Estimating WIA Net Impacts for Adults and Dislocated Workers: Updated & Revised Evaluation Design

Administrative DAta Research and Evaluation (ADARE) Project

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The ideal approach for evaluating the effects of workforce interventions such as intensive and training services under the Workforce Investment Act (WIA) is an experimental one with random assignment of participants into treatment and control groups, typically with further statistical adjustments to account for any remaining treatment/control differences. An experimental approach was strongly recommended by the Job Training Longitudinal Survey Research Advisory Committee in their influential report (Stromsdorfer et al. 1985). Experimental approaches have been utilized in many subsequent evaluations of the Job Training Partnership Act (Orr et al., 1995), Job Corps (Burghardt et al. 2001), and the National Welfare-to-Work Strategies program (Hamilton et al. 2001), and is featured in ongoing evaluations of labor exchange and other services as well.

In recent years, however, significant advances have been made in the use of quasi-experimental evaluation methods[1], addressing some of their shortcomings (e.g., Heckman et al. 1989; Bowman, 1993). These methods use comparison groups of individuals *similar* to those in the treatment group rather than random assignment. Quasi-experimental techniques have been used to evaluate state workforce training programs in Utah (Bowman, 1993), Missouri (Mueser et al. 2003), and Washington (Hollenbeck et al. 2001, 2002), as well as state welfare employment programs (King et al. 1994). They are also being applied in a national evaluation of the Trade Adjustment Assistance Reform Act of 2002 by researchers from Social Policy Research Associates and Mathematica Policy Research.

Access to extensive longitudinal data, especially pre-program employment and earnings information, enhance both experimental and quasi-experimental evaluation methods. Longitudinal data may enhance experimental methods, by allowing for statistical adjustment of results when observed characteristics of the treated cases thought to be correlated with key outcomes differ systematically from the control cases due to the "luck of the draw." Similar statistical adjustment can be made in the case of quasi-experimentation, but more importantly, longitudinal data allow construction of a comparison group based on time-varying as well as time-invariant characteristics.

[1] Some experts suggest that the term "quasi-experimental" should not be used. They argue that an analysis is either experimental or non-experimental. However, we believe that the term "quasi-experimental" conveys a particular type of non-experimental approach.

This paper presents an updated and revised quasi-experimental design that exploits rich longitudinal data sets to estimate near-term net impacts on employment, earnings, and other outcomes for early participants in WIA-funded services in (up to) nine states now participating in the Administrative DAta Research and Evaluation (ADARE) Project: California, Florida, Georgia, Illinois, Maryland, Missouri, Ohio, Texas, and Washington. It also provides specific data elements required for conducting the analysis.

Approach

This section describes the quasi-experimental approaches we will use to produce our net impact estimates, as well as the target groups, key program outcomes and time periods, and control variables for our analysis.

Quasi-Experimental Methods. The methodology proposed to estimate net program impacts will compare individuals who received certain specified services under WIA or under the Employment Service to individuals who registered with WIA or ES in the same quarter but exited after receiving different, and usually more limited, services. We will do so in a series of five different treatment/comparison group combinations as follows, each of which is described in more detail later in the paper:

Number	Treatment group	Comparison group
I.	*Net impact being estimated "quasi-experimentally" is assignment to WIA*	
	Received WIA services 7/00 to 6/02	Received ES services 7/00 to 6/02
II.	*Net impact being estimated "quasi-experimentally" is WIA assignment to training*	
	Received WIA training services, 7/00 – 6/02	Received WIA core or intensive services, 7/00 – 6/02
III.	*Net impact being estimated "quasi-experimentally" is WIA training vs. ES training referral*	
	Received WIA training services, 7/00 – 6/02	Referred to training services by ES, 7/00 – 6/02
IV.	*Net impact being estimated "quasi-experimentally" is training (or referral to training) vs. not receiving training and not being referred to training*	
	Received WIA training services or referred to training services by ES	Received WIA core or intensive services or received services, except for training referral, from ES,

7/00 – 6/02	7/00 – 6/02

V. Net impact being estimated "quasi-experimentally" is assignment to WIA intensive and/or training services

Received WIA intensive or training services, 7/00 – 6/02	Received WIA core services or received services at ES, 7/00 – 6/02

Because individuals were not randomly chosen to participate in their respective states, there may be systematic (nonrandom) differences between the recipients of intensive and training services and the individuals to whom they will be compared. The statistical estimators used to calculate the net impacts must attempt to control for those differences in order to produce unbiased impact estimates.

Two problems must be resolved. First, a comparison sample must be selected, and second, a net impact estimator must be chosen and calculated. Note that Heckman, Lalonde, and Smith (1999) point out that the technique used for constructing the comparison group sometimes precludes certain impact estimators. We will first discuss the construction of the comparison samples, and then the net impact estimators.

Comparison samples. The basic problem that has to be solved is how to choose the appropriate observations from the data set that will be used to extract the comparison samples for the programs being examined. The question is which observations in the WIA or ES (Wagner-Peyser) data systems are most comparable to those who received the particular "treatment," i.e., services in question under WIA or Wagner-Peyser.

Let *T* (for treatment) denote the administrative data for the individuals who will comprise the treatment group for one of the groupings (I – V above) during the period of interest. Let *U* (for universe) denote the set of observations from which we will choose the subset *C* (for matched comparison group) that will be used in the net impact analyses.

The idea is to have *C* be comprised of the observations where individuals are most 'like' the individuals comprising *T*. Because the program data come from either the WIA or the ES systems, all of the requisite variables are in the data sets, including age, race/ethnicity, education at program entry, disability status, ESL status, gender, region of state, and veteran status. Prior

employment and earnings history can be accessed for both comparison and treatment group members from UI wage records linking via SSNs.

There is a substantial and growing literature on how to sample individuals to construct the comparison sample.[2] The first candidate approach is *cell-matching algorithms*. Variables that are common to both data sets would be used to partition (cross-tabulate) the data into cells. Then for each treatment observation, the cell would be randomly sampled (with or without replacement) to select a comparison group observation. A substantial drawback to cell-matching is that the cross-tabulation of data, if there are many common variables, may result in small or empty cells.[3]

More sophisticated comparison group construction can be accomplished with *nearest-neighbor algorithms*. These algorithms minimize a distance metric between observations in T and U. If we let X represent the vector of variables that are common to both T and U, and let X_j, X_k be the values of X taken on by the jth observation in T and kth observation in U, then C will be comprised of the k observations in U that minimize the distance metric $*(X_j \text{ B } X_k)*$ for all j.[4] This approach is very mechanistic, but it does allow use of all of the X variables.

In his work on training program evaluation, Ashenfelter (1978) demonstrated that participants' pre-program earnings usually decrease just prior to enrollment in a program. This implies that a potential problem with the nearest-neighbor approach is that individuals whose earnings have 'dipped' might be matched with individuals whose earnings have not. Thus, even though their earnings *levels* would be close, these individuals would not be good comparison group matches.

An alternative nearest-neighbor type of algorithm involves *parametric modeling*. This approach would rely on propensity scores to estimate the likelihood of individuals' receiving intensive or training services (Dehejia and Wahba, 1995). Essentially, observations in T and U are pooled, and the probability of being in T would be estimated using logistic regression. The

[2] See Heckman, Lalonde, and Smith (1999) and references cited there.
[3] King et al. (1994) used a variation of this approach.
[4] The literature usually suggests that the distance metric be a weighted least squares distance; $(X_j \text{ B } X_k)\text{N}\Sigma^{\text{B}1}(X_j \text{ B } X_k)$, where $\Sigma^{\text{B}1}$ is the inverse of the covariance matrix of X in the comparison sample. This is called the Mahalanobis metric. If we assume that the X_j are uncorrelated, then this metric simply becomes least squared error.

predicted probability is called a propensity score. Treatment observations are matched to observations in the comparison sample with the closest propensity scores.

The whole reason for matching is to find similar observations in the comparison group to those in the treatment group when the 'overlap' or statistical support is weak. Consequently, some evaluators adjust the nearest-neighbor approach to require that the distance between the observations that are paired be less than some criterion distance. This is called *caliper or radii matching*.

Heckman et al. (1997) discuss an alternative matching technique that uses more than a single observation in the comparison group. *Kernel matching* effectively selects all of the observations in the comparison pool that are "near" (within an arbitrarily set "bandwidth") the treatment observation, but weights the selected comparison observations so that the closest observations receive the highest weights. In a "horse race" that compared matching techniques to experimental results, these researchers found that all of the matching techniques that they tried resulted in bias that was not statistically significantly different from 0. However, they found that a kernel matching technique using characteristics (*not* propensity scores) with regression-adjusted weights calculated by local linear regression tended to have the smallest bias.

Following the work of Hollenbeck and Huang (2002) with Washington State data, Mueser et al. (2003) with Missouri data, and our prior work (Hollenbeck et al. 2003) with multi-state data for the ADARE project, we propose to rely mainly on nearest-neighbor, propensity score pairwise matching with regression-adjustment, although we will be doing some experimentation with different matching techniques. One variant from prior work (specifically, the work Washington State data reported in Hollenbeck and Huang, 2002) that we will pursue is to disaggregate the samples by race and sex, so that the pairwise matching will be exact for those characteristics. That is, we will estimate separate logits for four race/sex groups: white/nonwhite and men/women. Then, we will conduct propensity score matching within those groups. We plan to carry out exact matching on several variables: quarter of registration, labor market (workforce) area, and gender.

<u>Issues</u>. With all comparison sample-matching options, there are at least three issues to confront in their precise implementation. First, should limits be considered about how 'close'

the nearest-neighbors must be in the comparison sample? That is, some observations in the treatment group may not match very closely with *any* comparison group observation. As we conduct the match, we will calculate distributional statistics about the 'distances.' We will then consider whether outliers should be 'un-matched,' i.e., deleted from the sample. A second issue is whether the comparison group should be sampled with or without replacement. The tradeoff here is that if we sample with replacement, then we will get better match quality on average, but if we use multiple observations from the comparison sample, then the standard errors for the net impact analyses will have to be adjusted to reflect the fact that observations are repeated. Finally, some data items that are used in the matching process may have missing values.

Implementation of the propensity score approach requires each observation in the participant administrative data and in the potential comparison group sample data to have a complete set of attributes and outcome variables from which to calculate the propensity score.

Net Impact Estimators. Once we have constructed the *T* and *C* matched samples, we will estimate the net impacts of the program. There are many potential estimators that have different properties and that make different assumptions about the data. Let us start the discussion with simple (unconditional) differences in outcome means. This nonparametric approach suggests that the net impact can be fully estimated by averaging the differences in outcomes for each individual matched pair of observations in *T* and *C*. (The sums that go into the averages may have to be weighted if weighted comparison group matches are pursued.) Suppose that average quarterly earnings is one of the outcome variables of interest. Then the net impact per participant would be estimated as follows:

(1) $$Y = \sum_j \{(ET_j - EC_j)\} / n$$

where ET_j = the average quarterly earnings (adjusted to constant $) after exiting intensive or training services for the *j*th individual

EC_j = the average quarterly earnings (adjusted to constant $) after the appropriate program year for the individual(s) in the comparison group matched to *j*

n = the number of individuals receiving intensive or training services

This estimate of the program's net impact may be biased if the propensity score approach does not produce matches that have well-matched pre-program levels of earnings. For example, suppose the administrative data reflect Ashenfelter's earnings 'dip' and that the propensity score approach results in matches that over-weight pre-program year earnings. Then, the unconditional difference in means may overestimate program impact. A second approach would be to estimate difference-in-differences in (unconditional) means. That is, for programs that have sufficient pre-program earnings data, we can estimate the program's net impact as follows:

$$(2) \qquad Y = \sum_j \left\{ \left(ETPOST_j - ETPRE_j \right) - \left(ECPOST_j - ECPRE_j \right) \right\} / n$$

where, *ECPREj*, *ETPREj* = average quarterly earnings of the *j*th individual (and his/her match) prior to being served

ECPOSTj, *ETPOSTj* = average quarterly earnings of the *j*th individual (and his/her match) after participating

This nonparametric approach makes few assumptions about the data or earnings mechanisms.

The estimator in (2) obviously relies on longitudinal data for the outcome variables of interest. For adult and dislocated worker programs, we are likely to have extensive pre- and post-program data on outcomes such as quarterly earnings or income maintenance program participation and benefits. However, were we to tackle net impact estimation for youth programs, pre-program earnings and related data may be lacking, and so the difference-in-differences estimator (2) may not be feasible.

The difference-in-means approaches in equations (1) and (2) assume that the matching technique has eliminated differences between the matched pair of observations that may affect program outcomes. This is a strong assumption with a non-experimental approach to net impact estimation. Consequently, we may wish to estimate regression-adjusted differences in means. This parametric approach assumes that we can use observed variables to control for differences between the matched pairs of observations. A simplified regression model is displayed in the following equation:

$$(3) \qquad Y_j = a + B\mathrm{N}\, X_j + c\, T_j + u_j$$

where, Y_j = outcome for individual *j* (or for the individual(s) matched to individual *j*); that is $ETPOST_j$, or $ECPOST_j$

X_j = vector of variables describing individual j (or for the individual(s) matched to individual j) that are thought to be correlated to the outcome Y_j
T_j = 1 if individual j is in the participant sample and 0 if not
u_j = error term, usually assumed to have a mean of 0 and standard deviation of 1.

The parameter estimate, c, would be the net impact of participation in the program.

The regression-adjusted model in equation (3) needs to be discussed very carefully to understand the underlying assumptions. First, some of the X_j may be used in constructing the matched sample. We have assumed in those cases that the X variables are important in determining program eligibility (matching process) and in determining program outcomes (equation (3)). If all of the X variables were used in the matching process and in the outcome equations, and if all of the matches were 'perfect,' e.g. a 29-year old male with seven years of work experience in a specific industry is matched with a 29-year old male with seven years of work experience in the same industry, then c will be equal to the net program impact estimator in (1). As mentioned, the reason to estimate (3) is because the non-experimental approach is likely to have 'imperfect' matches.

Equation (3) can be estimated for outcomes when there are no longitudinal pre-program observations. With appropriate pre-program data, then we would make (3) slightly more general, as in (3=).

(3=) $$Y_j = a + B'X_j + C'(X_j * Post) + dT_j + e(T_j * Post) + u_j$$

where, $Post$ = 1 for quarters of data that occur after individual j has left; 0 otherwise.

This specification has a fixed effect for program participation, d, and the per-participant net impact estimator is e. Another advantage to specification (3=) is that we can use pre- and post-changes in the outcomes as dependent variables. That is, the Y_j could equal ($ETPOST_j$ B $ETPRE_j$) or ($ECPOST_j$ B $ECPRE_j$).

Finally, we refer to (3) or (3=) as *simple* models because they assume that the Xs affect the outcome variable in an identical manner for both participant and comparison individuals. The participation net impacts, c or e, simply 'shift the intercept.' A more general model that gets at the spirit of subgroup analysis discussed below would have the participation variable interact with the X variables, as in (3@):

3@) $$Y_j = a + B'X_j + CT_j + d(T_j * X_j) + u_j$$

Depending on whether the X variable is continuous or discrete, the net program impact becomes a combination of c and $d*X$.[5]

The propensity score approach to matching observations and the process of regression-adjusting net impact estimates imply that differences between the program participants and individuals in the comparison group are observable (in the X=s). It may be the case that there are systematic differences that are unobservable. Evaluators attempt to adjust for systematic bias in unobservable characteristics by estimating equations for program participation. One such method is called instrumental variables. In this approach, two equations are estimated as follows:

(4a) $$P_j = f(X_j, Z_j) + v_j$$

(4b) $$Y_j = a + B'X_j + c\,\hat{P}_j + u_j$$

Here we assume that there are variables, Z_j, that are correlated with participation in the program, but are uncorrelated with the outcome variable Y_j. These are called instrumental variables. We use them to estimate whether or not an observation participated in intensive or training services and then use the predicted value of P_j in the outcome equation. For example, the distance in miles that an individual lives from a "one-stop" center, may be an instrument for earnings outcomes from the WIA program. That variable is likely to be strongly negatively related to participation, but arguably, should have no impact on quarterly earnings afterwards. (This is a strong assumption.) Similarly, parent participation in welfare programs may be correlated with participation in WIA intensive or training services, but arguably, should not be associated with post-program quarterly earnings. In this approach, the parameter, c, is the per-participant net impact.

Finally, Barnow, Cain, and Goldberger (1980) present a variant on the instrumental variable approach. They suggest that a first-stage participation equation can be estimated as in equation (4a) from which we can calculate an inverse Mills ratio for each observation. The

[5]With longitudinal data, we can generalize equation (3@) by adding interaction terms between T_i and *Post*, and between X_j, T_i, and *Post*.

inverse Mills ratio for each observation is included in (4b) rather than the predicted probability of participation as in the instrumental variable approach. With this approach, the net impact per participant is then the average of the sum of the differences in the predicted outcomes from (4b).

Subgroup/Distributional Analyses. An important element of a net impact evaluation is to examine impacts for subgroups of the population. All of the net impacts presented in the above equations represent an 'average' net impact. But it is almost certainly the case that the impacts will have considerable variation across individuals. Some participants may experience very large improvements, whereas others may have almost none. The question is whether that variation is systematic. Are there particular groups within the participants who benefited more or less than others? Net impacts may differ by whether the participants received intensive or training services, by whether they exited before completion, by whether they earned a credential, and by types of barriers to employment. Other subgroups of natural interest would be different regions of the states and different educational backgrounds.

Our proposed approach is to examine subgroups and distributions of impacts in several different ways. First, we will estimate net impacts for subgroups of interest, i.e., omitting observations that are not in the subgroup. This may be difficult to accomplish if the sample sizes for particular subgroups limit the precision of the estimated impacts. A second approach is to include the specific variables of interest that define a subgroup into the regression models. In effect, the treatment dummy variables can be replaced by a vector of variables such as services received, program completion, and others.

Target Groups. We will develop separate impact estimates for each of two target groups (with the comparison group drawn from those participating in and exiting from the specified WIA or ES services). Two sample tables for presenting the net impact results are attached. Key target groups for this analysis are:

- Adults
- Dislocated Workers

While Youth represent a very important target group for WIA, it is unlikely that a quasi-experimental approach would be able to provide reliable, unbiased impact estimates for them for several reasons. One of the most important of these is that few, if any, younger (14-18 year old)

youth will have established an employment history under the UI system with which to match on prior labor market experience. In addition, many of these youth may still be in the process of completing their education. Even experimental methods have had difficulty producing good estimates of employment and earnings impacts for youth (e.g., Orr et al. 1995).

Time Periods. The observations in the data to be analyzed will have received services from the WIA program or will have received services from the Employment Service (ES). (If an observation happens to be in both data sets, it be assigned to the WIA program). Both WIA and the ES offer services that may be classified as Core services, Intensive services, or Training services. In day to day program operations, we know that entry into a service is not instantaneous with referral to that service. However, for analytical purposes, we will assume that entry will be instantaneous—registration date will be the first date of services received and, similarly, referral date will be the first date of service delivery. To provide an exact specification for the five quasi-experiments, we have chosen the two-year time period of 7/00 – 6/02, which we will refer to as the analysis period. The primary source of outcome data is the UI wage record system. To get reasonable measures of outcome variables, we need to have at least four quarters of wage record data after individuals exit from the program, so our analyses will be limited to 2001 and 2002 WIASRD data and data from the ES for individuals who exited as of 6/30/2002.

Treatment and Comparison Groups. Each of the five "quasi-experiments" alluded to above has slightly different treatment and comparison groups. The first quasi-experiment is intended to determine the net impact of being assigned to WIA. The treatment group will be comprised of records from the 2001 and 2002 WIASRD data sets – all of these cases will have a registration date before 6/30/02 and an exit date is after 7/01/00. (The individual was "active" in WIA at some time during the analysis period.) The pool of records from which we will select the comparison group will be comprised of records from the ES management information system for which the service intervention period overlaps with the analysis period (again, registration date is before 6/30/02 and exit date is after 7/01/00.). Call the treatment group the WIA Universe and the pool for the comparison group, the ES Universe.

The second quasi-experiment is intended to estimate the net impact of receiving training

services for WIA clients. The treatment group will be comprised of records from the WIA Universe for which "the date of first training services" (element 333) has a valid (non-missing) value, which is prior to 6/30/2002. The comparison group pool will be the remainder of the WIA Universe (did not receive training.)

The third quasi-experiment will estimate the net impact of WIA training services for individuals who receive training. The treatment group will be identical to the treatment group for the second quasi-experiment. The pool of records from which the comparison group will be drawn, however, will be only those individuals from the ES Universe whose training referral date is prior to 6/30/2000.

The fourth quasi-experiment is intended to estimate the net impact of receiving or being referred to training whether through the WIA or ES systems. The treatment group will thus be the union of the treatment group and the comparison group pool in the third quasi-experiment (WIA training date or ES training referral date precedes the end of the analysis period.) The universe for the comparison group selection will be comprised of all other records in the WIA Universe or ES Universe.

The fifth and last quasi-experiment is intended to estimate the net impact of receiving WIA intensive or training services. The treatment group is comprised of observations with the WIA date of first training prior to 6/30/2002 or "date of first intensive services" (element 332) prior to 6/30/2002, the last date of the analysis period. The data set from which the comparison group will be selected is comprised of the records from the ES Universe and other WIA records, where the individual only received core services.

Matching. In each of these quasi-experiments, the pool of records from which the comparison group will be drawn is larger than the treatment group, and there are reasons to suspect that the individuals comprising the treatment groups are observationally and non-observationally different from the individuals comprising the comparison groups. We propose to use matching algorithms, as specified above, to identify subsets of the comparison group pools that seem to best "match" the treatment groups. In general, this means that the data sets with the treatment and potential comparison set observations will be merged and participation models (the dependent variable is set to one if the individual is in the treatment group; 0 otherwise) will be

estimated econometrically.

Program Outcomes. Key program outcomes of interest are categorized into labor market and additional outcomes. Labor market outcomes are measured by available UI wage records over the post-exit period. The *labor market outcomes* are:

- Ever employed, defined as having as least one quarter after exit with at least $100 in quarterly earnings,
- Employment**,** defined as percent of post-exit quarters with at least $100 of quarterly earnings;[6]
- Earnings, defined as mean post-WIA quarterly earnings;[7] and
- Turnover, defined as number of different employers in post-exit quarters divided by number of quarters with earnings.

In the difference-in-differences approaches, we need to specify a base period. In Washington, Hollenbeck (2002) uses the averages of pre-program entry quarters 3 through 6 as the base period. The logic behind this choice is that pre-program quarters 1 and 2 may be subject to pre-program labor market distress — Ashenfelter's earnings 'dip' — so that these four quarters (quarters -3 through -6) may be the annual period closest to program entry for which pre-program circumstances can be calculated accurately.

We may also examine a number of additional outcomes, depending on the availability and accessibility of data for measuring them in each of the participating states on a timely basis. *Additional outcomes* may include:

- Educational outcomes such as the following:

--Enrollment in postsecondary education or training since exit

--Type of postsecondary educational institutions/programs (adult education, apprenticeship, community/technical college, 4-year college or university)

--Full-time/part-time status

[6] We will impose a $100/quarter minimum earnings figure for counting whether an individual was employed.
[7] In the net impact analyses for Washington State, Hollenbeck (2002) found that earnings in the first post-exit quarter may be truncated, so we will likely omit that quarter from the averaging.

--Attainment of a postsecondary degree or certificate;

- Receipt of unemployment compensation benefits (number or percent of quarters with spell and average weekly benefit amount in quarters)

- Receipt of public assistance, including TANF, Food Stamps, and Medicaid (number or percent of quarters with benefit receipt and average benefit received in a quarter for TANF and Food Stamps; number of quarters with Medicaid eligibility.)

Control Variables. Time-invariant control (in italics) and pre-program variables that will be used in our analysis include *gender, race/ethnicity, educational attainment, veteran status, disability status, limited English proficiency,* welfare receipt, and employment and earnings history. An issue to be addressed will be how to summarize robustly the employment and earnings histories of individuals in the specification of the propensity logit. In Washington, Hollenbeck (2002) has found that the following five summary variables work well: percent of quarters employed since entering employment, conditional average earnings (pre-program), trend in earnings levels (constant dollars), variance in earnings levels, and average number of employers per quarter.

Data Sources and Required Data Elements

Major data sources for estimating the net impacts of training services are summarized here. They draw upon the new WIA Standardized Record Data (or WIASRD), Employment Service (ES) management information system data, UI wage records, as well as welfare, education, and other administrative records.

WIASRD. Stevens (2002a, 2002b) has described the available WIASRD data that we will be using for this and related analyses. Table 3 lists the data elements we will require from WIASRD for estimating WIA impacts.

ES Data. The ADARE Consortium has not used ES client data in previous or ongoing studies, although various project members have considerable experience with these data in their individual states. There is no standard formatting of these data, so the control and outcome variables from each state's data will need to be extracted using procedures unique to each state.

Furthermore, we may need to recode variables that may have different coding scheme across the states. Most states have a common intake application, and so the descriptive information about clients at that time should be consistent with the variables on WIA. It has been our experience that states have quite detailed service codes; however, we suspect that date of referral to training, which is necessary to form the treatment or comparison groups as described above, should be easily identified.

UI Wage Records. We will utilize linked UI wage records as the basis for measuring labor market outcomes. As we have done in our past and continuing research, we will secure the following UI wage data:

- Quarterly earnings for all employers for each quarter employed in the post-exit quarters, during the quarters that are coincident with receiving program services, as well as — *at a minimum* — 8 quarters of pre-WIA registration data. We prefer that each site secure 12-20 quarters of pre-WIA data if they can be obtained at a reasonable cost.
- Standard Industrial Classification (SIC) [or North American Industrial Classification System (NAICS)] codes for each employer of record.
- Employer identification number for each employer of record[8].

Welfare, Education and Other Records. Each of the partners already has ready access to longitudinal AFDC/TANF, Medicaid, and related records in their state. We may also want to pursue access to Food Stamp records; Texas and Washington have such data now. Maryland, Missouri, Texas and Washington also have accumulated substantial, student-level secondary and postsecondary education data that will be used to examine educational outcomes provided we receive the proper approvals from the U.S. Department of Education and appropriate state officials.

[8] These data are necessary to develop our proposed measure of turnover, which is number of different employers in the post-ext period divided by the number of quarters. Some states in the consortium may not want to supply employer identification numbers and/or may wish to supply linked wage record data with earnings, in a quarter, aggregated across employers. In these cases, the states need to supply the number of different employers in a quarter, and an indicator variable that denotes a change in employers. Our measure of turnover will slightly overestimate true turnover if there are changes in firm ownership or name, but no change in employment at that location.

Future Deliverables and Timelines

Future deliverables and their timelines under Evaluation Project Three are as follows:

- Data acquisition from each partner site, by June 15, 2004;
- Preliminary impact estimates, by July 30, 2004; and
- Final impact estimates, by September 30, 2004.

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Table 1: WIA Service Impacts, By Target Group, Service Strategy & Gender

Target Group & Service Strategy	**Males**	**Females**
Adults		
Core Services		
Intensive Services		
Training Services		
Dislocated Workers		
Core Services		
Intensive Services		
Training Services		

Table 2: WIA Service Impacts, By Target Group, Service Strategy & Race/Ethnicity

Target Group & Service Strategy	**White**	**African-American**	**Hispanic**	**Other**
Adults				
Core Services				
Intensive Services				
Training Services				
Dislocated Workers				
Core Services				
Intensive Services				
Training Services				

Table 3: WIASRD Data Elements Required* for Evaluation

WIASRD Element Number	Element Name	Description**
INDIVIDUAL/ DEMOG. INFORMATION		
Uncertain	Social Security Number	10-digit number; *essential for UI wage records links*
100	ID Number	Unique WIA ID number
102	Date of Birth	8-digits (YYYYMMDD)
103	Gender	Male/Female
104	Disability Status	3 codes
105	Hispanic/Latino Ethnicity	Yes/No
106-110	Race	5 categories
111-114	Veterans Status	4 veteran statuses
115	Employment Status at Registration	Employed/Not
116	Limited English Proficiency	Yes/No
117	Single Parent	Yes/No
118	Unemployment Compensation at Registration	Eligible claimant referred/not by WPRS, UI exhaustee/not
119	Low-income	Yes/No
120	TANF Recipient at Registration	Yes/No
121	GA Recipient at Registration	Yes if GA, RCA, SSI/SSA
122	Pell Grant Recipient	Yes/No
123	Highest Grade Completed	Years of education, HS diploma, GED, BA, Ed>BA
124 (DWs only)	Displaced Homemaker	Yes/No
125 (DWs only)	Date Actual Qualifying Dislocation	8-digit (YYYYMMDD)
126 (Youth only)	Homeless/Runaway Youth	Yes/No
127 (Youth only)	Offender	Yes/No
128 (Youth only)	Pregnant or Parenting Youth	Yes/No
129 (Youth only)	Youth Needing Additional Assistance	Yes/No

*Shaded items *not* required.

**USDOL, ETA, *TEGL No. 14-00, Change 1, Attachment E* (November 19, 2001) has details.

WIASRD Element Number	Element Name	Description
130 (Youth only)	Education Status at Registration	HS Student or less, Postsec. Student, Not attending HS dropout or graduate
131 (Youth only)	Basic Literacy Skills Deficiency	Yes/No
ACTIVITIES & SERVICES		
301	ETA-assigned Board Code	Complex, state variation
302	Date of WIA Title I-B Registration	8-digit (YYYYMMDD); complex, state variation
303	Date of WIA Exit	8-digit (YYYYMMDD); hard or soft exit possible
304	Adult (Local)	Yes/No, local WIA adult funds
305	Dislocated Worker (Local)	Yes/No, local WIA DW funds
306	Youth (Local)	Yes/No, local WIA youth funds
307	Youth (Statewide 15% Activities)	Yes/No, statewide 134 funds
308	Displaced Homemaker (Statewide 15% Activities)	Yes/No, DHs served with statewide 134 funds
309	Incumbent Worker (Statewide 15% Activities)	Yes/No, IWs served with statewide 134 funds
310	Other Statewide 15% Activities	Yes/No, others served with statewide 134 funds
311-312	Rapid Response	Yes if WIA 134 RR funds
313a-c	National Emergency Grants	
314	Adult Education	Yes
315	Job Corps	Yes
316	Migrant/Seasonal FW	Yes
317	Native American Programs	Yes
318	Veterans Programs	DVOP/LVER or WIA 121 program services
319	TAA	Yes
320	NAFTA/TAA	Yes
321	Vocational Education	Yes
322	Vocational Rehabilitation	Yes

WIASRD Element Number	Element Name	Description
323	Wagner-Peyser	Yes
324	WtW Participant	Yes
325	CSBG E&T Program	Yes
326	HUD E&T Program	Yes
327	Title V/Older Americans	Yes
328	Food Stamp E&T Program	Yes
329	Other Non-WIA Programs	Yes
330	Supportive Services Received, except needs-related payments	Yes/No
331	Needs-related Payments (Adults, DWs), Stipends (Youth)	Yes/No
332	Date First Intensive Service	8-digit (YYYYMMDD)
333	Date First Training Service	8-digit (YYYYMMDD)
334	Established ITA	Yes/No
335	Adult Ed, Basic Skills and/or Literacy Activities	Yes/No (WIA 134)
336	OJT	Yes/No
337	Occupational Skills Training, Skills Upgrading/Retraining and/or Workplace Training	Yes/No
338	Occupational Skills Training Code	6-digit SOC code, 8-digit O*Net 3.0 code, 9-digit DOT code, 5-digit OES code, or 5 or 6-digit O*Net code best describing training; 999999999 if no training
339	Occupational Skills Training Code Type	7 categories as above
Youth Services		
340	Educational Achievement Services	Yes/No
341	Employment Services	Yes/No for paid/unpaid work experience, internships, and occupational skills training
342	Received Summer Youth Employment Opportunities	Yes/No

WIASRD Element Number	Element Name	Description
343	Added Support for Youth Services	Yes/No for adult mentoring, comprehensive guidance counseling
344	Leadership Dev. Opportunities	Yes/No
345	Received Follow-up Services	Yes/No
PROGRAM OUTCOME INFORMATION	**Keyed to the Exit Quarter**	
Adult, Dislocated Workers & Older Youth		
601	Employed in 1st Post-Exit Qtr	Yes/No, Not Available
602	Source of Supplemental Data	1=Case Mgt, 2=Record Sharing
603	Occupational Code	See 338
604	Occupational Code Type	See 339
605	Entered Training-related Employment	Yes/No, 8=Non job-specific skills, 9=Employment Relationship to Training
606	Method Used to Determine Training-Related	
607	Entered Non-Traditional Employment	Yes/No
608	Employed 3rd Post-Exit Qtr	Yes/No, Not Available
609	Source of Supplemental Data	See 602
610	Employed 5th Post-Exit Qtr	Yes/No, Not Available
611	Source of Supplemental Data	See 602
612	Earnings 3rd Pre-Registration Qtr	00000.00, or 99999.99 if N/A yet, 88888.88 if >$99,998
613	Earnings 3rd Pre-Dislocation Qtr	For dislocated workers. See 612
614	Earnings 2nd Pre-Registration Qtr	See 612
615	Earnings 2nd Pre-Dislocation Qtr	See 613
616 - 620	Earnings 1st – 5th Post-Exit Qtr	See 612
621	Type of Recognized Education/ Occupation Certificate/ Credential, Diploma/ Degree Attained	8 codes

WIASRD Element Number	Element Name	Description
622	Other Exit Reasons	Institutionalized, Health/ Medical, Deceased, Reservists Called to Duty
623	In Postsec. Educ. or Advanced Training in 1st Post-Exit Qtr	Advanced Training, Postsec. Education, or Not
624	In Postsec. Educ. or Advanced Training in 3rd Post-Exit Qtr	See 624
Younger Youth (14-18 yrs.)		
625 - 676	Various Younger Youth Outcomes	