislocated workers using UI administrative data, and has used similar data to study the returns to training.

Dr. Steve Woodbury of Michigan State University and the Upjohn Institute has published important papers on the effectiveness of UI bonuses using experimental and nonexperimental techniques. He has also examined many other aspects of the UI system (sometimes with Dr. Jacobson) and is particularly familiar with techniques used with administrative data to determine the effect of UI on unemployment duration. Dr. Woodbury co-authored Chapter 6 of this report, which he does not comment on, but did not work on other aspects of this report. However, because Drs. Sullivan and Woodbury have co-authored papers with Dr. Jacobson and worked together on research contracts, we present their comments last.

8.3 Westat's Letters

This section includes the two letters Westat sent to the expert panel members describing the help needed. The second letter was required because ETA requested that we include a version of their comments in this report that would be understood by a wide audience. Earlier the experts were told in response to their questions that the report's authors were the primary audience for their comments, and their detailed comments would not be published.

8.3.1 The Initial Letter

24 July 2000

Dear [expert panel member's name],

Enclosed is a copy of our final report summarizing what we learned about PLX direct placement services. The expert panel reviews of the key papers were extremely useful in shaping this final report. I am sure that you can tell that a lot of hard work was required to take your comments into account.

I am deeply grateful for your excellent reviews of the initial papers, and also would be very appreciative if you could quickly review this report. A US-DOL seminar on this work has been scheduled for Thursday August 3rd. It would be of enormous value if you could at least give me a quick read on whether the key points in the executive summary and the summary in Chapter 7 are well supported by the evidence.

Westat is willing to pay you $200 for a quick overview delivered by noon Wednesday August 2nd, and an additional $500 for a more detailed review that includes specific comments on the strengths and weaknesses of the technical analysis in the report as well as suggestions for how to proceed with similar studies using data from Colorado, North Carolina, Massachusetts, and Michigan. I would like the more detailed review by the end of August, but can allow more time if necessary.

As with the material you previously reviewed, the technical core of the study is Chapter 3 which discusses the analysis of the Washington mail survey, and Chapter 4, which discusses the Washington analysis using administrative data.

Chapter 5, which directly compares the 1995 results for Oregon with results for Washington, has some interesting findings from a policy point of view because Oregon used its own funds to dramatically increase job orders and services going to claimants. However, the estimating equations are basically identical to those used in Chapter 4.

My prime short-run interest is obtaining your views on whether the estimates of the placement effects (relative to being referred but not placed) are as accurate as I claim. All panel members felt that one of the most interesting findings in the entire study is that about one-third of those referred but not placed were unable to secure interviews. I hope you concur that it is reasonable to believe that lags in removing listings after jobs were filled was responsible for almost all of these failures, and thus, this group strongly resembles a control group.

I should note that my confidence in these estimates also was boosted because I eventually realized that estimating placement effects poses less formidable estimation problems than measuring referral effects (relative to not being referred) because those placed are compared to those referred who also volunteered to obtain direct placement services.

With respect to the referral effect analysis in Chapter 4, I would appreciate your comments on the importance of the regression adjusted estimates converging to the unadjusted estimates based on mean values (see Table 4-2 on page 4-10). Does this support the view that breaking the estimates into five groups based on when services were delivered substantially reduces both measured **and unmeasured** difference between those referred but not placed and those not referred?

Finally, while my primary interest is in obtaining your views about the accuracy of the technical conclusions, I am also interested in your comments on the policy issues raised in the report. There are five policy-related conclusions about which I particularly could use your feedback:

1. Would it be a serious problem if 30 to 50 percent of referrals and placements were untrackable because contact information is unsuppressed? (Except in Oregon, when job seekers self refer themselves PLXs are unable to tell who was referred and whether those referrals resulted in placements.) In my view this is a serious problem because the distinction between job seekers who are placed, referred but not placed, and not referred would become very hazy.

2. More generally, does it make sense to only track staff-assisted services and not track self-service aid? Because states vary tremendously on how they provide services, omitting self-service aid would totally wreck any ability to make cross-state comparisons. Even more importantly, the more effective a state's self-service system, the less credit it is likely to get. The bottom line is that it seems obvious to me that the only fair way to evaluate a program is to compare all the benefit to all the costs. Omitting what is likely to be the greatest benefit makes little sense.

3. Wouldn't it be useful for making key resource allocation decisions to have the information about how benefits are divided between: (a) placement effects and referral effects, (b) services delivered early in a claimant's unemployment spell versus late in a spell, and (c) services delivered to claimants and others with strong work histories versus those with weak work histories (see page 7-13).

4. More generally, wouldn't use of the WIA entered-employment and similar measures create huge incentives to cream by treating those who are most likely to find jobs on their own? Wouldn't value-added measures be far superior?

5. Finally, now that Oregon has the capacity to create the measures presented in chapters 4 and 5 would you recommend that it use those measures instead of the entered employment rate and similar measures? It seems to me that, although the measures presented in the report are imperfect, they are substantially better than existing alternatives to warrant their use, but I would greatly appreciate your views on this issue.

Thanks again for your already outstanding contribution to this work, and I hope you will be able to provide further aid.

Sincerely yours,

Lou Jacobson

8.3.2 The Followup Letter

MEMO

TO: Expert Panel Members
FROM: Lou Jacobson, Westat
SUBJECT: Adding a summary to your reviews and confirming the delivery date.
DATE: 14 August 2000

I greatly appreciate the assistance you have already provided in dealing with the difficult task of giving policymakers at the Employment and Training Administration (ETA) an honest appraisal of what the work contained in the PLX report implies about the benefits and costs of direct placement services.

Your primary role has been to provide a technical review of the strengths and weaknesses of the evidence and how the weaknesses can be overcome in future work. As most of you know, ETA has requested that your reviews be included in the final version of the report. Thus, I am amending the original request for comments on the five points relating to the usefulness of value-added measures and reviews similar to those appropriate for a professional journal that describes: (a) specific points in the report that need to be corrected, and (b) what additional work should be undertaken in the future to deal with the weaknesses in the report.

In addition to a your technical review, I would like you to provide a summary of your main conclusions that is designed to be easily understood by policymakers and others not familiar with the technical issues dealt with in your detailed comments. If you provide the review with this summary by Thursday August 31st Westat will pay you an additional $100, which will bring the total to $800 for most of you. Please let me know if you will, or will not, be able to meet this deadline.

Also, you might want to provide a second version of your technical comments that you feel is more suitable for publication. (Reviewers noted that there is some material they would like me to have, some of these comments are interesting, but tangential to the main issues, while others are useful only for technical experts.)

In producing the summary I am asking you to deal with a problem analogous to the one I had to deal with in drafting the executive summary—making the key results as clear as possible to non-experts, without giving them a view that is too negative or too positive.

While I know that reviewers feel that I erred on the side of being too positive, I am concerned that economists tend to hold results up to a standard that is rarely possible to obtain. Thus, we end up caveating results in a way that makes it extremely difficult for policymakers to figure out what they should make of the research they funded. Indeed, as Bob noted, the $23 million random assignment National JTPA study was persuasive mainly because it confirmed what other studies had already indicated were true, but were not proven.

Finally, it is very important that you clearly summarize your recommendations for addressing your technical concerns. In particular, ETA needs guidance on how pursuing the points you raise will affect the value of the evidence produced in our on-going benefit-cost study.

8.4 Expert Panel's Comments

This section presents the summary comments prepared by each of the four panel members.

8.4.1 Comments by Jeffrey Smith

Both evaluation of public labor exchange (PLX) programs and the development of reasonable performance measures for them are important and worthy tasks. The PLX is a program with a long history, but with little in the way of either theoretical economic justification for its existence or a solid evaluation track record. The strength of this report is that it outlines a promising methodology both for ongoing evaluation of the program and, potentially, for use in performance management. With some changes in administrative data collection procedures and some training, state staff probably could employ the methods proposed here using administrative data. The comments in the report on current and proposed performance measures are particularly appropriate, and reflect the consensus of the small amount of literature that has examined the performance of performance measures.

The report is also notable for highlighting the role of displacement effects in properly evaluating active labor market policies. Displacement occurs when a program participant takes a job that would otherwise have been filled by a nonparticipant. Failure to take such effects on nonparticipants into account means that an evaluation will likely present an overly positive picture of the net impact of a program. The North American literature on program evaluation (including otherwise high-quality experimental evaluations) often ignores displacement effects, even though the available evidence suggests their importance for programs that focus on job search skills and direct placement, as the PLX does.

This study also has important weaknesses. Substantively, there are many reasons to question the estimated impacts present in both the analysis of the mail survey data in Chapter 3 and in the analysis of the unemployment insurance (UI) claimant data for Washington and Oregon in Chapters 4 and 5. I detail these problems below. Taken together, these problems with the estimates suggest that claims for their accuracy in the text of the report should be ignored. These estimates should be used to guide policy *only* after the problems have been corrected. Instead, they should simply be taken to illustrate the feasibility of the proposed procedures. In terms of style, I found the report consistently took positions that were far too positive towards the program. This was off-putting to me and reduced the credibility of the analysis. The role of independent evaluators should be restricted to sharp and sober analysis.

8.4.1.1 Mail Survey Estimates

The methodology outlined in this section relies on the insight that some persons referred to employers by the PLX do not obtain interviews because the job has already been filled. These late referrals constitute a valuable comparison group that can properly be described as resulting from a natural experiment. As such, this methodology represents a promising way to estimate the impact of an interview generated by the PLX. By extension, the analysis then proposes to use a variant of the so-called Bloom estimator to generate an estimate of the impact of a placement. The reasoning here is that if an interview not leading to a placement has an impact of zero, then all of the mean impact associated with interviews must be concentrated among those getting placements. By rescaling the mean impact of an interview by the fraction placed, an estimate of the impact of a placement is obtained.

While the basic strategy is sound, some important issues of implementation and interpretation remain. These issues are important enough that they cast strong doubt, not on the estimator itself, but on the estimates actually presented in the report. First, the report does not present sufficient evidence to show that all persons who tried but failed to obtain interviews had applied after the jobs or interview slots were already filled. There are other reasons why such persons might not have obtained an interview, such as employer prescreening. If those reasons are important, then they undermine the natural experiment relied on in constructing the estimates. Clarifying this issue requires the collection of further data on the experiences of these individuals; such data collection has been proposed by Westat and is supported by this reviewer.

Second, it is likely that the set of jobs for which referrals are made after the job is filled are not a random sample of all jobs to which persons are referred by the PLX. Intuitively, one would expect that "better" jobs would get filled faster and thus be more likely to have disappointed referrals. If the set of jobs that generate disappointed referrals is not a random sample of all jobs to which persons are referred by the PLX, then the estimates obtained by this strategy do not estimate the quantity of primary interest, which is the average effect of a referral. Instead, they represent the average effect of a referral to a job that is better than average! Further evidence on the representativeness of the jobs that generate disappointed referrals is required before putting the method proposed here into practice.

Two issues of implementation also raise doubts about the reliability of the actual numbers presented in the report. First, the response rate to the mail survey is very low. The report points this out, and provides suggestions about how to improve the response rates, but then largely ignores its own caveats in interpreting the impact estimates. Second, in constructing the comparison group, disappointed referrals who had another referral within four weeks are excluded. This is incorrect. More precisely, the counterfactual generated by this procedure does not correspond to the stated parameter of interest. What should be being estimated is the impact of an interview, not the impact of an interview plus not getting a referral again for four weeks. The second restriction should lead to an upward bias in the impact estimates—probably a fairly large upward bias. As a result, there is good reason to heavily discount the estimates presented in the report and instead to focus on what it reveals about the future potential of the proposed methods.

8.4.1.2 Administrative Data Estimates

I do not have much to say about the administrative data estimates. In addition to the problems that carry over from the analysis of the mail survey data, the main issue here is the inclusion of some quite clearly endogenous variables in the estimating equation used to generate the impact estimates. These variables capture the "job status" of the claimant—in particular whether or not he or she returned to the same firm. This variable is almost certain to be correlated, possibly strongly so, with the error term, thereby leading to bias in all of the estimated coefficients. This variable should be dropped from the analysis. Also, given my concerns about upward bias in the estimates from the mail survey, I do not find the fact that the estimates using administrative data come close to the mail survey estimates particularly comforting.

8.4.1.3 Displacement Effects

The report does a good job of laying out the issue of displacement and of highlighting its importance in the evaluation of active labor market policies. Westat also deserves credit for contracting with two of the top researchers in the field to perform its analysis. My main concern is that the amount of sensitivity analysis presented is well below the norm in the literature for this type of modeling. There are a number of fairly controversial assumptions that underlie the model; it would be nice to know the sensitivity of the resulting estimates to relaxing these assumptions or to handling them in different ways.

Secondly, the sensitivity analysis that is presented is largely ignored in the textual discussion. In fact, the estimates are quite sensitive to small perturbations of the two model parameters that are examined in the sensitivity analysis. The pattern of this sensitivity is not an intuitive one and so cries out for further investigation and explanation. Taking the results at face value suggests that in times of low unemployment, such as the present, displacement may offset over 70 percent of the gross impact of the PLX, making it almost certain to fail a cost-benefit test. This is potentially a very important finding but it is one that is glossed over in the report's discussion, which consistently, and incorrectly, refers to effects of between 25 and 70 percent of the gross impact as "small."

Thirdly, the discussion of the displacement estimates in the text does not place them into the context of the existing literature in this area. Indeed, it does not even relate the estimates to the author's own estimates of displacement effects from the UI bonus experiments! Given the well-known sensitivity of the general equilibrium models of the labor market used to estimate displacement effects, a careful link to the existing literature is crucial for assessing the reasonableness of the estimates.

8.4.1.4 Cost-benefit Analysis

The report does a good job of emphasizing the importance of holding government programs to a strict cost-benefit standard. Other than my concerns (described above) about the estimates that feed into the cost-benefit analysis, I have two main concerns about it.

Firstly, the cost-benefit analyses in the report omit the deadweight costs of taxation from their calculations. A dollar in tax money to pay for a program such as the PLX costs the economy more than a dollar in resources because the tax system that collects the funds distorts individual choices. These costs are real and reflect the burdens associated with providing services through the government rather than the market. In some cases they are worth paying due to market failures of various sorts that make the benefits of government provision large enough to cover them. However, in all cases they must be factored into the cost-benefit analysis. As there is some disagreement in the scholarly literature about the size of these costs, the appropriate way to incorporate them is to repeat the analysis with two or three reasonable values selected from the literature.

Secondly, some of the cost-benefit calculations presented in the report ignore the report's own estimates of the effects of displacement on the net impacts associated with the PLX. Displacement also represents a real cost associated with a program such as the PLX, and should be included in all of the cost-benefit estimates that the report presents.

8.4.1.5 Performance Measures

There is a small amount of literature on performance measures in employment and training programs. This literature suggests that existing measures based on outcome levels do a poor job of proxying for the actual effect of programs on participants. This report correctly summarizes the points made in the literature and suggests the value of pursuing measures that actually attempt to estimate value-added. Such measures are almost certain to do better than the dismal performance reported for existing measures in the literature. As a result, such measures, including the ones suggested in this report, clearly deserve further investigation with an eye toward implementation in practice as soon as is feasible. It is also important to note that once a reasonable set of measures based on value-added has been developed, if at all possible they should be validated using a true random assignment experiment.

8.4.1.6 Summary

The main points of my comments are five. First, the estimator and potential performance measure proposed in the report show promise and deserve further investigation. Second, there are good reasons to heavily discount the actual impact estimates presented in the report. In my view, they probably are strongly upwardly biased. A lot of additional work is required to produce estimates that could be used a guide to policy. Third, the report does a good job of illustrating the importance of accounting for the displacement of unemployed workers not served by the PLX by unemployed workers who are served by the PLX. However, the analysis in the report leaves many questions open regarding its estimates of displacement effects, questions that deserve further investigation. Fourth, the points made in the report about current and proposed performance measures for the PLX program are right on target. The literature indicates that measures based on outcome levels do not provide a sound guide to performance. Value-added measures such as those proposed in this report are almost certain to do better. Fifth and finally, the overly positive view of the program that occurs throughout the report serves only to irritate the reader and diminish the credibility of the analysis.

8.4.1.7 Answers to Questions in the Cover Letter

Q1. Would it be a serious problem if 30 to 50 percent of the referrals and placements were untrackable?

A1. Yes. I agree that the absence of this data is a serious problem.

Q2. Does it make sense to only track staff-assisted services?

A2. My impression is that self-service aid in terms of job referral is becoming more important over time as the technology improves and because it can yield budgetary savings. To the extent that it does become (or has become in some states) a major component of the assistance provided to relatively job-ready nonemployed persons, it should be included in the performance management and evaluation system. One reason is the "fairness" (always a word to be careful of) argument made in your letter. I would not say fairness, but rather that you do not want to artificially penalize or reward states for having or not having self-service systems. This distorts their incentives away from efficient service provision. Second, you want to evaluate the self-service component of services as well. This component can be operated well or poorly and that is something that the states and the Federal Government should want solid evidence about.

Q3. Would it be useful to know how benefits are divided between: (a) placement effects and referral effects, (b) services delivered early in a claimant's unemployment spell versus late in a spell, and (c) services delivered to claimants and others with strong work histories versus those with weak work histories

A3. Yes, estimates conditional on each of these things would be very useful.

Q4. Would the use of measures like the entered employment rate create incentives to cream?

A4. I agree that the WIA and JTPA-style entered employment rate standards create incentives for cream skimming. What the literature lacks are two things: evidence that cream skimming is a bad thing and evidence that these incentives play out in practice. In our paper with Clements, Jim Heckman and I present some estimates that suggest that the efficiency costs of not cream-skimming in JTPA may be low. In this case, the equity benefits of not cream-skimming may be worth the costs. However, much more evidence on this score is needed. If the benefits of treatment are larger for the cream, then cream skimming is what you want to do, at least from an efficiency standpoint. We argue in the Heckman, LaLonde and Smith chapter in the *Handbook of Labor Economics* that this is probably true for the services usually provided by JTPA, WIA and their analogues elsewhere. In terms of the second question, the empirical evidence of cream skimming in practice is weak. Jim and I survey it in our paper with Carolyn Heinrich presented at the NAS conference on incentives last fall. The Burkhauser et al. papers confuse self-selection with selection by JTPA. The evidence in the Cragg paper in the *Rand Journal* is mixed, and the evidence from our paper with Chris Taber on Corpus Christi shows not cream skimming but bottom scraping in JTPA.

Q5. Should Oregon use the measures of your study?

A5. Yes. Value added measures are conceptually far superior to measures based on outcome levels. There is a small literature that tries to "validate" the measures based on outcome levels by comparing them to experimental impact estimates. This literature is surveyed in the Heckman, Heinrich and Smith NAS paper, where we also discuss some issues regarding the interpretation of these validation exercises. Other than the AFDC Homemaker Home Health Aide program, which provided a homogeneous treatment to a homogeneous population and so is not very comparable to WIA or the ES, the performance measures based on outcome levels do not perform very well.

8.4.2 Comments by Burt Barnow

This report attempts to determine the impact of the Employment Service (ES) labor exchanges. Because the ES provides a low-cost treatment that is likely to produce relatively small benefits per person, measuring the impact of the program is quite difficult. The mail survey used here provides an interesting and potentially useful way to measure the benefits of the ES labor exchange services.

While I agree that this highly innovative approach may be appropriate here, the authors need to provide better evidence to support their central contention that delays in removing job listings lead to many PLX users obtaining referrals after the jobs have already been filled and that placements among those referred are virtually random events. I did not see the anecdotal evidence from PLX managers and staff as sufficient to establish this central point beyond a reasonable doubt. This is such an important point it cries out for an independent survey of employers.

In addition, the authors make some extremely strong assumptions when they use their basic results to obtain overall benefit-cost estimates and validate nonexperimental estimators. Thus, several key results are of questionable validity because the assumptions are untested, and in some cases, the authors do not make the tenuousness of the underlying assumptions sufficiently clear.

The worst problem is that the authors assume that point estimates clearly drawn from a small sample, plagued by a very high nonresponse rate, apply to the universe of placements. While the authors may be correct that a useful natural experiment exists, a much larger and far more representative survey is required to produce unbiased estimates that are reasonably close to the true values. The high nonresponse rate could lead to biased results, and the only way to know for sure is to actually contact a sample of the nonrespondents to see if their omission biases the results.

A similar problem is that the authors take the above point estimates and use them as evidence that the nonexperimental estimates of placement effects are unbiased. Again, while it is plausible that the authors are correct in implicitly assuming biases are small, they are stretching their results beyond reasonable limits. At the very least, they need to make it clearer to the reader that better evidence is needed to prove their point.

Finally, the displacement simulations are interesting and illustrate that displacement may not be a serious problem, but they certainly do not prove that.

Overall, I would have been much happier with the report had the authors presented their findings as illustrating interesting approaches, rather than asserting that they have obtained highly accurate, unbiased estimates, of the impact of the program. I think that with additional work they can obtain the evidence needed to convince me that the placement effect estimates are valid. However, it will be much more difficult to rigorously demonstrate the validity of the referral effect estimates generated using Washington administrative data. I saw nothing in the report to suggest a means to test the key assumptions underlying those estimates.

I also found the tone of the report, especially the executive summary, to be entirely too upbeat.[1] One of my major concerns is that the findings may be specific to particular types of clients and very well may not apply to states where the ES and/or employers use the system differently. More

[1] The executive summary and several other parts of the report were extensively modified in response to the expert panel's review. These comments were made before those changes were implemented.

specifically, in the absence of survey data from job seekers and employers using the Oregon PLXs, I would discount the nonexperimental evidence in Chapter 5 in its entirety. In short, while I applaud the approach, and think that the paper has done a fine job of bringing up new ways to evaluate the ES, the presentation overstates what can legitimately be concluded from the work.

In addition, the authors present their findings in a rather unorthodox manner, and this is likely to confuse many readers. Usually in an evaluation, one wishes to compare what happens to those who receive a treatment (labor exchange services) to what would have happened if they had not received the treatment. Thus, the treatment is not a referral and it certainly is not a placement, but rather it is the receipt of labor exchange services. It would be helpful if Chapter 2 began with a discussion of the flow of participants through the ES. It would then be clearer that placements are an outcome. The services are interviews, reviewing resumes, taking job orders, etcetera. The authors then need to provide a better explanation for why they departed from the standard approach.

Ultimately, as the authors point out, programs should be judged on how their benefits relate to the costs. The authors provide a cost-benefit analysis, but I would view their figures as highly preliminary and I would certainly not use them as the basis for policy. My first concern is that I have little confidence in the impact estimates; if the authors had not been so confident in their findings, they would have conducted some sensitivity analysis. Second, the preferred method of presenting the results of cost-benefit analysis is using a net present value rather than a benefit-cost ratio. This is especially important for low-cost treatments such as the employment service, because a small change in the estimated benefit can have a large change in the ratio.

Third, the report should acknowledge that it is missing some of the potential benefits of the program. For example, society could benefit because employers get a better match for their jobs in the long run. Also, even though the simulation suggests that about 80 percent of the benefits stem from decreases in the time it takes employers to fill vacancies, the value of this short-run benefit to society is not estimated.

Fourth, it is conventional to provide cost-benefit calculations from the perspectives of the participants, the government or taxpayers, and society as a whole. From the participant perspective, we know from other studies that some unemployment insurance recipients would prefer not to use the ES but do so because of the work test. Thus, to some extent there is a cost from their perspective in obtaining labor exchange services they do not want. It is important to consider the government perspective because current law calls for program changes to be "cost neutral" from the fiscal perspective. Finally, in looking at the results from a societal perspective, both the benefit payments and the employer taxes wash out as transfer payments. What would remain are the costs and the unmeasured benefits of reductions in the time it takes to fill vacancies.

In sum, I view this report as an interesting attempt to rigorously evaluate a program that is very hard to evaluate. As such, it is an important piece of work that should stimulate additional research. One must be very cautious, however, in how much credence one places on the point estimates presented in the report.

The above comments should not be interpreted to mean that the survey approach used by the authors is wrong. Indeed, their evidence suggests that the approach may be viable. My primary concern is that the pilot study did not go far enough to produce valid point estimates. I strongly recommend that the approach be further refined to see if the key assumptions can be validated and more reliable estimates produced. This is especially important because the services being studied have now become the center piece of employment policies and universal access precludes use of experimental designs.

In order to produce valid estimates of placement effects it is of great importance that the authors obtain evidence about the reasons job seekers are unable to obtain interviews from a large and diverse sample of employers. Assuming that information confirms the authors' central hypothesis, it then would be necessary to estimate placement effects using a large, representative, survey of referred PLX users. Finally, I would want to see job-seeker and employer survey results from at least four states before I would be willing to accept that the method for producing estimates of placement effects using administrative data alone is valid.

Responses to specific questions you raise in your letter of July 24, 2000

Q1. **Would it be a serious problem if 30 to 50 percent of the referrals and placements were untrackable?**

A1. I agree that it is potentially a serious problem. Large-scale attrition/nonresponse is always a concern if you don't know how it affects your answers.

Q2. Does it make sense to only track staff-assisted services?

A2. No. As you note, self-service is the way we are headed- not just in the ES. Both WIA and TANF (at least in some places) place heavy emphasis on self-service. We need to know how this compares to assisted service. By the way, here is where you can do an experiment where no one need be denied service, but some would get full service and others get shown to the kiosk.

Q3. Would it be useful to know how benefits are divided between referrals and placements?

A3. Not exactly. As I have said many times, placements are an outcome, not a service. However, it is quite legitimate to ask if only those who are placed get any benefits from the ES.

Q4. Would the use of measures like the entered employment rate create incentives to cream?

A4. Perhaps. In the old days, JTPA adjusted the standard based on characteristics so that you could neutralize the incentive to cream-I have written about this in several papers. In theory, you could do this with ES as well.

Q5. Should Oregon use the measures of your study?

A5. I really do not know enough to answer this one.

8.4.3 Comments by Daniel Sullivan

This study provides the best evidence to date on the benefits generated by public labor exchanges (PLXs). It employs two approaches to estimating the extent to which PLX job placements shorten unemployment spells. One approach is a standard, regression-adjustment methodology that controls for the large number of labor market and worker characteristics that are available in administrative data on UI claimants. The other is a highly innovative approach that appears to be a breakthrough in the difficult problem of evaluating PLXs. It exploits the fact that, because of lags in removing job listings from the PLX, referrals are often made to jobs that already have been filled or for which the employer is no longer interested in more applicants. The study identifies a sample of workers receiving such referrals from a mail survey to use as a control group, comparing their unemployment durations to those of workers who were able to obtain interviews with the firms to which they were referred. These groups should closely resemble each other in terms of characteristics other than whether the worker was able to obtain an interview. Thus the comparison should provide a good estimate of the value of a PLX referral leading to an interview compared to one that doesn't. Assuming that the value of the interviews is entirely attributable to those that result in placements allows the authors to estimate the value of placements. The report also presents useful evidence on the extent to which PLX job referrals that do not lead to placements shorten unemployment spells and on the extent to which benefits provided to those using the PLX come at the expense of those that do not use the PLX.

The results strongly suggest that PLX placements significantly reduced workers' unemployment durations relative to receiving a referral that does not result in a placement. The regression-adjustment methodology suggests an average reduction of 7.7 weeks for workers with significant work experience, while the natural experiment afforded by identifying referrals to jobs that could not have led to interviews suggests a slightly smaller effect of 7.2 weeks for this group. This rather close agreement adds to the credibility of both estimators. The natural experiment methodology is also applied to workers with minimal work experience, yielding an estimate of 3.8 weeks. This suggests that PLX placements are more valuable for workers who have some work experience and thus may have specialized career needs. Valuing the reduced unemployment durations according to workers' earnings on their new jobs and taking account of the number of placements, these estimates imply significant benefits from PLX placements.

The report also quantifies the benefits derived by workers who received referrals that did not result in a placement. These estimates are based only on a regression-adjustment methodology because there is no obvious analogue to the control group identified for estimating the effects of placement. Results suggest that referrals that do not lead to placements give workers enough additional information about the labor market to reduce their unemployment durations by about two weeks, an effect which also implies significant monetary benefits from PLXs.

The final part of the study estimates the extent to which the benefits derived from PLX clients make it more difficult for those who do not use PLX services to find jobs. These results make use of the state-of-the-art methodology developed by Carl Davidson and Stephen Woodbury in several published articles. This methodology makes use of an equilibrium search model calibrated to Washington State data. The results suggest that approximately 20 percent of the benefits derived by PLX clients come from crowding-out effects imposed on those who do not receive PLX services.

Combining the results from the various parts of the study, the report presents estimates of benefit-cost ratios associated with PLX services. Even the most conservative estimates, which completely discount the value of PLX referrals that do not lead to placements, suggest that PLXs provide a good return on investment. Less conservative, but still highly plausible, assumptions suggest that PLX services are an extremely good investment. As is discussed below, there are still several reasons that the benefit-cost ratios discussed in the report are subject to some uncertainty. However, for the purposes of formulating policy, they represent the best available evidence on the effectiveness of PLXs. Of course, policymakers should keep in mind that the results apply to what are now relatively modest expenditures on PLX services. Very large expansions of those services likely would be subject to some diminishing returns, which would reduce the benefit-cost ratio.

While the evidence presented in the report is the best now available, there are still reasons to view the conclusions as subject to some uncertainty. These reasons are discussed forthrightly in the report. The potential problem with any application of the regression-adjustment methodology is that there may be unmeasured differences between those receiving and those not receiving services. Such differences would not be controlled for by the methodology unless the characteristics happen to be perfectly correlated with characteristics that are measured. Moreover, when workers or program administrators have some freedom to choose whether a worker participates on the basis of unmeasured characteristics, some bias is possible even after controlling for all known characteristics of workers. The present regression-adjustment results are no different from others in facing this challenge. The extent of information available on UI claimants is, however, significantly greater than in most program evaluations. This is especially true given the report's adjustments for time spent in unemployment prior to receiving services. As in any study, it is always possible that an additional control variable would have changed the results. But given the extensive number of controls already employed, it seems unlikely that new controls would significantly change the results.

In the case of the effects of placements, the regression-adjustment methodology is, moreover, significantly buttressed by the addition of information based on the natural experiment afforded by the identification of "stale listings" in the PLX. This new method is a definite breakthrough. But, as the report notes, the current results are still somewhat preliminary, owing mainly to a low response rate and less than fully complete information on why some referred workers were unable to obtain interviews. Specifically, because responses were obtained from somewhat less than a quarter of those surveyed, the sample sizes are small enough that statistical variable is likely nontrivial. Moreover, the low response rate raises the question of whether those who did respond were representative of all targeted workers. If not, then the results may be subject to some bias. Also, the interpretation of the report is that workers who reported being unable to obtain an interview are identical in other respects to workers who did obtain interviews. This interpretation is supported by the reports of PLX administrators. However, it is possible that some who could not obtain interviews were screened out by employers on the basis of pre-interview information. If so, they would likely be different than those who did receive interviews in ways that would independently effect unemployment durations. Unfortunately, the survey did not allow such workers to be identified.

Subsequent analyses should be able to address some of the reasons for uncertainty just described, yielding even better estimates of the effects of PLX services. In particular, bigger samples with higher response rates could be derived from a (more expensive) telephone survey. Such a survey could also probe more deeply about the reasons why some referrals do not lead to interviews. As the report discusses, both of these steps would go a significant way towards further reducing the uncertainty about the current estimates.

Additionally, if the requisite data could be obtained, there is another possible improvement that the report does not discuss. That is, the administrative data files may contain information that may help to predict when a job listing is likely to be stale. For instance, it may be known how long the job had been listed when the referral was made, or better, how long after the referral was made that the job was removed. If such variables were obtainable, they could be employed as what are known as instrumental variables in the analysis of job placements. Survey data would be used to verify that these variables do have a correlation with whether workers are able to obtain interviews and then to estimate a function relating these variables to the probability of obtaining an interview. This formula could then be used with the administrative data to compute predictions of whether an interview was obtainable which would replace the indictor for placement in the current analysis. Of course, as noted, the feasibility of implementing this strategy depends on the availability in administrative data of variables that are correlated with workers ability to obtain an interview after referral.

In summary, though improvements are possible in future work, the current report provides a good base of information for policymakers concerned with the operation of PLXs.

Responses to questions posed in July 24, 2000 letter. Daniel Sullivan 8/31/2000

Q1. Would it be a serious problem if 30 to 50 percent of the referrals and placements were untrackable?

A1. It seems clear that being unable to track self-referrals will make it much harder to evaluate the benefits of PLXs.

Q2. Does it make sense to only track staff-assisted services?

A2. It wouldn't be logical to base a cost-benefit evaluation of a PLX on all of the costs and only some of the benefits and clearly those self-referring obtain some benefits from a PLX.

Q3. Would it be useful to know how benefits are divided between referrals and placements?

A3. Having such an accounting of benefits would be useful.

Q4. Would the use of measures like the entered employment rate create incentives to cream?

A4. The entered-employment rate is not a good measure of the value of a PLX and is likely to lead program administrators to avoid difficult-to-serve populations.

Q5. Should Oregon use the measures of your study?

A5. I think the measures discussed in Chapters 4 and 5 are likely to be superior to the entered-employment rate in judging the value of a PLX and in giving the operators the proper incentives to maximize the benefits for society.

8.4.4 Comments by Stephen Woodbury

This report describes the results of an extensive study of the impact of the public labor exchange (PLX) on the unemployment duration of jobless workers. The report includes four main substantive chapters. The first (Chapter 3) relies on a mail survey of workers who were referred by the Washington State PLX between January and July 1998. This mail survey, which was sent to 6,000 referred workers, yielded 587 usable responses. The second (Chapter 4) relies on administrative data on over 300,000 spells of insured unemployment between 1987 and 1995 in Washington State. The third (Chapter 5) uses administrative data on nearly 140,000 spells of insured unemployment during 1995 in Oregon. The fourth (Chapter 6) reports on some simulations of the extent to which PLX placement activities crowd out workers who are not users of the PLX. (Because I am a co-author of Chapter 6, I will have no comment on that chapter.)

Estimating the impact of the PLX is difficult for two reasons. First, randomized trials are ruled out because basic services cannot be denied to workers who desire them. Second, comparisons of PLX users with nonusers in a nonexperimental evaluation are potentially contaminated by self-selection—workers who use the PLX may different from non-users in ways that may be difficult to control for. Accordingly, the efforts in Chapters 3, 4, and 5 to sort out the impacts of the PLX require some innovative and rather clever evaluation designs.

8.4.4.1 Chapter 3

The estimator of PLX impact that is used in Chapter 3 makes use of the survey data and compares (1) interviewed workers with (2) workers who got a referral and followed up by trying to get an interview, but then did not get an interview. In terms of Figure 8-1, the comparison is across the third row from the bottom. Interviewed workers may or may not be offered a job, and may or may not accept and report to the job. Nevertheless, all interviewed workers are compared with workers who tried to get an interview but did not.

This estimator depends for its appeal on the following logic. Suppose two similar workers are suited to a given job vacancy and are both referred to that vacancy. One follows up immediately, gets the interview, gets the job offer, and fills the job vacancy. The other waits a day or two and learns that the job has been filled when he or she calls for an interview. If the logic holds, then this estimator mimics random assignment of workers who come to the ES for referrals—the interviewed workers are analogous to a group of workers randomly assigned to an interview, whereas the workers not interviewed are analogous to a randomly assigned control group.

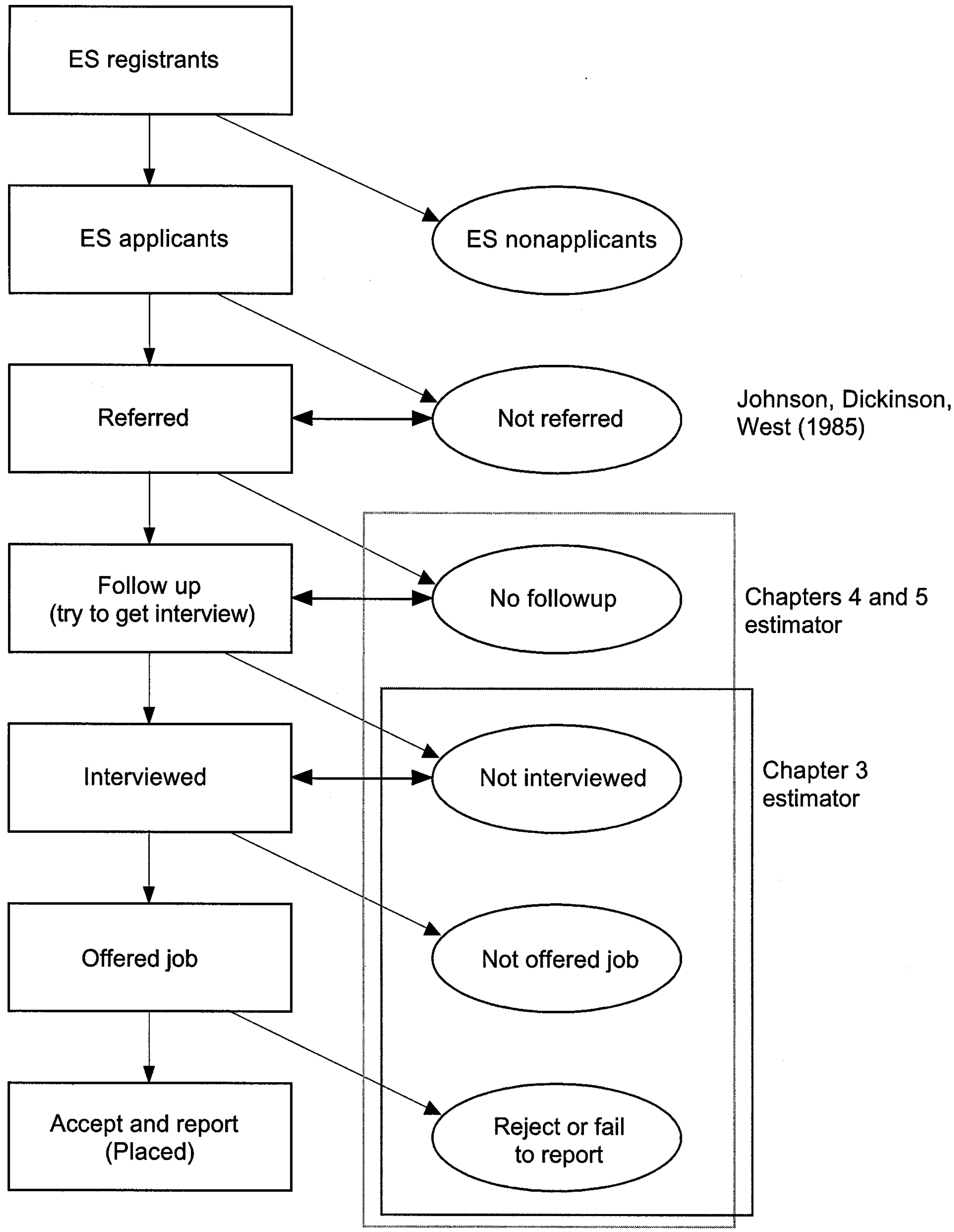

Figure 8-1. Flow Diagram of Employment Service Participation, with Estimators of the Impact of ES Activities Illustrated

Accordingly, this is an appealing and original estimator. The authors discuss a possible weakness associated with this estimator. If a worker does not get an interview because she is, in effect, screened-out by the employer when she calls for an interview, then the comparison between interviewed and not interviewed workers is no longer clean because the two workers were (apparently) not similar after all. Rather, the worker who didn't get the interview was rejected based on information obtained over the telephone. Jacobson argues correctly that this possibility could be tested by a followup interview in which workers would be asked why they did not obtain an interview. This is a potentially important approach that should be followed up.

8.4.4.2 Chapters 4 and 5

The estimator used in Chapters 4 and 5 can be thought of in either of two ways. First, it can be thought of as a comparison between placed workers (that is, the bottom left box in Figure 8-1) with the sum of workers who were referred but ultimately were not placed (that is, the sum of workers in the bottom four ovals, which are enclosed by the dashed rectangle labeled "Chapters 4 and 5 estimator"). This latter group is the sum of referred workers who (1) did not follow up on a referral, (2) followed up but did not get an interview, (3) got an interview but were not offered a job, and (4) were offered a job but rejected the offer or did not report.

Alternatively, the estimator used in Chapters 4 and 5 can be thought of as a comparison between workers who followed up on a referral and workers who did not follow up, adjusted for "participation." Referring to Figure 1, the comparison would be across the fourth row from the bottom, labeled "Chapters 4 and 5 estimator."

8.4.4.3 Results

The estimates of the impact of the PLX presented in Chapter 3 suggest that PLX placements have a differential impact on the unemployment spell duration of workers with strong and weak work records. For workers with strong records, spell durations are reduced on the order of 7 weeks, whereas for workers with weak work records, the reduction is somewhat over 3 weeks. Although higher than might be expected a priori, these are the most convincing existing estimates of the impact of the PLX, in my view.

8.4.4.4 Importance of Followup Work

For at least three reasons, this study makes clear the importance of further work on the impact of the PLX. First, the survey approach piloted in this study (and reported in Chapter 3) is extremely promising and deserves to be expanded to other states, in sample size, and in the intensity of followup. Second, it would be highly useful to supplement the survey of applicants with a survey of employers that would obtain data on the circumstances under which an applicant had not obtained a job offer. Third, given the growing importance of self-referrals, the effectiveness of self-referrals needs to be monitored and evaluated. Because it tracks self-referrals, Oregon appears to be a natural state in which to examine and evaluate self-referrals.

8.4.4.5 Responses to Specific Questions

A letter of July 24 requested responses to five specific questions, as follows:

Q1. Would it be a serious problem if 30 to 50 percent of referrals and placements could not be tracked because applicants have direct access to job contact information?

A1. The answer is clearly yes because if this were the case, there would be no way of detecting which applicants made use of PLX services.

Q2. Does it make sense to track only staff-assisted services and not track self-service assistance?

A2. No, because increasingly under WIA, the services offered to PLX applicants will be not be staff assisted. To ignore these would be to ignore a growing proportion of the services and assistance offered by PLXs.

Q3. Would it be useful to have information about how benefits are divided among (a) placement effects and referral effects, (b) services delivered early in a claimant's unemployment spell versus late in a spell, and (c) services delivered to claimants and others with strong work histories versus those with weak histories?

A3. Yes to all. Interpreting the impact of placements versus referrals is intrinsically interesting and can give insight into how information and matching technologies work. Linking the timing of services to program outcomes can tell programs operators when it is most effective to deliver services. Linking the characteristics of applicants to program outcomes can tell programs operators which applicants are best served.

Q4. **Would the use of the WIA entered-employment and similar measures create incentives to cream; that is, to provide services to those who are most likely to find jobs on their own?**

A4. Yes, clearly, value-added measures would be superior.

Q5. **Now that Oregon has the capacity to create the measures presented in chapters 4 and 5, would you recommend that Oregon use those measures instead of the entered-employment rate and similar measures?**

A5. Are the measures presented in the report substantially better that WIA performance measures like the entered employment rate? Yes in both cases.

8.5 Summary of the Comments of All Four Panelists

This section summarizes the comments of the expert panel in each of six areas:

1. Use of a natural experiment to measure placement effects;
2. Use of nonexperimental methods to measure placement and referral effects;
3. Estimation of displacement effects;
4. Overall benefit-cost computations;
5. Use of the information to improve performance measures; and
6. Tone of the report.

As noted earlier, most of the comments discussed in the section are presented in Section 8.4, but some come from written comments not intended for publication, and some from conversations with the experts.

For the most part there was broad agreement among the panel members. In a few cases there was some disagreement, and not all panelists discussed every issue. Thus, Westat asked the panelists to review this summary to make sure that it accurately reflects their views.

One preliminary point of importance, all the panelists felt that the term “accurate” needs to be carefully defined. In this report an accurate estimator is one that produces unbiased estimates that have small confidence intervals. In other words, if we used the estimator on repeated trials the difference between the “true” effect and the “measured” effect would be small and randomly distributed around the “true” effect.

8.5.1 Use of a Natural Experiment to Measure Placement Effects

All the panelists felt that the identification of referrals made to “stale” listings (those where the jobs or interview slots were already filled) could provide unbiased estimates of placement effects based on a natural experiment. They agreed that this highly innovative approach provided a means to at least partially deal with an inability to implement experimental designs that, in the past, made it extremely difficult to obtain acceptable estimates of the value of PLX direct placement services.

They also agreed that the study’s results should be regarded as illustrative of the method, rather than being close to the “true” value of the effects being measured. Direct confirmation was needed to demonstrate that “disappointed referees” (those job seekers who tried, but failed to obtain interviews) could not secure interviews because the listings were stale, rather than they were screened out by employers. Several reviewers felt that surveying employers would provide the best evidence about the extent to which referrals were stale, but evidence from job seekers also would be useful.

Also of great importance was surveying a large, representative sample of referred clients to obtain results that would reflect the average placement-effect. The panelists agreed that a comparison group of disappointed job-seekers referred to stale listing could be used to measure the effect of placements to **those listings**. However, some panelists noted that there would be cases where placement effects would be difficult to measure because no disappointed job seekers could be identified. They felt that determining the extent to which results can be generalized hinges on how different are the job openings for which there is, and there is not, a comparison group of disappointed job seekers.

A final problem that required further analysis was how to treat disappointed job seekers who subsequently were placed at jobs to which they were referred. No specific recommendations on how to deal with the problem were made, but it seems reasonable to assess how various restrictions on comparison group measurement affect the results.

8.5.2 Use of Nonexperimental Methods to Measure Placement and Referral Effects

The panelists gave much less attention to the nonexperimental evidence than the potential natural experiment. In part this was because the panelists felt that they should defer commenting on the validity of the nonexperimental placement estimates until accurate benchmark estimates from the natural experiment were obtained. Importantly, the panelists felt that obtaining benchmark estimates based on natural experiments from at least four states would be needed to assess if a nonexperimental technique would produce accurate estimates across all states.

The panelists also agreed that experimental evidence on the validity of the PLX referral estimates could not be obtained. One panelist felt that the biases in the nonexperimental referral-effect estimates could be relatively small because: (1) there was an usually large amount of information available to hold observable differences constant; (2) use of the duration of unemployment may have held constant most of the non-observable differences; and (3) the selection bias that tended to lead to underestimation during the first 10 weeks was balanced by selection bias that tended to lead to overestimation in the subsequent 16 weeks.

Two other panelists felt that the best evidence to judge referral effects is comparing the estimates in this report to experimental estimates about various types of job search assistance in situations were services could be withheld. Using that approach they concluded that the Washington referral estimates were more than twice as great as the experimental estimates for programs they felt provided services with similar effects.

However, it is possible that the experimental results were smaller because the services were delivered at the point claimants had been unemployed for short periods. Also, the experimental results may have been smaller because the services were not comparable. The panel may not have appreciated the total amount of time job seekers spent using PLX services nor the duration of the assistance provided. The inclusion of a table showing that placements often occur after clients obtained a series of referrals over many weeks clearly was insufficient. Thus, it would be useful to develop much more precise information about the nature of the PLX services received by study members as part of future work.

The above discussion suggests that a practical way to further assess the accuracy of referral estimates is to carefully review the existing literature on job search assistance to analyze: (1) differences in the treatments to determine if use of PLXs is likely to have larger effects than the treatments studied; (2) whether timing of the treatments differs in a way that would affect outcomes, and if possible; (3) determine what were the magnitude and direction of selection and other biases in the experimental results relative to nonexperimental estimates. This last piece of evidence could be used to assess whether the adjustments made using the nonexperimental estimators were similar to those made using experimental estimators.

Finally, the panelists felt that the report would greatly benefit from better explanation of why an unorthodox approach was used; that is, why placement effects were estimated relative to obtaining referrals but not being placed, and why referrals not leading to placement effects were estimated relative to not obtaining referrals (and usually not obtaining any PLX service). In contrast, the conventional approach would be to estimate the effect of receiving PLX placement services versus not receiving those services.

The answer is that the conventional estimator can be decomposed into three components: (1) placement effect, (2) referral effect, and (3) looking effect. The total benefits to job seekers equals the sum of the number of individuals in each category times the average effect of each type. The above decomposition allows use of the natural experiment discussed in Chapter 3 as a benchmark for assessing the accuracy of the placement effect estimates. Experimental evidence about similar treatments can help determine if the referral effect estimates are accurate. However, the data used in the study precluded examining looking effects—comparing those who tried to get a referral, but were unable to do so, to job seekers who did not use PLX job listings at all. Fortunately, since this last effect is likely to be smallest of all, and its omission probably leads to only a small underestimate.

8.5.3 Estimation of Displacement Effects

All the panelists were very pleased to see that an attempt was made to assess "crowding-out" or "displacement" effects—the adverse effects on non-users resulting from those receiving PLX aid more effectively competing for jobs. There was general agreement that the results were based on the best available methods. However, using those methods require considerably care because the results can be highly sensitive to the assumptions made.

One panelist felt that a fuller discussion of the limitations of the method should be included. Another felt that more should have been made of the single result showing that the crowding-out effects could be large in periods where jobs were scarce, and there should have been more discussion of how the results cited in this report compared to those obtained in other studies.

8.5.4 Overall Benefit-cost Computations

The expert panel had a lot to say about the benefit-cost analysis. First, they all seemed pleased that the report made it clear that looking at the benefits versus the costs was the appropriate framework for accessing the value of any government program. Second, they felt that more should have been said about:

- What benefits should be included in a comprehensive study versus what benefits are actually included.
- What are the benefits and costs from the point of view of society, taxpayers, and direct beneficiaries.

Third, they felt that the benefit-cost analysis was not sufficiently refined for use in making policy decisions because the basic estimates of benefits required executing the additional work discussed in Sections 8.5.1 and 8.5.2. In that case, the report's authors were surprised that so much attention was given to the conceptual framework for the benefit-cost analysis. However, these comments are highly relevant in providing guidance for the ongoing PLX study.

Interestingly, some experts focused on the importance of making clear that some benefits that should be included were omitted, while others focused more on how costs might be underestimated. Such differences may largely explain why the experts felt including a well-rounded discussion was so important to avoid creating the feeling that the report is biased either in favor or against showing that the program is cost effective.

8.5.5 Use of the Information to Improve Performance Measures

There was broad agreement that value-added measures of the type being developed in this report would provide highly useful information for assessing overall benefits and for making program changes that would increase the effectiveness of PLX services. There was equally broad agreement that the type of statistics traditionally reported are unsuitable for both of the above purposes.

The panelists were particularly enthusiastic about the prospect of quickly putting into use at least one accurate value-added measure based on evidence from the natural experiment. Several panelists felt that it should be highly feasible to create those measures on an ongoing bases, and agreed with the point made in the study, that ongoing estimation of the short-term gains would be a major improvement over existing measures. However, panelists also felt that it would be highly desirable to at least once conduct the research needed to determine how positive (or negative) were the long-term effects stemming from the length of time matches last, and long term changes in earnings.

They also felt that it was very important to have a comprehensive set of measures that was not limited to staff referrals that are currently tracked reasonably well, but also include self referrals that, currently, are only tracked in Oregon.

In general, the panel had no difficulty advocating use of performance measures once they were validated with experimental evidence. However, they had some reservations about advising program managers be provided with potentially flawed value-added measures instead of descriptive statistics that certainly are unsuitable for improving performance.

These reservations may stem from a lack of sufficient information to determine use of which measures will do less harm. It is the view of the report's authors that the additional analysis recommended by the expert panel would provide the evidence needed to take a more definitive position.

One final issue that was addressed by the experts is whether the new PLX measures would create incentives to cream—give staff help to clients likely to have the best outcomes, rather than those who can be helped the most—and whether such creaming would reduce effectiveness. The view was that use of measures like the entered-employment rate would create incentives to cream. However, evidence from JTPA programs suggests that creaming may not reduce program effectiveness because the treatments may have higher returns for eligibles with the best prospect to find work on their own.

Importantly, creaming incentives can be reduced by regression adjusting the descriptive statistics to take into account observable differences that affect the employment prospects of participants. These factors include personal characteristics, work histories, and labor market indicators. Also side constraints, such as requiring JTPA participants have multiple impediments to being reemployed, can limit incentives to cream. However, reducing creaming incentives is not at all the same as providing service providers with the information needed to improve the cost effectiveness of their programs. Only value-added measures can provide this information.

8.5.6 Tone of the Report

All the panelist felt that the report tended to be much too upbeat in claiming that the positive results obtained from the analysis accurately reflected the actual overall benefits for the groups studied. They similarly agreed that overstating the accuracy and applicability of the results made readers less likely to believe that PLXs were highly cost effective.

Since much of their discontent stemmed from statements in the executive summary, the authors thoroughly revised the summary to make it far clearer that: (1) we developed promising techniques that, when fully implemented, had the potentially to provide accurate estimates of placement effects, and (2) the preliminary results indicated that PLXs in Washington and Oregon were highly cost-effective, but those results should be considered as illustrative until additional analysis is completed.

Other changes were made throughout the report to try to correct the impression that point-estimates were highly accurate, and the benefit-cost analysis did more than provide evidence that because costs were low per person, relatively small per-person effects would lead to high benefit-cost ratios.

8.6 Implications for Future Analysis

In the view of the report's authors the expert panel did an outstanding job in pointing out the strengths and weaknesses of the report. Thus, the panelists made a major contribution to this project, and we are extremely grateful for their assistance.

In particular, they clarified the technical econometric issues that need to be addressed in order to produce results that experts would agree are accurate. They similarly clarified what basic factual information about PLX services is needed to provide experts with the background needed to place the econometric results into an appropriate context. Finally, they made some excellent suggestions about how to use the evidence about reductions in unemployment in producing benefit-cost estimates.

This report's authors concur with the expert panel's views about what issues need to be addressed and what additional work should be carried out. Specifically, we feel that the panel's findings suggest taking the following course of action:

1. Top priority should go to surveying employers listing jobs with PLXs to determine the extent to which referrals are made to stale listings and the disappointed referees are denied interviews based on information obtained by any type of prescreening.

2. Given that Step 1 confirms that many referrals are made to stale listings, the next step is to obtain survey evidence from a large, representative, sample of placed individuals and individuals referred to the same listings to which those individuals were referred who tried to obtain interviews, but were unable to do so because the jobs or interview slots were already filled.

3. These data should be used to estimate placement effects for sample members and assess the applicability of those results to placements made to job listings for which there were no disappointed referees to stale listings.

4. The natural experiment evidence should be used as a benchmark for assessing the accuracy of nonexperimental estimates of placement effects, and adjustments made in those techniques to improve the accuracy of the nonexperimental estimators.

5. As part of the survey work designed to identify a suitable comparison group, evidence should be gathered on the nature and extent of PLX services received by individuals who were referred but not placed by PLXs. Additional information also should be collected about how difficult it is to get to PLX offices and how much time is spent waiting to obtain assistance, as well as whether referees can figure out which listings are likely stale.

6. The above survey information as well as additional administrative data on dates listings are posted should be used as instrumental variables to see if the nonexperimental estimators can be improved.

7. The nonexperimental techniques should be used to examine the relationship between short-term reductions in unemployment and long-term gains in earnings and stability of job matches.

8. Existing evidence on the effectiveness of various forms of job search assistance based on random-assignment (experimental) designs should be reviewed to assess:

    a. How the attributes of services examined in those studies compares to the attributes of PLX services.

    b. How much bias there would be had conventional comparison groups and nonexperimental estimation techniques been used instead of control groups.

9. The above information should be used to assess whether the nonexperimental techniques used to estimate referral effects are in keeping with reasonable expectations, as well as whether the bias removed by those techniques is comparable to the bias removed by experimental techniques.

10. New estimates of the benefits to all job-seekers should be developed based on the above information. The estimates should include appropriate caveats about the accuracy of various results used, and include a range of estimates when that is appropriate.

11. New estimates of crowding-out effects should be produced based on the benefit estimates. The crowding-out estimates should be subjected to a wider range of sensitivity analyses and results compared to those of similar studies.

12. Estimates of the adverse effects of using payroll taxes to finance PLXs should be obtained from the existing literature.

13. Benefit-cost estimates should be developed from the above information which clearly describe what benefits are included and what are omitted. Estimates of the omitted benefits should be cited from existing literature, to the extent such estimates exist.

14. The above analysis should be conducted in at least four states with reasonably diverse PLXs to determine the extent to which the analytic methods and results can be generalized.

It is our view that following this outline will produce solid evidence about the overall effectiveness of PLX direct placement services that experts agree are accurate, and provide a means to provide policy-makers and program operators with information they can use to improve program effectiveness. Some attention also should be given to using random-assignment designs to assess the benefits of viewing listing as well as obtaining staff assistance by claimants who are required to participate in profiling and similar programs. Random-assignment designs also should be considered to assess the value of staff assistance for a broad range of PLX clients who would otherwise leave PLX offices without obtaining such aid.

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Appendix A

WASHINGTON STATE JOB SERVICE SURVEY

WIN A $100 GIFT CERTIFICATE*

I want my gift certificate from (select one):

- ☐ Wal-Mart
- ☐ Fred Meyer

*To enter drawing, simply fill out this survey and return it in the enclosed pre-paid envelope. You have a one in 100 chance of winning.

Please see inside front cover for details.

WIN A $100 GIFT CERTIFICATE!

We Really Want Your Opinion.

Recently, you may have been sent a survey asking for your opinion of the Washington State Job Service.* We didn't hear back from you, but we are still very interested in your opinion. We are Westat, an independent firm, which is conducting the survey for the Job Service.

Win a $100 Gift Certificate from Fred Meyer Stores or Wal-Mart!

Complete and mail back the enclosed survey with a postmarked date no later than **March 9, 1999** for a chance to win a $100 gift certificate. We will randomly draw **one out of every 100 responses.** Not only do you have great odds of winning, but you can choose either Wal-Mart or Fred Meyer Stores for your shopping! Simply **check off one of the boxes on the survey cover,** and tell us which gift certificate you want. There is no obligation for entering except to **complete the survey** and return it in the postage-paid envelope.

We Respect Your Privacy.

We guarantee you complete confidentiality. Your answers will have no effect on any benefits or services you receive from the Job Service.

You May Have These Questions:

If I win, when will I get my gift certificate? Winners will receive their gift certificates no later than April 9, 1999.

What is the KEY Job Service Referral? This is the job referral cited on the back of this survey.

What if I have a question? If you have any questions about the survey or the contest, contact Deborah Kitchell, at 1-800-WESTAT1 (ext. 2849). Thank you for your help on this important study and good luck!

* You also may have moved since visiting the Job Service and this is your first contact.

Q1. Your Name: ______________________ | ______________ | ________

Last First MI

Q2. The **company name** and **referral date** that appears on the back of this survey:

________________________________ _____ | _____ | 99

Company Name MO DAY YR

This referral will be called the KEY Job Service referral in the following questions.

Q3. How clear is your memory of receiving the KEY Job Service referral on the date specified?

- ☐ 1. Very clear
- ☐ 2. Clear
- ☐ 3. Somewhat hazy
- ☐ 4. Very hazy
- ☐ 5. No recollection at all *(CALL 1-800-WESTAT-1, x2849 for directions on what to do next)*

Questions 4 through 8 refer to your job search **before** you received your KEY Job Service referral.

Q4. How did you search for work:

A During the 4 weeks **before** your KEY Job Service referral.
B Before those 4 weeks but during the same job search?

(Check all that apply for each column)

	A In the 4 weeks before referral	**B** Before those 4 weeks
a. Contacted the Job Service.	☐	☐
b. Responded to want ads in a newspaper or periodical.	☐	☐
c. Responded to electronic want ads on the worldwide web or similar computerized system.	☐	☐
d. Made direct applications at work sites.	☐	☐
e. Sent resume to firms that did not advertise a vacancy.	☐	☐
f. Acted on leads from friends or relatives.	☐	☐
g. Used the services of a private employment agency.	☐	☐
h. Other: ______________________________	☐	☐

Q5. During this same job search how many visits did you make to Job Service offices before you received your KEY Job Service referral?

Number of Visits ____________________

Q6. Which of the following services did you use while visiting the Job Service? *(Check all that apply)*

- ☐ a. Job search workshops on job search strategies, resume writing, interview skills, etc.
- ☐ b. Resource room for pc, fax, phone use
- ☐ c. Assessment and counseling
- ☐ d. Referral to training
- ☐ e. Referral to other assistance/programs

Q7. How many different jobs did you apply for through the Job Service or any other means:

a. During the 4 weeks before your KEY Job Service referral

_______ Number of Jobs

b. Before those 4 weeks but during the same job search

_______ Number of Jobs

Q8. Before receiving your KEY Job Service referral:

a. How many job interviews were you offered?

_______ Number of Interviews Offered

b. How many job interviews did you have?

_______ Number of Interviews Completed

c. How many job offers resulted from these interviews?

_______ Number of Offers Received

d. How many of these job offers did you accept?

_______ Number of Offers Accepted

Q9. At the time you received your KEY Job Service referral, what was your employment status?

- ☐ 1. Working, looking for a NEW job *(GO TO Q11)*
- ☐ 2. Working, looking for a second job *(GO TO Q11)*
- ☐ 3. Laid-off, not expecting recall
- ☐ 4. Laid-off, expecting recall
- ☐ 5. Not working, unemployed less than 6 months
- ☐ 6. Not working, unemployed for more than 6 months

Q10. At the time you received your KEY Job Service referral, how many weeks were you without paid work?

_______ Weeks ☐ Check here if less than 1 week

Q11. At the time you received your KEY Job Service referral, how many weeks had you been looking for a new employer?

_______ Weeks ☐ Check here if less than 1 week

Q12. How did you obtain your KEY Job Service referral?

- ☐ 1. Viewed computerized listing at a Job Service office
- ☐ 2. Viewed computerized listing at library or other public place
- ☐ 3. Viewed computerized listing from home pc
- ☐ 4. Received a call from the Job Service
- ☐ 5. Called the Job Service 800 number

Q13. Did you accept a job offer as a result of the KEY Job Service referral?

- ☐ 1. Yes *(GO TO Q23)*
- ☐ 2. No

Q14. Did you followup the KEY Job Service referral by trying to arrange a job interview?

- ☐ 1. Yes, and had the interview *(GO TO Q16)*
- ☐ 2. Yes, but did not have the interview *(i.e., the job was already filled) (GO TO Q26)*
- ☐ 3. No

Q15. How important were the following factors in deciding not to interview following your KEY Job Service referral?

	Very	Somewhat	Slightly	Not at all
a. I had the chance to interview for a better job	1	2	3	4
b. The type of work was not suitable	1	2	3	4
c. The pay was too low	1	2	3	4
d. The commute was too difficult	1	2	3	4
e. The working conditions were not desirable	1	2	3	4

Go to Q26 if you did not interview for the job.

Q16. Did you receive a job offer from the firm you interviewed with?

☐ 1. Yes *(GO TO Q20)*

☐ 2. No

Q17. Which of the following characteristics of this job were acceptable to you? (Check all that apply)

☐ a. Type of work

☐ b. Pay package

☐ c. Working conditions

Q18. At the time you interviewed, did you have an offer or potential offer for another job you preferred?

☐ 1. Yes

☐ 2. No

Q19. Which of the following factors do you believe contributed to your not receiving a job offer? (Check all that apply)

☐ a. I did not have the skills and experience the employer needed.

☐ b. The employer already filled the job.

☐ c. The employer preferred another candidate.

☐ d. I did not interview well.

☐ e. I indicated that I was not enthusiastic about taking this job.

☐ f. Other:______________________________________

Go to Q26 if you did not receive a job offer.

Q20. Did you accept the job offer?

☐ 1. Yes *(GO TO Q23)*

☐ 2. No

Q21. Which statement best describes why you decided not to accept the job offer? *(Check one)*

☐ 1. I found a better job.

☐ 2. I decided not to work and stopped looking.

☐ 3. I decided that I could find a better job by continuing to search.

Q22. Which of the following led to your decision not to accept the job offer? *(Check all that apply)*

☐ a. I didn't like the type of work.

☐ b. The pay was too low.

☐ c. Too few hours per week were offered.

☐ d. Too many hours per week were expected.

☐ e. The work schedule was inconvenient.

☐ f. I didn't like the working conditions.

☐ g. The commute was too long or inconvenient.

☐ h. Other: ______________________________

Go to Q26, if you did not accept the job offer.

Q23. Did you report for work after accepting the job?

☐ 1. Yes

☐ 2. No *(GO TO Q26)*

Q24. Are you still working for this employer?

☐ 1. Yes *(GO TO Q28)*

☐ 2. No

Q25. How long did you work for this employer?

_______ Months ☐ Check here if less than 1 month *(GO TO Q28)*

Questions 26, 27 and 28 refer to the period of time **after** you received your KEY Job Service referral.

Q26. **After** you received your KEY Job Service referral, how many weeks did you remain without **new** paid employment?

_______ Weeks ☐ Check here if less than 1 week

☐ Check here if you are still unemployed or looking for a **NEW** job *(GO TO Q29)*

☐ Check here if you are employed but did not find a second job *(GO TO Q29)*

Q27. How did you find the first job you accepted **after** you received your KEY Job Service referral? (Check only one)

☐ 1. Referral by the Job Service
☐ 2. Responded to a want ad in a newspaper or periodical
☐ 3. Responded to an electronic want ad on the worldwide web or similar computerized system
☐ 4. Made direct applications at a work site
☐ 5. Sent resume or inquires to a firm that did not advertise a vacancy
☐ 6. Acted on a lead from friends or relatives
☐ 7. Used the services of a private employment agency
☐ 8. Other: ________________________________

Q28. How **satisfied** were/are you with that job?

☐ 1. Very satisfied
☐ 2. Somewhat satisfied
☐ 3. Slightly satisfied
☐ 4. Not at all satisfied
☐ 5. Haven't started the job

Q29. How useful was the Job Service in helping you find a job (even if you did not obtain a job from a Job Service referral)?

☐ 1. Very useful
☐ 2. Somewhat useful
☐ 3. Slightly useful
☐ 4. Not at all useful

Q30. How **satisfied** were you with the following help you received from the Job Service?

	Very	Somewhat	Slightly	Not at all	Not observed
a. Number of jobs listed	1	2	3	4	0
b. Jobs listed that matched your needs	1	2	3	4	0
c. How current job listing was	1	2	3	4	0
d. Usefulness of job referrals	1	2	3	4	0
e. Ease of using computers to get information	1	2	3	4	0
f. Speed of getting help from staff	1	2	3	4	0
g. Usefulness of help from staff	1	2	3	4	0
h. Ease of travel to Job Service office	1	2	3	4	0
i. Usefulness of mandatory workshop sessions	1	2	3	4	0
j. Usefulness of voluntary workshop sessions	1	2	3	4	0
k. Usefulness of counseling sessions	1	2	3	4	0
l. Usefulness of training information	1	2	3	4	0
m. Usefulness of counseling information	1	2	3	4	0
n. Usefulness of information about other services	1	2	3	4	0

Q31. How **important** is it for the Job Service to improve each of the following features to better serve job seekers like yourself?

	Very	Somewhat	Slightly	Not at all	Not observed
a. Number of jobs listed	1	2	3	4	0
b. Jobs listed that matched your needs	1	2	3	4	0
c. How current job listing was	1	2	3	4	0
d. Usefulness of job referrals	1	2	3	4	0
e. Ease of using computers to get information	1	2	3	4	0
f. Speed of getting help from staff	1	2	3	4	0
g. Usefulness of help from staff	1	2	3	4	0
h. Ease of travel to Job Service office	1	2	3	4	0
i. Usefulness of mandatory workshop sessions	1	2	3	4	0
j. Usefulness of voluntary workshop sessions	1	2	3	4	0
k. Usefulness of counseling sessions	1	2	3	4	0
l. Usefulness of training information	1	2	3	4	0
m. Usefulness of counseling information	1	2	3	4	0
n. Usefulness of information about other services	1	2	3	4	0

Q32. Of all the features listed in **Q31,** which do you think is the most important for the Job Service to improve? *(Circle one letter)*

a b c d e f g h i j k l m n

Q33. If you were looking for work in the future, would you use the Job Service?

☐ 1. Yes

☐ 2. No

Q34. At the time you received your KEY Job Service referral:

a. Excluding yourself, how many other persons lived in your household?

_______Persons ☐ No one else in household

b. How many of these persons earned more than $5,000 during the 12 months **before** your KEY Job Service referral?

_______Persons ☐ No one else in household

Q35. What was the total income for your household (including your income) for the 12-month period **before** your KEY Job Service referral?

☐ 1. Less than $10,000

☐ 2. $10,000 to $39,000

☐ 3. $40,000 or more

Q36. Please tell us more about your experience using the Job Service and how the Job Service could improve. Enter your comments here and use the back of the booklet if you need more space.

____

____

____

____

____

Thank You

Please place this questionnaire in the postage-paid envelope provided and mail to:

WESTAT

1650 Research Blvd., Room RP 4017

Rockville, MD 20850

Call 1-800-937-8281 (ext. 2849) if you have any questions about this survey.

Appendix B

APPENDIX B

Appendix B-1 is a description of the variables used for the analysis of the Washington State Mail Survey. The variables derived from the mail survey are listed first followed by the variables derived from the UI and ES administrative data. The number of observations, mean, and standard deviation are included for each variable.

Appendix B-2 presents the full OLS regression for the Washington State Mail Survey with the dependent variable being the number of weeks the respondent was unemployed following receipt of a referral. The placement and reasons for not being placed coefficients appear in column 3 of Table 3-6. The regression uses most of the variables listed in Appendix B-1 as independent variables. For each variable the parameter estimate, standard error, and the *p*-value are listed to the right of the variable description. The *p*-value refers to the level of significance for the parameter estimate and *p*-values less than .05 are usually considered statistically significant.

Appendix B-1. Description of Variables Used in the WA Mail Survey

Mail Survey Variables

Variable	Description	N	Mean	Std Dev
Q3_1	Recall of Referral: Very Clear	587	0.382	0.486
Q3_2	Recall of Referral: Clear	587	0.431	0.496
Q3_3	Recall of Referral: Somewhat Hazy	587	0.187	0.391
Q4BA	Responded to Want Ads: < 4 Weeks Before Referral	587	0.681	0.466
Q4BB	Responded to Want Ads: 4-8 Weeks Before Referral	587	0.579	0.494
Q4CA	Responded to E-want Ads: < 4 Weeks Before Referral	587	0.235	0.424
Q4CB	Responded to E-want Ads: 4-8 Weeks Before Referral	587	0.193	0.395
Q4DA	Direct Application: < 4 Weeks Before Referral	587	0.627	0.484
Q4DB	Direct Application: 4-8 Weeks Before Referral	587	0.501	0.500
Q4EA	Sent Unsolicited Resume: < 4 Weeks Before Referral	587	0.296	0.457
Q4EB	Sent Unsolicited Resume: 4-8 Weeks Before Referral	587	0.257	0.437
Q4FA	Acted on Leads from Friends: < 4 Weeks Before Referral	587	0.555	0.497
Q4FB	Acted on Leads from Friends: 4-8 Weeks Before Referral	587	0.479	0.500
Q4GA	Private Employment Agency: < 4 Weeks Before Referral	587	0.215	0.411
Q4GB	Private Employment Agency: 4-8 Weeks Before Referral	587	0.174	0.379
Q4HA	Other Search Method: < 4 Weeks Before Referral	587	0.065	0.246
Q4HB	Other Search Method: 4-8 Weeks Before Referral	587	0.083	0.277
Q5	Number of Visits to Job Service Before Referral	587	5.874	20.044
Q5_MIS	Missing: Number of Visits to Job Service Before Referral	587	0.147	0.354
Q6A	Services Used: Job Search Workshops	587	0.349	0.477
Q6B	Services Used: Resource Room	587	0.378	0.485
Q6C	Services Used: Assessment and Counseling	587	0.249	0.433
Q6D	Services Used: Referral to Training	587	0.162	0.369
Q6E	Services Used: Referral to Other Assistance	587	0.211	0.409
Q7A	Number of Jobs Applied for: < 4 Weeks Before Referral	587	5.790	8.684
Q7A_MIS	Missing: Number of Jobs Applied for: < 4 Weeks Before Referral	587	0.153	0.361
Q7B	Number of Jobs Applied for: 4-8 Weeks Before Referral	587	5.080	9.922
Q7B_MIS	Missing: Number of Jobs Applied for: 4-8 Weeks Before Referral	587	0.279	0.449
Q8A	Number of Interviews Offered Before Referral	587	2.005	2.824
Q8A_MIS	Missing: Number of Interviews Offered Before Referral	587	0.082	0.274
Q8B	Number of Interviews Completed Before Referral	587	1.997	2.903
Q8B_MIS	Missing: Number of Interviews Completed Before Referral	587	0.080	0.272
Q8C	Number of Job Offers Received Before Referral	587	0.637	0.929
Q8C_MIS	Missing: Number of Job Offers Received Before Referral	587	0.068	0.252
Q8D	Number of Job Offers Accepted Before Referral	587	0.484	0.863
Q8D_MIS	Missing: Number of Job Offers Accepted Before Referral	587	0.126	0.332
Q9_NEW	Employment Status: Working, Looking for a New Job	587	0.136	0.343
Q9_2JB	Employment Status: Working, Looking for a Second Job	587	0.032	0.177

Variable	Description	N	Mean	Std Dev
Q9_REC	Employment Status: Laid-off	587	0.061	0.240
Q9_NR6	Employment Status: Not Working Less than Six Months	587	0.494	0.500
Q9_GT6	Employment Status: Not Working More than Six Months	587	0.227	0.419
Q10	Number of Weeks without Paid Work	587	10.893	28.729
Q10_MIS	Missing: Number of Weeks without Paid Work	587	0.325	0.469
Q11	Number of Weeks Looking for New Employer	587	7.753	11.443
Q11_MIS	Missing: Number of Weeks Looking for New Employer	587	0.233	0.423
Q12_NORS	How Referral Received: No Response	587	0.126	0.332
Q12_1	How Referral Received: At Job Service Office	587	0.659	0.474
Q12_2	How Referral Received: At Library or Public Place	587	0.026	0.158
Q12_3	How Referral Received: At Home	587	0.017	0.130
Q12_4	How Referral Received: Call from Job Service Office	587	0.119	0.324
Q12_5	How Referral Received: Called Job Service 800 Number	587	0.053	0.224
Q34A1	Number of Persons in Household	587	2.206	1.745
Q34A2	No Other Persons in Household	587	0.145	0.352
Q34B1	Number of Persons Earning More than $5,000	587	0.671	0.801
Q34B2	No Other Persons Earning More than $5,000	587	0.288	0.457
Q35_10K	Total Household Income: Less than $10,000	587	0.378	0.485
Q35_39K	Total Household Income: Between $10,000 and $39,000	587	0.438	0.497
Q35_40K	Total Household Income: More than $40,000	587	0.138	0.345
Q35_MIS	Total Household Income: Missing	587	0.000	0.000
	Administrative Variables			
AGE	Age (Years)	587	36.083	11.205
AGE25	Age Less than 25	587	0.201	0.401
AGE40	Age Between 25 and 40	587	0.451	0.498
AGE54	Age Between 41 and 54	587	0.267	0.443
AGE55	Age Greater than 55	587	0.058	0.234
AGRI	Industry: Agriculture	587	0.051	0.220
AREOSP	Industry: Aerospace	587	0.015	0.123
AVG97	Average Quarterly Earnings in 1997	587	2297.730	2585.330
COLLEGE	Education: College Degree	587	0.015	0.123
EAST	East Washington State	587	0.358	0.480
ECON_DS1	Program Participant: Economically Disadvantaged	587	0.305	0.461
EXP6M	Months of Experience Required: 6	587	0.232	0.422
EXP12M	Months of Experience Required: 12	587	0.220	0.414
EXP24M	Months of Experience Required: 24	587	0.082	0.274
EXP36M	Months of Experience Required: 36	587	0.043	0.202
EXPMISS	Months of Experience Required: Missing	587	0.000	0.000
EXPNONE	Months of Experience Required: None	587	0.424	0.495
FANCY	Mail Survey Package: Fancy	587	0.562	0.497

Variable	Description	N	Mean	Std Dev
FEMALE	Gender: Female	587	0.475	0.500
FOODSTM1	Program Participant: Food Stamp	587	0.014	0.116
FULLTIME	Hours Required on Job: 40 or More	587	0.670	0.471
GRAD	Education: Graduate Degree	587	0.138	0.345
HDCP_DS1	Program Participant: Handicapped/Disabled	587	0.034	0.182
HIGH	Education: High School Degree	587	0.394	0.489
HIGHWAGE	Minimum Acceptable Wage: $13.21 or Higher	587	0.009	0.092
HIHR_97	Hours Worked in the Highest Earning Quarter in 1997	587	249.104	231.272
HITEN_97	Longest Tenure in the Highest Earning Quarter in 1997	587	1.821	1.373
HI_97	Highest Earning Quarter in 1997	587	2985.480	3404.560
HRHIWA97	Missing: Hourly Wage of the Highest Earning Quarter in 1997	587	9.262	48.785
HSRQ	Job Requires High School Degree	587	0.390	0.488
IN_SCHO1	Attending School	587	0.048	0.213
LESSHIGH	Education: Less than High School	587	0.143	0.350
LESSHSRQ	Job Requires Less than High School Degree	587	0.581	0.494
LOWWAGE	Minimum Acceptable Wage: $6.59 or Less	587	0.138	0.345
LO_97	Lowest Earning Quarter in 1997	587	1318.550	2025.480
MAEQUA18	Minimum Age Required: 18 Years	587	0.790	0.407
MAGREA18	Minimum Age Required: More than 18 Years	587	0.058	0.234
MALESS18	Minimum Age Required: Less than 18 Years	587	0.152	0.359
MANU	Industry: Manufacturing	587	0.135	0.342
MAVG97	Missing: Average 1997 Earnings	587	0.239	0.427
MHIHR_97	Missing: Highest Earning Quarter in 1997	587	0.239	0.427
MHRHIWA9	Missing: Hourly Wage of Highest Earning Quarter in 1997	587	0.308	0.462
MIDWAGE	Minimum Acceptable Wage: Between $6.60 and $13.20	587	0.070	0.255
MINMISS	Minimum Wage Required Not Listed	587	0.775	0.418
MINNONE	No Minimum Wage Required	587	0.009	0.092
MIN_AGE1	Minimum Age Required (Years)	587	15.693	6.286
MISS_ED	Missing: Education Category	587	0.085	0.279
MMINWAGE	Missing: Minimum Wage Required	587	0.775	0.418
MONEY	Mail Survey Package: Extra Money	587	0.080	0.272
MOS_EXP1	Months of Experience Required	587	7.876	11.802
MOREHSRQ	Job Requires More than High School Degree	587	0.029	0.168
MRADIVHI	Missing: Range in 1997/Highest Earning Quarter in 1997	587	0.239	0.427
MSALLES5	Missing: Salary Reported < 50	587	0.065	0.246
MSALGRT5	Missing: Salary Reported > 50	587	0.935	0.246
MSFW1	Program Participant: Migrant and Seasonal Farm Worker	587	0.015	0.123
NOMINAGE	No Minimum Age Required	587	0.136	0.343
NW	Northwest Washington State	587	0.201	0.401
OCC_0	Occupational Classification: First Digit 0	587	0.027	0.163
OCC_1	Occupational Classification: First Digit 1	587	0.003	0.058

Variable	Description	N	Mean	Std Dev
OCC_2	Occupational Classification: First Digit 2	587	0.000	0.000
OCC_3	Occupational Classification: First Digit 3	587	0.049	0.217
OCC_4	Occupational Classification: First Digit 4	587	0.153	0.361
OCC_5	Occupational Classification: First Digit 5	587	0.337	0.473
OCC_6	Occupational Classification: First Digit 6	587	0.118	0.322
OCC_7	Occupational Classification: First Digit 7	587	0.128	0.334
OCC_8	Occupational Classification: First Digit 8	587	0.080	0.272
OCC_9	Occupational Classification: First Digit 9	587	0.104	0.305
OPEN1	Number of Openings for Job Order: 1	587	0.618	0.486
OPEN2	Number of Openings for Job Order: 2	587	0.189	0.392
OPEN3	Number of Openings for Job Order: 3	587	0.053	0.224
OPEN4	Number of Openings for Job Order: 4	587	0.015	0.123
OPEN5	Number of Openings for Job Order: 5	587	0.027	0.163
OPEN6	Number of Openings for Job Order: 6	587	0.097	0.296
OPENING1	Number of Openings for Job Order	587	4.124	14.619
OUTC_PLC	Placed at the Job Interviewed	587	0.562	0.497
OUTC_NSH	Accepted Offer, Did not Show Up for Work	587	0.024	0.153
OUTC_NAC	Did not Accept Offer After Interviewing	587	0.029	0.168
OUTC_NOF	Did not Obtain Offer After Interviewing	587	0.063	0.243
OUTC_NOT	Tried, but Failed to Obtain an Interview	587	0.145	0.352
OUTC_TRY	Did not Try to Obtain Interview	587	0.177	0.382
OUTSTATE	Outside of Washington State	587	0.009	0.092
PARTTIME	Hours Required at Job: < 40	587	0.330	0.471
PLAIN	Mail Survey Package: Plain	587	0.358	0.480
QREF_981	Referral Received in Q1 1998	587	0.458	0.499
QREF_982	Referral Received in Q2 1998	587	0.542	0.499
Q_97_0	Number of Non-zero Quarters in 1997: 0	587	0.239	0.427
Q_97_1	Number of Non-zero Quarters in 1997: 1	587	0.095	0.294
Q_97_2	Number of Non-zero Quarters in 1997: 2	587	0.138	0.345
Q_97_3	Number of Non-zero Quarters in 1997: 3	587	0.174	0.379
Q_97_4	Number of Non-zero Quarters in 1997: 4	587	0.354	0.479
RADIVHI	Range in 1997 Divided by Highest Earning Quarter in 1997	587	0.406	0.363
RANGE97	Highest Earning Quarter in 1997 - Lowest Earning Quarter	587	1666.930	2363.150
REF1PLC1	Job Order Has One Referral and One Placement	587	0.107	0.310
REF1PLC2	Job Order Has One Referral and Two Placements	587	0.034	0.182
REF1PLC3	Job Order Has One Referral and Three Placements	587	0.003	0.058
REF2PLC1	Job Order Has Two Referrals and One Placement	587	0.099	0.299
REF2PLC2	Job Order Has Two Referrals and Two Placements	587	0.036	0.186
REF2PLC3	Job Order Has Two Referrals and Three Placements	587	0.000	0.000
REF3PLC1	Job Order Has Three Referrals and One Placement	587	0.465	0.499
REF3PLC2	Job Order Has Three Referrals and Two Placements	587	0.164	0.370

Variable	Description	N	Mean	Std Dev
REF3PLC3	Job Order Has Three Referrals and Three Placements	587	0.092	0.289
RELOCAT1	Willing to Relocate	587	0.240	0.428
RESP	Response to Mail Survey	587	1.000	0.000
RESP0	Response Type: Unidentified	587	0.271	0.445
RESP1	Response Type: Completed Survey	587	0.635	0.482
RESP2	Response Type: Returned Without Forwarding Information	587	0.005	0.071
RESP3	Response Type: Unidentified	587	0.089	0.284
SALARY1	Salary Reported	587	796.078	4564.220
SALGRT50	Salary Reported < 50 (Hourly Wage)	587	789.489	4565.360
SALLES50	Salary Reported > 50 (Weekly, Monthly, or Annual Wage)	587	6.589	3.417
SEATTLE	Job-seeker Located in Seattle, Washington	587	0.187	0.391
SIC_0	SIC Code: First Digit 0	587	0.051	0.220
SIC_1	SIC Code: First Digit 1	587	0.089	0.284
SIC_2	SIC Code: First Digit 2	587	0.066	0.249
SIC_3	SIC Code: First Digit 3	587	0.068	0.252
SIC_4	SIC Code: First Digit 4	587	0.066	0.249
SIC_5	SIC Code: First Digit 5	587	0.194	0.396
SIC_6	SIC Code: First Digit 6	587	0.039	0.194
SIC_7	SIC Code: First Digit 7	587	0.232	0.422
SIC_8	SIC Code: First Digit 8	587	0.162	0.369
SIC_9	SIC Code: First Digit 9	587	0.032	0.177
SOMECOLL	Education: Some College	587	0.225	0.418
SW	Southwest Washington State	587	0.245	0.431
TIMBER	Industry: Timber	587	0.005	0.071
UI_CLAI1	Program Participant: UI Claimant	587	0.417	0.494
WHITE	Race: White	587	0.835	0.372

Appendix B-2. Regression on Weeks of Unemployment for WA Mail Survey

Variable	Parameter Estimate	Standard Error	Prob > |T|
R-Squared	0.415		
Intercept	25.451	51.315	0.6202
Recall of Referral: Very Clear	-0.094	1.038	0.9282
Recall of Referral: Clear	-0.323	0.955	0.7356
Recall of Referral: Somewhat Hazy (Omitted)	--	--	--
Responded to Want Ads: < 4 Weeks Before Referral	0.540	0.851	0.5263
Responded to Want Ads: 4-8 Weeks Before Referral	-0.947	0.894	0.2902
Responded to E-want Ads: < 4 Weeks Before Referral	-0.200	0.907	0.8253
Responded to E-want Ads: 4-8 Weeks Before Referral	1.371	1.007	0.1739
Direct Application: < 4 Weeks Before Referral	-0.331	0.829	0.6903
Direct Application: 4-8 Weeks Before Referral	-0.286	0.903	0.7520
Sent Unsolicited Resume: < 4 Weeks Before Referral	-0.678	0.922	0.4627
Sent Unsolicited Resume: 4-8 Weeks Before Referral	-0.608	1.014	0.5491
Acted on Leads from Friends: < 4 Weeks Before Referral	-0.267	0.817	0.7435
Acted on Leads from Friends: 4-8 Weeks Before Referral	-0.481	0.922	0.6020
Private Employment Agency: < 4 Weeks Before Referral	-0.754	0.929	0.4175
Private Employment Agency: 4-8 Weeks Before Referral	0.867	1.009	0.3902
Other Search Method: < 4 Weeks Before Referral	-2.244	1.643	0.1727
Other Search Method: 4-8 Weeks Before Referral	3.433	1.463 **	0.0194
Number of Visits to Job Service Before Referral	0.005	0.018	0.7765
Missing: Number of Visits to Job Service Before Referral	2.296	1.086 **	0.0350
Services Used: Job Search Workshops	0.020	0.792	0.9798
Services Used: Resource Room	-0.648	0.781	0.4073
Services Used: Assessment and Counseling	-0.649	0.824	0.4315
Services Used: Referral to Training	0.203	1.040	0.8457
Services Used: Referral to Other Assistance	0.538	0.949	0.5713
Number of Jobs Applied for: < 4 Weeks Before Referral	0.116	0.060 *	0.0520
Missing: Number of Jobs Applied for: < 4 Weeks Before Referral	1.921	1.250	0.1250
Number of Jobs Applied for: 4-8 Weeks Before Referral	-0.097	0.052	0.0619
Missing: Number of Jobs Applied for: 4-8 Weeks Before Referral	-1.640	0.965 *	0.0899
Number of Interviews Offered Before Referral	-0.396	0.293	0.1760
Missing: Number of Interviews Offered Before Referral	-1.620	2.499	0.5170
Number of Interviews Completed Before Referral	0.568	0.291 *	0.0517
Missing: Number of Interviews Completed Before Referral	1.199	2.937	0.6832
Number of Job Offers Received Before Referral	-0.245	0.541	0.6500
Missing: Number of Job Offers Received Before Referral	-2.320	2.267	0.3067

Variable	Parameter Estimate	Standard Error		Prob > |T|
Number of Job Offers Accepted Before Referral	-0.021	0.554		0.9701
Missing: Number of Job Offers Accepted Before Referral	3.186	1.373	**	0.0208
Employment Status: Working, Looking for a New Job	1.430	1.966		0.4676
Employment Status: Working, Looking for a Second Job	1.943	2.551		0.4467
Employment Status: Laid-off	3.099	2.110		0.1425
Employment Status: Not Working Less than Six Months	2.281	1.655		0.1689
Employment Status: Not Working More than Six Months	4.694	1.753	***	0.0077
Number of Weeks without Paid Work	-0.013	0.014		0.3488
Missing: Number of Weeks without Paid Work	0.586	1.000		0.5583
Number of Weeks Looking for New Employer	0.038	0.036		0.2876
Missing: Number of Weeks Looking for New Employer	-2.803	0.976	***	0.0043
How Referral Received: No Response	-2.408	1.788		0.1789
How Referral Received: At Job Service Office	-2.513	1.547		0.1050
How Referral Received: At Library or Public Place	0.239	2.610		0.9272
How Referral Received: At Home	-5.308	3.374		0.1164
How Referral Received: Call from Job Service Office	-4.512	1.789	**	0.0120
How Referral Received: Called Job Service 800 Number (Omitted)	--	--		--
Number of Persons in Household	0.309	0.240		0.1986
No Other Persons in Household	0.555	1.264		0.6608
Number of Persons Earning More than $5,000	-0.286	0.591		0.6281
No Other Persons Earning More than $5,000	-1.024	0.965		0.2895
Total Household Income: Less than $10,000	0.670	1.680		0.6902
Total Household Income: Between $10,000 and $39,000	-0.626	1.659		0.7060
Total Household Income: More than $40,000	-0.943	1.923		0.6239
Total Household Income: Missing (Omitted)	--	--		--
Age (Years)	0.068	0.090		0.4535
Age Less than 25	-2.696	2.830		0.3414
Age Between 25 and 40	-4.128	2.377	*	0.0832
Age Between 41 and 54	-2.408	2.407		0.3176
Age Greater than 55	-2.456	3.157		0.4371
Industry: Agriculture	-1.254	2.745		0.6480
Industry: Aerospace	0.609	3.225		0.8502
Average Quarterly Earnings in 1997	-0.002	0.001	**	0.0250
Education: College Degree	-4.713	2.978		0.1143
East Washington State	0.358	0.927		0.6998
Program Participant: Economically Disadvantaged	1.642	0.935	*	0.0796
Months of Experience Required: 6	-0.370	1.861		0.8425
Months of Experience Required: 12	0.313	3.519		0.9291
Months of Experience Required: 24	-1.118	6.938		0.8720

Variable	Parameter Estimate	Standard Error		Prob > |T|
Months of Experience Required: 36	-0.713	1.135		0.5304
Months of Experience Required: Missing (Omitted)	--	--		--
Mail Survey Package: Fancy	-0.806	0.727		0.2684
Gender: Female	-0.687	0.882		0.4363
Program Participant: Food Stamp	9.004	3.126	***	0.0042
Hours Required on Job: 40 or More	0.619	0.826		0.4539
Education: Graduate Degree	-2.053	1.223	*	0.0940
Program Participant: Handicapped/Disabled	0.331	1.946		0.8651
Education: High School Degree	0.080	0.908		0.9295
Minimum Acceptable Wage: $13.20 or Higher	-13.610	11.110		0.2212
Hours Worked in the Highest Earning Quarter in 1997	0.000	0.003		0.9847
Longest Tenure in the Highest Earning Quarter in 1997	-0.250	0.628		0.6909
Highest Earning Quarter in 1997	0.000	0.000		0.3980
Missing: Hourly Wage of the Highest Earning Quarter in 1997	0.001	0.007		0.9369
Job Requires High School Degree	-2.340	2.439		0.3379
Attending School	-2.557	1.652		0.1225
Education: Less than High School	0.903	1.241		0.4670
Job Requires Less than High School Degree	-3.574	2.511		0.1553
Minimum Acceptable Wage: Less Than $6.60 and $13.20	-7.548	4.460		0.0913
Lowest Earning Quarter in 1997	0.002	0.001	**	0.0111
Minimum Age Required: 18 Years	0.666	6.974		0.9240
Minimum Age Required: More than 18 Years	0.097	16.418		0.9953
Minimum Age Required: Less than 18 Years (Omitted)	--	--		--
Industry: Manufacturing	2.800	2.581		0.2787
Minimum Acceptable Wage: Between $6.60 and $13.20	-10.817	5.332	**	0.0431
Minimum Wage Required Not Listed	-4.693	3.623		0.1959
No Minimum Wage Required (Omitted)	--	--		--
Minimum Age Required (Years)	-0.449	3.179		0.8877
Missing: Education Category	0.741	0.458		0.1063
Missing: Minimum Wage Required	-1.185	1.374		0.3891
Mail Survey Package: Extra Money	-1.140	1.354		0.4002
Months of Experience Required	0.020	0.135		0.8831
Job Requires More than High School Degree (Omitted)	--	--		--
Program Participant: Migrant and Seasonal Farm Worker	-1.199	3.245		0.7119
No Minimum Age Required	-7.529	50.908		0.8825
Northwest Washington State	2.291	1.048	**	0.0293
Number of Openings for Job Order: 1	2.462	2.661		0.3554
Number of Openings for Job Order: 2	0.399	2.492		0.8729
Number of Openings for Job Order: 3	0.562	2.834		0.8428

Variable	Parameter Estimate	Standard Error	Prob > |T|
Number of Openings for Job Order: 4	0.886	3.677	0.8097
Number of Openings for Job Order: 5	1.277	2.482	0.6072
Number of Openings for Job Order: 6 (Omitted)	--	--	--
Number of Openings for Job Order	0.000	0.029	0.9924
Outside of Washington State	-5.988	3.827	0.1184
Hours Required at Job: < 40 (Omitted)	--	--	--
Mail Survey Package: Plain (Omitted)	--	--	--
Referral Received in Q1 1998	0.848	0.684	0.2158
Referral Received in Q2 1998 (Omitted)	--	--	--
Number of Non-zero Quarters in 1997: 0	-1.061	3.296	0.7476
Number of Non-zero Quarters in 1997: 1	-0.240	2.488	0.9233
Number of Non-zero Quarters in 1997: 2	-2.079	1.446	0.1512
Number of Non-zero Quarters in 1997: 3	-0.867	1.123	0.4405
Number of Non-zero Quarters in 1997: 4 (Omitted)	--	--	--
Range in 1997 Divided by Highest Earning Quarter in 1997	3.146	2.666	0.2386
Highest Earning Quarter in 1997 - Lowest Earning Quarter	0.000		
Willing to Relocate	-0.330	0.828	0.6906
Salary Reported	-0.103	0.134	0.4414
Salary Reported < 50 (Hourly Wage)	0.103	0.134	0.4418
Salary Reported > 50 (Weekly, Monthly, or Annual Wage)	0.000		
Job-seeker Located in Seattle, Washington	0.876	1.113	0.4317
Education: Some College	0.000		
Southwest Washington State (Omitted)	--	--	--
Industry: Timber	1.068	5.543	0.8473
Program Participant: UI Claimant	0.431	0.825	0.6015
Race: White	-0.664	0.988	0.5017
Missing: Minimum Wage Required	0.288	1.810	0.8736
Missing: Range in 1997/Highest Earning Quarter in 1997	0.000		
Missing: Salary Reported < 50	-1.216	2.075	0.5583
Missing: Salary Reported > 50 (Omitted)	--	--	--
Occupational Classification: First Digit 0	4.907	2.618 **	0.0616
Occupational Classification: First Digit 1	11.390	6.397 **	0.0757
Occupational Classification: First Digit 2	0.000		
Occupational Classification: First Digit 3	4.728	2.204 **	0.0325
Occupational Classification: First Digit 4	0.910	1.671	0.5863
Occupational Classification: First Digit 5	1.070	1.463	0.4648
Occupational Classification: First Digit 6	2.452	1.775	0.1679
Occupational Classification: First Digit 7	2.388	1.574	0.1299
Occupational Classification: First Digit 8	0.759	1.799	0.6733

Variable	Parameter Estimate	Standard Error		Prob > |T|
Occupational Classification: First Digit 9 (Omitted)	0.000			
SIC Code: First Digit 0	0.000			
SIC Code: First Digit 1	0.917	2.352		0.6968
SIC Code: First Digit 2	-0.020	2.069		0.9921
SIC Code: First Digit 3	0.000			
SIC Code: First Digit 4	-0.285	2.474		0.9082
SIC Code: First Digit 5	2.102	2.134		0.3251
SIC Code: First Digit 6	4.120	2.659		0.1219
SIC Code: First Digit 7	1.729	2.127		0.4168
SIC Code: First Digit 8	0.725	2.179		0.7395
SIC Code: First Digit 9	0.000			
Response Type: Unidentified	0.196	1.307		0.8812
Response Type: Completed Survey	-1.916	1.267		0.1313
Response Type: Returned Without Forwarding Information	-4.287	4.901		0.3822
Job Order Has One Referral and One Placement	-3.999	2.808		0.1552
Job Order Has One Referral and Two Placements	2.510	2.786		0.3681
Job Order Has One Referral and Three Placements	-4.778	6.095		0.4335
Job Order Has Two Referrals and One Placement	-0.160	2.809		0.9547
Job Order Has Two Referrals and Two Placements	-1.247	3.026		0.6806
Job Order Has Two Referrals and Three Placements	0.000			
Job Order Has Three Referrals and One Placement	-2.297	2.588		0.3752
Job Order Has Three Referrals and Two Placements	-0.715	2.498		0.7747
Job Order Has Three Referrals and Three Placements	0.000			
Placed at the Job Interviewed	-4.695	0.942	***	0.0001
Accepted Offer, Did not Show Up for Work	-1.248	2.383		0.6006
Did not Accept Offer After Interviewing	-6.517	2.164	***	0.0027
Did not Obtain Offer After Interviewing	1.651	1.591		0.2999
Tried, but Failed to Obtain an Interview	-1.360	1.198		0.2571
Did not Try to Obtain Interview (Omitted)	--	--		--

Note: *=Significant at the .10 level, **=Significant at the .05 level, ***=Significant at the .01 level

Appendix C

APPENDIX C

Appendix C-1 is a description of the variables used in the Washington State administrative data analysis. The variables were created from Washington State UI and ES administrative data. The number of observations, mean, and standard deviation are included for each variable.

Appendix C-2 shows the estimates using equation 4.1 with weeks of unemployment as the dependent variable restricted to the referrals and placements made during weeks 10-13. We present the regression for this time period only as an example and the results for the other periods are available upon request. For each variable the parameter estimate, standard error, and the *p*-value are listed to the right of the variable description. The *p*-value refers to the level of significance for the parameter estimate and *p*-values less than .05 are usually considered statistically significant.

Appendix C-3 shows the estimates using equation 4.1 with UI compensated weeks of unemployment as the dependent variable restricted to the referrals and placements made during weeks 10-13. We present the regression for this time period only as an example and the results for the other periods are available upon request. For each variable the parameter estimate, standard error, and the *p*-value are listed to the right of the variable description. The *p*-value refers to the level of significance for the parameter estimate and *p*-values less than .05 are usually considered statistically significant.

Appendix C.1. Description of Variables Used in the WA Administrative Data

Variable	Description	N	Mean	Std Dev
SP_START	Spell Start (SAS Date)	328815	11729.070	842.788
WEEK1BEN	Weekly Benefit Amount	328815	180.522	74.641
PRETENCN	Number of Continuous Quarters with Pre-EIN	328815	5.047	5.911
JOBCHG	Job Change Dummy	328815	2.824	1.838
EARN1	Earnings 1 Quarter after Spell Start	324622	2381.750	3179.100
EARN2	Earnings 2 Quarters after Spell Start	318199	2946.260	3428.790
EARN3	Earnings 3 Quarters after Spell Start	309136	3142.130	3511.340
EARN4	Earnings 4 Quarters after Spell Start	297165	3057.980	3489.770
EARN5	Earnings 5 Quarters after Spell Start	282423	3025.790	3447.340
EARN6	Earnings 6 Quarters after Spell Start	272094	3257.520	3604.110
EARN7	Earnings 7 Quarters after Spell Start	259521	3314.310	3661.440
EARN8	Earnings 8 Quarters after Spell Start	246365	3186.060	3594.110
EARN9	Earnings 9 Quarters after Spell Start	230523	3130.040	3579.950
EARN10	Earnings 10 Quarters after Spell Start	218804	3301.290	3704.370
EARN11	Earnings 11 Quarters after Spell Start	207699	3336.600	3721.490
EARN12	Earnings 12 Quarters after Spell Start	194559	3213.810	3658.550
EMP	Employment at Month of Spell Start	328815	256372.650	301020.760
SP_LNGTH	Spell Length Grouped By Ref/Plt Categories	328815	4.316	2.916
QTR	Quarter of Spell Start	328815	2.587	1.177
YEAR	Year of Spell Start	328815	1991.600	2.305
EDATTAIN	Educational Attainment Code	328815	2.987	1.284
N0QBASE	Number of Non-Zero Quarters in Base Period	328815	3.277	1.193
N0QBEFB	Number of Non-Zero quarters Year Before	328815	2.798	1.539
AGE	Age at Start of Spell	328815	35.497	9.904
EMP_CHG	Employment in Month-Employment in Month	328815	125.406	5136.060
UNEMPRTE	WA Unemployment Rate in Month of Spell Start	328815	0.076	0.029
QTRAFT	First Quarter after Spell with Earnings > 0	328815	2.059	1.967
IREF1	Indicator of Referral	328815	0.014	0.119
IREF2	Indicator of Referral	328815	0.026	0.160
IREF3	Indicator of Referral	328815	0.021	0.144
IREF4	Indicator of Referral	328815	0.019	0.136
IREF5	Indicator of Referral	328815	0.021	0.143
IREF6	Indicator of Referral	328815	0.015	0.123
IREF7	Indicator of Referral	328815	0.015	0.120
IREF8	Indicator of Referral	328815	0.018	0.132
IREF9	Indicator of Referral	328815	0.013	0.115
IREF10	Indicator of Referral	328815	0.012	0.110
IREF11	Indicator of Referral	328815	0.032	0.177
IREF12	Indicator of Referral	328815	0.046	0.211

Variable	Description	N	Mean	Std Dev
IREF13	Indicator of Referral	328815	0.043	0.202
IREF14	Indicator of Referral	328815	0.039	0.193
IREF15	Indicator of Referral	328815	0.036	0.187
IREF16	Indicator of Referral	328815	0.034	0.181
IREF17	Indicator of Referral	328815	0.032	0.176
IREF18	Indicator of Referral	328815	0.030	0.171
IREF19	Indicator of Referral	328815	0.028	0.164
IREF20	Indicator of Referral	328815	0.136	0.342
IPLT1	Indicator of Placement	328815	0.003	0.051
IPLT2	Indicator of Placement	328815	0.009	0.093
IPLT3	Indicator of Placement	328815	0.008	0.089
IPLT4	Indicator of Placement	328815	0.007	0.084
IPLT5	Indicator of Placement	328815	0.008	0.088
IPLT6	Indicator of Placement	328815	0.006	0.078
IPLT7	Indicator of Placement	328815	0.006	0.078
IPLT8	Indicator of Placement	328815	0.008	0.086
IPLT9	Indicator of Placement	328815	0.006	0.076
IPLT10	Indicator of Placement	328815	0.006	0.075
IPLT11	Indicator of Placement	328815	0.014	0.119
IPLT12	Indicator of Placement	328815	0.012	0.111
IPLT13	Indicator of Placement	328815	0.011	0.106
IPLT14	Indicator of Placement	328815	0.011	0.103
IPLT15	Indicator of Placement	328815	0.010	0.099
IPLT16	Indicator of Placement	328815	0.009	0.094
IPLT17	Indicator of Placement	328815	0.009	0.092
IPLT18	Indicator of Placement	328815	0.008	0.091
IPLT19	Indicator of Placement	328815	0.008	0.088
IPLT20	Indicator of Placement	328815	0.047	0.212
IAVGBASE	Average Base Earnings Inflation Adjusted	328815	4610.280	3707.220
ITOTBASE	Total Base Earnings Inflation Adjusted	318185	17133.700	13695.460
IAVGBEFB	Average Earnings Before Base Inflation Adjusted	328815	3995.040	3922.000
ITOTBEFB	Total Earnings Before Base Inflation Adjusted	299538	15571.890	14605.430
WEEKS	Number of Weeks in Spell	328815	14.259	13.746
QTRAFT4	First Qtr after Spell Earning > 4 x Weekly Benefits	328815	30.626	27.886
EMPD	Dummy=1 if Emp=.	328815	0.002	0.042
EMP_CHGD	Dummy=1 if Emp_chg=.	328815	0.002	0.042
UNEMPRTD	Dummy=1 if Unemprte=.	328815	0.002	0.042
SP_TRUNC	Dummy=1 if Spell Starts after Q2 1993	328815	0.207	0.405
JC234	Job Change Groups 2,3,4	328815	0.161	0.367
JC1	Job Change: Retain Job	328815	0.454	0.498
JC2	Job Change: No Pre-Spell ID	328815	0.047	0.211

Variable	Description	N	Mean	Std Dev
JC3	Job Change: Dropout	328815	0.108	0.310
JC4	Job Change: Possible Firm ID Change	328815	0.006	0.080
JC5	Job Change: Change Job	328815	0.386	0.487
Y87	Year 1987 Dummy Variable	328815	0.069	0.253
Y88	Year 1988 Dummy Variable	328815	0.061	0.239
Y89	Year 1989 Dummy Variable	328815	0.070	0.255
Y90	Year 1990 Dummy Variable	328815	0.103	0.305
Y91	Year 1991 Dummy Variable	328815	0.143	0.350
Y92	Year 1992 Dummy Variable	328815	0.149	0.356
Y93	Year 1993 Dummy Variable	328815	0.159	0.365
Y94	Year 1994 Dummy Variable	328815	0.156	0.362
Y95	Year 1995 Dummy Variable	328815	0.092	0.288
QTR_1	Quarter 1	328815	0.258	0.437
QTR_2	Quarter 2	328815	0.212	0.408
QTR_3	Quarter 3	328815	0.217	0.412
QTR_4	Quarter 4	328815	0.314	0.464
EDTAIN1	Education: Less than High School	328815	0.095	0.293
EDTAIN2	Education: High School	328815	0.106	0.308
EDTAIN3	Education: Some College	328815	0.662	0.473
EDTAIN4	Education: College	328815	0.108	0.310
EDTAIN9	Education: Not Reported	328815	0.029	0.169
ABERDEEN	WA Job Service Office Location	328815	0.018	0.134
AUBURN	WA Job Service Office Location	328815	0.048	0.213
BELLEVUE	WA Job Service Office Location	328815	0.043	0.202
BELLINGH	WA Job Service Office Location	328815	0.030	0.170
BELLTOWN	WA Job Service Office Location	328815	0.004	0.061
BOEING_E	WA Job Service Office Location	328815	0.001	0.032
BOEING_R	WA Job Service Office Location	328815	0.002	0.039
BREMERTO	WA Job Service Office Location	328815	0.028	0.165
COLUMBIA	WA Job Service Office Location	328815	0.009	0.095
COLVILLE	WA Job Service Office Location	328815	0.013	0.114
COWLITZ	WA Job Service Office Location	328815	0.031	0.172
DATASYS	WA Job Service Office Location	328815	0.055	0.228
ELLENSBU	WA Job Service Office Location	328815	0.006	0.075
EVERETT	WA Job Service Office Location	328815	0.049	0.216
FORKS	WA Job Service Office Location	328815	0.004	0.066
INVALID	WA Job Service Office Location	328815	0.002	0.042
LAKEWOOD	WA Job Service Office Location	328815	0.052	0.222
LEWIS	WA Job Service Office Location	328815	0.021	0.143
LYNNWOOD	WA Job Service Office Location	328815	0.034	0.181
MOSES	WA Job Service Office Location	328815	0.027	0.163

Variable	Description	N	Mean	Std Dev
MTVERNON	WA Job Service Office Location	328815	0.039	0.194
NSEATTLE	WA Job Service Office Location	328815	0.031	0.173
OKANOGAN	WA Job Service Office Location	328815	0.019	0.138
OLYMPIA	WA Job Service Office Location	328815	0.034	0.181
PTANGELE	WA Job Service Office Location	328815	0.013	0.114
PTTOWNSE	WA Job Service Office Location	328815	0.005	0.070
PULLMAN	WA Job Service Office Location	328815	0.005	0.070
RAINIER	WA Job Service Office Location	328815	0.036	0.186
RAYMOND	WA Job Service Office Location	328815	0.006	0.077
RENTON	WA Job Service Office Location	328815	0.039	0.194
RENTGRAD	WA Job Service Office Location	328815	0.001	0.029
SPOKANE	WA Job Service Office Location	328815	0.053	0.223
SUNNYSID	WA Job Service Office Location	328815	0.036	0.187
TACOMA	WA Job Service Office Location	328815	0.037	0.189
TRICITY	WA Job Service Office Location	328815	0.029	0.168
VANCOUVE	WA Job Service Office Location	328815	0.035	0.185
WALLA	WA Job Service Office Location	328815	0.013	0.115
WENATCHE	WA Job Service Office Location	328815	0.035	0.185
YAKIMA	WA Job Service Office Location	328815	0.057	0.231
GEND	Gender (0=Male, 1=Female)	328815	0.316	0.465
RACE1	Race: White	328815	0.784	0.412
RACE2	Race: Black	328815	0.032	0.176
RACE3	Race: Hispanic	328815	0.133	0.340
RACE4	Race: Native American	328815	0.015	0.122
RACE5	Race: Asian	328815	0.022	0.146
RACE6	Race: Unknown	328815	0.014	0.118
PUB0	Public Assistance: Not Receiving Assistance	328815	0.983	0.128
PUB3	Public Assistance: Receiving Assistance	328815	0.004	0.064
PUB4	Public Assistance: Family Independence Program	328815	0.002	0.048
PUB6	Public Assistance: Status Unknown	328815	0.010	0.101
FOOD	Registered to Receive Food Stamps	328815	0.014	0.116
DISLOCN	Dislocated Worker: No	328815	0.819	0.385
DISLOC9	Dislocated Worker: Unknown	328815	0.173	0.378
DISLOCY	Dislocated Worker: Yes	328815	0.008	0.088
UISIC0	Industry: Forestry, Agriculture	328815	0.136	0.343
UISIC1	Industry: Construction, Mining	328815	0.177	0.382
UISIC2	Industry: Non-durable Manufacturing	328815	0.139	0.346
UISIC3	Industry: Durable Manufacturing	328815	0.085	0.278
UISIC4	Industry: Transportation and Utilities	328815	0.054	0.227
UISIC5	Industry: Trade	328815	0.188	0.391
UISIC6	Industry: Finance, Insurance, and Real Estate	328815	0.031	0.174

Variable	Description	N	Mean	Std Dev
UISIC7	Industry: Services	328815	0.083	0.275
UISIC8	Industry: Government	328815	0.087	0.283
UISIC9	Industry: Other	328815	0.020	0.141
I_REF	Referred (All Periods)	328815	0.054	0.226
I_PLT	Placed (All Periods)	328815	0.023	0.150
R87	Referred in 1987	328815	0.000	0.019
P87	Placed in 1987	328815	0.000	0.020
N87	Neither Referred or Placed in 1987	328815	0.068	0.251
R88	Referred in 1988	328815	0.001	0.031
P88	Placed in 1988	328815	0.001	0.029
N88	Neither Referred or Placed in 1988	328815	0.059	0.236
R89	Referred in 1989	328815	0.002	0.049
P89	Placed in 1989	328815	0.002	0.040
N89	Neither Referred or Placed in 1989	328815	0.066	0.248
R90	Referred in 1990	328815	0.007	0.084
P90	Placed in 1990	328815	0.003	0.057
N90	Neither Referred or Placed in 1990	328815	0.093	0.291
R91	Referred in 1991	328815	0.009	0.097
P91	Placed in 1991	328815	0.004	0.061
N91	Neither Referred or Placed in 1991	328815	0.130	0.336
R92	Referred in 1992	328815	0.009	0.094
P92	Placed in 1992	328815	0.004	0.060
N92	Neither Referred or Placed in 1992	328815	0.136	0.343
R93	Referred in 1993	328815	0.009	0.093
P93	Placed in 1993	328815	0.003	0.059
N93	Neither Referred or Placed in 1993	328815	0.146	0.353
R94	Referred in 1994	328815	0.010	0.098
P94	Placed in 1994	328815	0.004	0.061
N94	Neither Referred or Placed in 1994	328815	0.142	0.349
R95	Referred in 1995	328815	0.006	0.080
P95	Placed in 1995	328815	0.002	0.049
N95	Neither Referred or Placed in 1995	328815	0.083	0.275
RECREF	Referred and Job Change: Retain Job	328815	0.019	0.136
RECPLT	Placed and Job Change: Retain Job	328815	0.009	0.095
RECNEI	Neither Referred or Placed and Job Change: Retain Job	328815	0.426	0.494
J234REF	Referred and Job Change Groups 2,3,4	328815	0.009	0.092
J234PLT	Placed and Job Change Groups 2,3,4	328815	0.003	0.055
J234NEI	Neither Referred or Placed and Job Change Groups 2,3,4	328815	0.149	0.356
CHAREF	Referred and Job Change: Change Job	328815	0.027	0.161
CHAPLT	Placed and Job Change: Change Job	328815	0.011	0.105
CHANEI	Neither Referred or Placed and Job Change: Change Job	328815	0.348	0.476

Appendix C.2. Regression Measuring the Effect of Referrals and Placements Made in Weeks 10-13 on Weeks of Unemployment for Claimants Unemployed at Least 10 Weeks

Variable	Parameter Estimate	Standard Error		Prob > |T|
R-square	0.203			
Intercept	15.868	0.742	***	0.0001
Referred	-0.616	0.174	***	0.0004
Placed	-4.811	0.327	***	0.0001
DEMOGRAPHICS				
Age (Years)	0.070	0.003	***	0.0001
Race				
White	-0.474	0.230	**	0.0394
Black	1.267	0.268	***	0.0001
Hispanic	-0.668	0.261	***	0.0106
Native American	-0.701	0.321	**	0.0291
Asian	-0.146	0.293	*	0.6169
Unknown (Omitted)	--	--	--	--
Female	1.972	0.067	***	0.0001
Education				
Less than high school	-0.260	0.217		0.2295
Hish school	0.425	0.189	**	0.0249
Some college	0.362	0.170	**	0.0337
College degree	-0.325	0.189	*	0.0849
Not reported (Omitted)	--	--	--	--
WORK HISTORY				
Job Change Status				
Retain Job	-6.741	0.069	***	0.0001
No Pre-Spell ID	-1.339	0.179	***	0.0001
Dropout	2.211	0.095	***	0.0001
Possible Firm ID Change	2.408	0.327	***	0.0001
Change Job (Omitted)	--	--	--	--
Tenure (quarters)	0.240	0.006	***	0.0001
Maximum weekly payment	0.014	0.001	***	0.0001

Variable	Parameter Estimate	Standard Error		Prob > |T|
Earnings 5-8 quarters before spell	0.000	0.000	***	0.0001
Earnings 1-4 quarters before spell	0.000	0.000	***	0.0001
LABOR MARKET				
Employment	0.000	0.000		0.4486
Employment Change	-0.113	0.000	**	0.0496
Unemployment rate	0.407	2.334		0.8616
YEAR				
1987	3.090	0.299	***	0.0001
1988	3.323	0.279	***	0.0001
1989	2.726	0.264	***	0.0001
1990	3.655	0.254	***	0.0001
1991	8.571	0.243	***	0.0001
1992	10.361	0.236	***	0.0001
1993	7.076	0.232	***	0.0001
1994	3.760	0.148	***	0.0001
1995 (Omitted)	--	--	--	--
QUARTER				
1	-0.250	0.091	***	0.0057
2	1.750	0.085	***	0.0001
3	1.234	0.085	***	0.0001
4 (Omitted)	--	--	--	--
PROGRAM PARTICIPATION				
Registered to collect Food Stamps	0.825	0.236	***	0.0005
Public Assistance Status				
Not receiving public assistance	-0.896	0.267	***	0.0008
Receiving public assistance	-0.298	0.514		0.5625
In Family Independence program	-1.258	0.624	**	0.0439
Status unknown (Omitted)	--	--	--	--
Dislocated Worker Status				
Not a dislocated worker	-3.459	0.334	***	0.0001
Maybe a dislcated worker	-1.983	0.326	***	0.0001
Definitely a dislocated worker (Omitted)	--	--	--	--

Variable	Parameter Estimate	Standard Error		Prob > |T|
INDUSTRY (1-digit SIC)				
1. Forestry, Agriculture	-1.152	0.212	***	0.0001
2. Construction, Mining	-1.876	0.203	***	0.0001
3. Non-durable manufacturing	-0.456	0.208	**	0.0286
4. Durable manufacturing	0.468	0.213	**	0.0281
5. Transportation and Utilities	-0.693	0.226	***	0.0022
6. Trade	-0.129	0.199		0.5168
7. Finance, Insurance, and Real Estate	-0.221	0.237		0.3512
8. Services	0.032	0.210		0.8795
9. Government	-0.144	0.208		0.4888
10. Other (Omitted)	--	--	--	--
Spell Truncated	-1.768	0.176	***	0.0001
JOB SERVICE OFFICE				
ABERDEEN	2.459	0.308	***	0.0001
AUBURN	2.806	1.775		0.1139
BELLEVUE	1.874	1.776		0.2916
BELLINGH	0.060	0.260		0.8182
BELLTOWN	3.314	1.837	*	0.0713
BOEING_E	11.368	0.924	***	0.0001
BOEING_R	2.330	1.880		0.2152
BREMERTO	1.349	0.271	***	0.0001
COLUMBIA	-0.153	0.384		0.6908
COLVILLE	0.354	0.349		0.3112
COWLITZ	0.191	0.287		0.5063
DATASYS	-0.675	0.239	***	0.0047
ELLENSBU	0.603	0.437		0.1676
EVERETT	1.011	0.429	**	0.0185
FORKS	-0.302	0.581		0.6032
INVALID	-0.081	0.798		0.9194
LAKEWOOD	1.570	0.459	***	0.0006
LEWIS	-0.397	0.315		0.2076
LYNNWOOD	0.370	0.437		0.3975
MOSES	-0.462	0.280	*	0.0984
MTVERNON	-0.019	0.255		0.9420
NSEATTLE	2.540	1.776		0.1528
OKANOGAN	-1.075	0.313	***	0.0006
OLYMPIA	0.794	0.259	***	0.0022

Variable	Parameter Estimate	Standard Error		Prob > |T|
PTANGELE	0.222	0.365		0.5432
PTTOWNSE	0.960	0.477	**	0.0444
PULLMAN	-1.718	0.542	***	0.0015
RAINIER	3.772	1.774	**	0.0335
RAYMOND	0.061	0.459		0.8948
RENTON	2.590	1.774		0.1442
RENTGRAD	2.859	2.073		0.1679
SPOKANE	1.008	0.294	***	0.0006
SUNNYSID	-0.149	0.209		0.4765
TACOMA	1.176	0.465	**	0.0114
TRICITY	-0.734	0.246	***	0.0029
VANCOUVE	-0.179	0.278		0.5205
WALLA	-1.202	0.349	***	0.0006
WENATCHE	-0.696	0.257	***	0.0067
YAKIMA (Omitted)	--	--	--	--

Note: *=Significant at the .10 level, **=Significant at the .05 level, ***=Significant at the .01 level

Appendix C.3. Regression Measuring the Effect of Referrals and Placements Made in Weeks 10-13 on UI Compensated Weeks of Unemployment for Claimants Unemployed at Least 10 Weeks

Variable	Parameter Estimate	Standard Error		Prob > |T|
R-square	0.346			
Intercept	25.323	1.525	***	0.0001
Referred	-2.460	0.358	***	0.0001
Placed	12.388	0.671	***	0.0001
DEMOGRAPHICS				
Age (Years)	0.046	0.006	***	0.0001
Race				
White	-0.746	0.473		0.1150
Black	-1.169	0.551	**	0.0338
Hispanic	-4.322	0.537	***	0.0001
Native American	0.873	0.660		0.1856
Asian	-0.012	0.601		0.9835
Unknown (Omitted)	--	--	--	--
Female	1.344	0.138	***	0.0001
Education				
Less than high school	-3.599	0.445	***	0.0001
Hish school	-0.351	0.389		0.3676
Some college	-0.874	0.350	**	0.0124
College degree	-0.194	0.388		0.6163
Not reported (Omitted)	--	--	--	--
WORK HISTORY				
Job Change Status				
Retain Job	10.961	0.141	***	0.0001
No Pre-Spell ID	-1.160	0.368	***	0.0016
Dropout	40.221	0.196	***	0.0001
Possible Firm ID Change	44.492	0.672	***	0.0001
Change Job (Omitted)	--	--	--	--
Tenure (quarters)	0.108	0.011	***	0.0001
Maximum weekly payment	0.025	0.001	***	0.0001

Variable	Parameter Estimate	Standard Error		Prob > |T|
Earnings 5-8 quarters before spell	0.000	0.000	***	0.0001
Earnings 1-4 quarters before spell	0.000	0.000	***	0.0001
LABOR MARKET				
Employment	0.000	0.000	***	0.0030
Employment Change	0.000	0.000	***	0.0008
Unemployment rate	-10.230	4.794	**	0.0329
YEAR				
1987	11.648	0.613	***	0.0001
1988	14.116	0.573	***	0.0001
1989	13.029	0.542	***	0.0001
1990	14.094	0.521	***	0.0001
1991	16.511	0.500	***	0.0001
1992	15.350	0.484	***	0.0001
1993	14.076	0.476	***	0.0001
1994	11.474	0.304	***	0.0001
1995 (Omitted)	--	--	--	--
QUARTER				
1	0.549	0.186	***	0.0031
2	2.775	0.175	***	0.0001
3	1.414	0.175	***	0.0001
4 (Omitted)	--	--	--	--
PROGRAM PARTICIPATION				
Registered to collect Food Stamps	4.365	0.484	***	0.0001
Public Assistance Status				
Not receiving public assistance	-5.562	0.549	***	0.0001
Receiving public assistance	-5.198	1.056	***	0.0001
In Family Independence program	0.112	1.283		0.9301
Status unknown (Omitted)	--	--	--	--
Dislocated Worker Status				
Not a dislocated worker	-0.177	0.686		0.7968
Maybe a dislcated worker	-1.188	0.670	*	0.0763
Definitely a dislocated worker (Omitted)	--	--	--	--

Variable	Parameter Estimate	Standard Error		Prob > |T|
INDUSTRY (1-digit SIC)				
1. Forestry, Agriculture	1.846	0.435	***	0.0001
2. Construction, Mining	-3.944	0.418	***	0.0001
3. Non-durable manufacturing	-0.768	0.428	*	0.0726
4. Durable manufacturing	0.539	0.438		0.2184
5. Transportation and Utilities	-1.898	0.464	***	0.0001
6. Trade	0.069	0.408		0.8663
7. Finance, Insurance, and Real Estate	0.059	0.486		0.9029
8. Services	0.814	0.431	*	0.0588
9. Government	0.202	0.426		0.6354
10. Other (Omitted)	--	--	--	--
Spell Truncated	-1.940	0.362	***	0.0001
JOB SERVICE OFFICE				
ABERDEEN	2.129	0.633	***	0.0008
AUBURN	14.474	3.646	***	0.0001
BELLEVUE	13.681	3.649	***	0.0002
BELLINGH	1.741	0.534	***	0.0011
BELLTOWN	10.325	3.774	***	0.0062
BOEING_E	14.502	1.898	***	0.0001
BOEING_R	20.968	3.862	***	0.0001
BREMERTO	5.105	0.557	***	0.0001
COLUMBIA	7.599	0.789	***	0.0001
COLVILLE	4.203	0.717	***	0.0001
COWLITZ	3.195	0.590	***	0.0001
DATASYS	7.460	0.490	***	0.0001
ELLENSBU	0.925	0.897		0.3025
EVERETT	5.678	0.881	***	0.0001
FORKS	2.492	1.194	**	0.0370
INVALID	1.586	1.639		0.3333
LAKEWOOD	6.425	0.943	***	0.0001
LEWIS	0.629	0.648		0.3317
LYNNWOOD	4.958	0.898	***	0.0001
MOSES	-0.879	0.574		0.1259
MTVERNON	1.427	0.524	***	0.0064
NSEATTLE	13.462	3.649		0.0002
OKANOGAN	-0.482	0.643		0.4539
OLYMPIA	2.471	0.532	***	0.0001

Variable	Parameter Estimate	Standard Error	Prob > |T|
PTANGELE	-0.345	0.750	0.6453
PTTOWNSE	4.470	0.981 ***	0.0001
PULLMAN	4.508	1.113 ***	0.0001
RAINIER	15.508	3.644 ***	0.0001
RAYMOND	-1.061	0.942	0.2599
RENTON	13.190	3.644 ***	0.0003
RENTGRAD	8.818	4.258 **	0.0384
SPOKANE	3.799	0.605 ***	0.0001
SUNNYSID	-1.076	0.429 **	0.0122
TACOMA	6.087	0.955 ***	0.0001
TRICITY	0.402	0.505	0.4263
VANCOUVE	8.472	0.571 ***	0.0001
WALLA	2.163	0.717 ***	0.0026
WENATCHE	-3.380	0.527 ***	0.0001
YAKIMA (Omitted)	--	-- --	--

Note: *=Significant at the .10 level, **=Significant at the .05 level, ***=Significant at the .01 level

Appendix D

APPENDIX D

Appendix D-1 is a list of the variables used in the Oregon State administrative data analysis. The variables were created from Oregon State UI and ES administrative data by staff from the Oregon Employment Department (OED). The number of observations, mean, and standard deviation are included for each variable.

Appendix D-2 shows the estimates using equation 4.1 with weeks of unemployment as the dependent variable restricted to the referrals and placements made during weeks 10-13. We present the regression for this time period only as an example and the results for the other periods are available upon request. For each variable the parameter estimate, standard error, and the *p*-value are listed to the right of the variable description. The *p*-value refers to the level of significance for the parameter estimate and *p*-values less than .05 are usually considered statistically significant.

Appendix D-3 shows the estimates using equation 4.1 with UI compensated weeks of unemployment as the dependent variable restricted to the referrals and placements made during weeks 10-13. We present the regression for this time period only as an example and the results for the other periods are available upon request. For each variable the parameter estimate, standard error, and the *p*-value are listed to the right of the variable description. The *p*-value refers to the level of significance for the parameter estimate and *p*-values less than .05 are usually considered statistically significant.

Appendix D.1. Description of Variables Used in the OR Administrative Data

Variable	N	Mean	Std Dev	
SPLNGTH	58980	20.190	7.150	Spell length
BSYRWGE	58980	16944.123	12404.629	Base-year wage
RWBA	58980	186.810	83.080	Weekly benefit amount
SEX	58980	0.580	0.490	Male = 1, Female = 0
BSYRWEEK	58980	40.800	10.980	
CLAIMAGE	58980	37.460	10.230	Age in years
WRACE	58980	0.850	0.350	1 = white, 0 = other
HRACE	58980	0.077	0.270	1 = Hispanic, 0 = other
ARACE	58980	0.018	0.130	1 = American Indian, 0 = other
IRACE	58980	0.021	0.140	1 = other than African American
SICA	58980	0.045	0.210	Industry Dummies
SICB	58980	0.002	0.044	
SICC	58980	0.095	0.290	
SICD	58980	0.230	0.420	
SICE	58980	0.047	0.210	
SICF	58980	0.061	0.240	
SICG	58980	0.180	0.390	
SICH	58980	0.044	0.200	
SICI	58980	0.240	0.430	
SICJ	58980	0.042	0.200	
BAKE	58980	0.009	0.095	Office Dummies
CROO	58980	0.007	0.083	
DOUGLA	58980	0.037	0.190	
HARNE	58980	0.004	0.067	
JEFFERS	58980	0.006	0.074	
JOSEPHI	58980	0.028	0.160	
KLAMAT	58980	0.024	0.150	
SHERMA	58980	0.001	0.025	
UMATILL	58980	0.025	0.160	
UNIO	58980	0.010	0.098	
WASC	58980	0.010	0.099	
WASHING	58980	0.086	28.000	
SameEmp	58980	0.220	0.410	Recalled = 1
EMPCHG	58980	9.860	15.620	Local area employment change (2)
QTR1SPST	58980	0.200	0.400	Spell start quarter 1 = 1, other = 0
QTR2SPST	58980	0.220	0.420	Spell start quarter 2 = 2, other = 0
QTR3SPST	58980	0.200	0.400	Spell start quarter 3 = 3, other = 0
QTR4SPST	58980	0.380	0.490	Spell start quarter 4 = 4, other = 0

Variable	N	Mean	Std Dev	
YR95SPST	58980	1.000	0.000	Spell start in 1995
DOT_01	58980	0.210	0.400	Occupation Dummies
DOT_2	58980	0.210	0.410	
DOT_3	58980	0.120	0.330	
DOT_4	58980	0.071	0.260	
DOT_5	58980	0.043	0.200	
DOT_6	58980	0.068	0.250	
DOT_7	58980	0.036	0.190	
DOT_8	58980	0.100	0.300	
ESREF4	58980	0.041	0.200	Referred in weeks 10-13
ESPLT4	58980	0.003	0.056	Placed in weeks 10-13
UNEMPSP	58980	80.360	7.340	Unemployment rate x 10 in local area
EMPSP	58980	1656.200	24.670	Employment in local area
DENSITY	58980	329.280	489.810	Local area density (pop./sq. mile)
AGESQ	58980	1507.980	796.930	Age squared
REPWAGE	58980	0.312	0.070	Replacement wage (WBA/high qtr. earning)

Appendix D.2. Regression Measuring the Effect of Referrals and Placements Made in Weeks 10-13 on Weeks of Unemployment for Claimants Unemployed at Least 10 Weeks

Variable	Parameter Estimate	Standard Error		Prob > |T|	
R-squared	0.095				
(Constant)	59.993	11.510	***	0.0000	
BSYRWGE	0.000	0.000	***	0.0000	Base-year wage
RWBA	-0.004	0.001	***	0.0030	Weekly benefit amount
SEX	-0.230	0.147		0.1170	Male = 1, Female = 0
BSYRWEEK	-0.133	0.007	***	0.0000	
CLAIMAGE	0.081	0.043	*	0.0580	Age in years
WRACE	0.465	0.355		0.1900	1 = white, 0 = other
HRACE	-0.046	0.427		0.9140	1 = Hispanic, 0 = other
ARACE	1.132	0.564	**	0.0450	1 = American Indian, 0 = other
IRACE	0.964	0.549	*	0.0790	1 = other than African American
SICA	-2.498	0.608	***	0.0000	Industry Dummies
SICB	-5.931	1.463	***	0.0000	
SICC	-2.778	0.557	***	0.0000	
SICD	-1.714	0.529	***	0.0010	
SICE	-2.934	0.587	***	0.0000	
SICF	-2.877	0.569	***	0.0000	
SICG	-2.649	0.536	***	0.0000	
SICH	-3.752	0.593	***	0.0000	
SICI	-3.055	0.532	***	0.0000	
SICJ	-1.514	0.596	**	0.0110	
BAKE	1.765	0.637	***	0.0060	Office Dummies
CROO	3.262	0.729	***	0.0000	
DOUGLA	1.196	0.324	***	0.0000	
HARNE	-1.733	0.910	*	0.0570	
JEFFERS	2.067	0.816	**	0.0110	
JOSEPHI	2.275	0.370	***	0.0000	
KLAMAT	0.600	0.398		0.1310	
SHERMA	5.828	2.369	**	0.0140	
UMATILL	1.408	0.394	***	0.0000	
UNIO	0.280	0.620		0.6520	
WASC	-0.761	0.611		0.2130	
WASHING	-0.870	0.220	***	0.0000	
SameEmp	-10.052	0.155	***	0.0000	Recalled = 1
EMPCHG	0.032	0.009	***	0.0000	Local area employment change (2)

Variable	Parameter Estimate	Standard Error		Prob > |T|	
QTR1SPST	5.993	0.687	***	0.0000	Spell start quarter 1 = 1, other = 0
QTR2SPST	0.840	0.236	***	0.0000	Spell start quarter 2 = 2, other = 0
QTR3SPST	2.075	0.258	***	0.0000	Spell start quarter 3 = 3, other = 0
DOT_01	0.583	0.231	**	0.0120	Occupation Dummies
DOT_2	0.309	0.228		0.1760	
DOT_3	-0.252	0.256		0.3250	
DOT_4	-0.105	0.307		0.7310	
DOT_5	1.345	0.348	***	0.0000	
DOT_6	1.542	0.287	***	0.0000	
DOT_7	0.861	0.364	**	0.0180	
DOT_8	0.635	0.264	**	0.0160	
ESREF4	-2.781	0.306	***	0.0000	Referred in weeks 10-13
ESPLT4	-6.943	1.085	***	0.0000	Placed in weeks 10-13
UNEMPSP	-0.205	0.021	***	0.0000	Unemployment rate x 10 in local area
EMPSP	-0.006	0.007		0.4200	Employment in local area
DENSITY	0.000	0.000		0.9250	Local area density (pop./sq. mile)
AGESQ	-0.001	0.001		0.3490	Age squared
REPWAGE	-4.859	1.078	***	0.0000	Replacement wage (WBA/high qtr. earning)

Note: *=Significant at the .10 level, **=Significant at the .05 level, ***=Significant at the .01 level

Appendix D.3. Regression Measuring the Effect of Referrals and Placements Made in Weeks 10-13 on UI Compensated Weeks of Unemployment for Claimants Unemployed at Least 10 Weeks

Variable	Parameter Estimate	Standard Error		Prob > |T|	
R-squared	0.105				
(Constant)	75.055	5.341	***	0.0000	
BSYRWGE	0.000	0.000	***	0.0000	Base-year wage
RWBA	0.009	0.001	***	0.0000	Weekly benefit amount
SEX	-0.558	0.068	***	0.0000	Male = 1, Female = 0
BSYRWEEK	0.059	0.003	***	0.0000	
CLAIMAGE	0.208	0.020	***	0.0000	Age in years
WRACE	-0.425	0.165	**	0.0120	1 = white, 0 = other
HRACE	-0.433	0.198	**	0.0290	1 = Hispanic, 0 = other
ARACE	0.027	0.262		0.9170	1 = American Indian, 0 = other
IRACE	-0.035	0.255		0.8910	1 = other than African American
SICA	-0.179	0.282		0.5270	Industry Dummies
SICB	-1.280	0.679	*	0.0590	
SICC	-0.654	0.259	**	0.0110	
SICD	0.745	0.246	***	0.0020	
SICE	-0.048	0.272		0.8600	
SICF	0.311	0.264		0.2390	
SICG	0.057	0.249		0.8180	
SICH	0.452	0.275		0.1010	
SICI	0.074	0.247		0.7640	
SICJ	0.004	0.277		0.9890	
BAKE	-1.162	0.295	***	0.0000	Office Dummies
CROO	0.381	0.338		0.2600	
DOUGLA	0.547	0.150	***	0.0000	
HARNE	-1.438	0.422	***	0.0010	
JEFFERS	-0.354	0.379		0.3500	
JOSEPHI	0.766	0.172	***	0.0000	
KLAMAT	0.422	0.285	**	0.0220	
SHERMA	1.525	1.099		0.1660	
UMATILL	-0.623	0.183	***	0.0010	
UNIO	-1.185	0.287	***	0.0000	
WASC	-0.097	0.284		0.7320	
WASHING	-0.322	0.102	***	0.0020	
SameEmp	-3.252	0.072	***	0.0000	Recalled = 1
EMPCHG	-0.014	0.004	***	0.0010	Local area employment change (2)

Variable	Parameter Estimate	Standard Error		Prob > |T|	
QTR1SPST	-0.998	0.319	***	0.0020	Spell start quarter 1 = 1, other = 0
QTR2SPST	-0.660	0.109	***	0.0000	Spell start quarter 2 = 2, other = 0
QTR3SPST	0.768	0.120	***	0.0000	Spell start quarter 3 = 3, other = 0
DOT_01	0.424	0.107	***	0.0000	Occupation Dummies
DOT_2	0.801	0.106	***	0.0000	
DOT_3	0.598	0.119	***	0.0000	
DOT_4	0.280	0.142	**	0.0490	
DOT_5	0.626	0.161	***	0.0000	
DOT_6	0.573	0.133	***	0.0000	
DOT_7	0.680	0.169	***	0.0000	
DOT_8	-0.071	0.122		0.5600	
ESREF4	-0.099	0.142		0.4860	Referred in weeks 10-13
ESPLT4	-2.915	0.503	***	0.0000	Placed in weeks 10-13
UNEMPSP	-0.053	0.010	***	0.0000	Unemployment rate x 10 in local area
EMPSP	-0.035	0.003	***	0.0000	Employment in local area
DENSITY	0.000	0.000	***	0.0020	Local area density (pop./sq. mile)
AGESQ	-0.002	0.000	***	0.0000	Age squared
REPWAGE	-0.479	0.500		0.3390	Replacement wage (WBA/high qtr. earning)

Note: *=Significant at the .10 level, **=Significant at the .05 level, ***=Significant at the .01 level