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REPORT

WORKFORCE INVESTMENT ACT (WIA) ADULT AND DISLOCATED WORKER PROGRAMS GOLD STANDARD EVALUATION

30-Month Follow-Up Survey Extension Request

Part B: Collection of Information Involving Statistical Methods

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WIA GOLD STANDARD EVALUATION 30-MONTH FOLLOW-UP SURVEY EXTENSION REQUEST

PART B: COLLECTION OF INFORMATION INVOLVING STATISTICAL METHODS

The U.S. Department of Labor's (DOL) Employment and Training Administration (ETA) is currently undertaking the Workforce Investment Act (WIA) Adult and Dislocated Worker Programs Gold Standard Evaluation (The Evaluation). Although WIA was replaced by the Workforce Innovation and Opportunity Act of 2014 (WIOA), in July 2014, the Adult and Dislocated Worker programs continue to exist and offer job seekers a similar set of services. Lessons learned from this evaluation will inform policymakers and program administrators as WIOA is implemented.

The overall aim of this evaluation is to determine whether certain adult and dislocated worker services and training funded by Title I of WIA, and now Title I of WIOA—currently the largest source of Federal funding of employment and training services—are effective and whether their benefits exceed their costs. ETA has contracted with Mathematica Policy Research and its subcontractors—Social Policy Research Associates, MDRC, and the Corporation for a Skilled Workforce—to conduct this evaluation. The evaluation was launched in 28 randomly selected Local Workforce Investment Areas (LWIAs) starting in November 2011, and all sites began intake of customers into the study by August 2012.

This will be the third clearance package submitted to the Office of Management and Budget (OMB) for this evaluation. An initial data collection package, approved by OMB in September 2011 (OMB Control Number 1205-0482, Information Collection Reference (ICR) Number 201101-1205-001), requested clearance for a form to check the study eligibility of the customer, a customer study consent form and the collection of data at baseline through a study registration form and contact information form, as well as site visit guides for the collection of qualitative information on WIA program processes, services, and training. A second data collection package was approved on January 18, 2013 (OMB Control Number 1205-0504, ICR Number 201208-1205-012) to allow for the collection of additional qualitative data in order to analyze veterans' experiences in the 28 selected LWIAs, two participant follow-up surveys conducted at 15 and 30 months after random assignment, and cost data collection. In March 2015, a nonsubstantive change request was approved by OMB to modify the incentives used for both follow-up surveys (OMB Control Number 1205-0504, ICR Number 201502-1205-001).

This new request is to extend OMB clearance of the final 30-month follow-up survey administration (cleared under OMB Control No. 1205-0504), which currently will expire on January 31, 2016, for an additional six months, to July 31, 2016. This extension will allow additional time to locate sample members for administration of the 30-month survey and will achieve a higher response rate. There are no proposed changes to the survey instrument or the way it is administered.

This package includes:

1. Appendix A: Authorization for Evaluation, Section 172 of WIA and Section 169 of WIOA
2. Appendix B: Study Registration, Consent, and Contact Information Forms
3. Appendix C: 30-Month Follow-Up Survey Instrument, Results of Survey Pretests, and List of Frequently-Asked Questions
4. Appendix D: Letters and Reminders to Survey Sample Members
5. Appendix E: 60-Day Federal Register Notice

1. Respondent Universe and Sampling

One of the main goals of the evaluation is to broadly generalize the findings to the national population of adults and dislocated workers who are served by the program during the period covered by the evaluation. To accomplish this, a two-stage clustered design was employed, first by randomly selecting sites (LWIAs) and then by randomly assigning all adults and dislocated workers (with a few exceptions) who reach the point of being offered intensive services.

a. Site Selection

The evaluation will estimate the impact of intensive and training services funded by WIA (superseded by WIOA) adult and dislocated worker local formula funding. As this funding is administered by LWIAs, the LWIA is the sampling unit for the evaluation.

The sample frame. To construct the sample frame for LWIA selection, a list of all active LWIAs was assembled from the latest two years of the WIA Standardized Record Data (WIASRD) available, which were from April 2006 through March 2008. For each LWIA, these data include the annual number of adults and dislocated worker customers who received intensive services (some of whom also received training) and exited the program (referred to as "exiters"). This average annual number was then multiplied by 1.5 to represent the number of such customers who would be served in an 18-month period. The study includes only persons who were eligible for and sought intensive services. Thus, the 2006 to 2008 counts of exiters were used to construct a sample frame for assessing the likely flow of customers in each LWIA who would be subject to random assignment during the 18-month sample intake period.

In recent years, some LWIAs changed their service receipt definitions so that nearly all American Job Center customers are reported as having received intensive services, even though the intensive service received might be defined as a staff-assisted core service in other areas. These definition changes resulted in large increases in reported intensive service customers in some areas in recent years. For example, in Program Year (PY) 2007, New York received 7 percent of all WIA formula funding, but nearly 20 percent of all customers who were designated as having received intensive or training services. On the basis of this information, ETA decided that random assignment should be conducted at the point when customers start receiving intensive services as defined by most sites.

The population counts in some LWIAs were adjusted to reflect the definition of what constitutes "intensive services." This adjustment moves the point of random assignment later in the WIA service flow process in sites that define intensive service receipt particularly early in the

process. Two approaches were used for identifying these sites: (1) gathering information from the study's advisory panel and evaluation team on LWIAs that were known to have changed their service designations, and (2) identifying large program year increases in intensive service customer counts using recent WIASRD data.

This analysis identified four areas for count adjustments: (1) three LWIAs in Texas, (2) all LWIAs in Oklahoma, (3) the "balance-of-state" LWIA in Indiana (which excludes the Indianapolis LWIA), and (4) all LWIAs in New York. Intensive service customer counts were adjusted downwards in these sites using two approaches: (1) dividing their trainee rates in the years after the definition changes by their typical trainee rates during the years prior to the changes, and (2) using the ratio of WIA funding levels to counts of intensive service customers. The first adjustment was made in all four sets of sites mentioned above. The second adjustment was made on top of the first only for the New York sites, where definitional changes began before our earliest available data, and hence, the first deflation approach alone was insufficient for estimating the number of intensive customers using a common definition. The main implication of these adjustments is that LWIA counts in New York were reduced to about 35 percent of the unadjusted counts. Smaller adjustments were made in the aforementioned sites in Texas, Oklahoma, and Indiana.

In 2006-2008, there were slightly fewer than 600 active LWIAs. The smallest sites—defined as those with fewer than 100 intensive service customers annually—were excluded from the sample frame, as well as sites outside the 48 contiguous states and the District of Columbia. The exclusion of the smallest LWIAs and those outside the U.S. mainland avoided the expenditure of substantial resources on recruiting and supporting the sites with little added to the precision of the impact estimates. Thus, the sample frame included 487 LWIAs representing more than 98 percent of the WIA population of intensive service customers in the mainland United States.

Site selection approach. WIA services vary by region, so that regional balance was a top priority in site selection. Accordingly, the evaluation team explicitly stratified by the six DOL administrative regions and selected sites within each region with probabilities proportional to the size of the site (PPS), where the size of the site was measured by the number of customers who received intensive services. The random selection of sites was conducted without replacement.

The number of LWIAs to select within each region was determined based on the regional shares of the total sample universe. This resulted in the following allocation of sites across the six regions: four sites in Region 1, three sites in Region 2, seven sites in Region 3, five sites in Region 4, seven sites in Region 5, and four sites in Region 6. These allocations reflect (1) the allocation of a "residual site" due to rounding to Region 2 which had only two sites based on their population shares, and (2) one site being added to Region 5 from Region 3 to ensure an adequate representation of large Midwest states.

The New York City LWIA and Gulf Coast Workforce Board LWIA were selected with "certainty" because they each contained a large fraction of the WIA customer population in their regions and so they had selection probabilities of greater than one.

The noncertainty sites were selected without replacement using PPS sampling within the explicit strata defined by the six DOL administrative regions. Within each region, the team implemented the PPS sampling process using systematic sampling, where sites were sorted

(implicitly stratified) in order by (1) whether they are big or small (greater or less than 600 exiters annually), (2) their state, and (3) whether their training rate for the adult and dislocated worker populations (the percentage of intensive service customers who participated in a WIA-funded training program) is greater or less than 50 percent. This approach ensured a diverse set of states within each region, protected against getting many small sites by chance, and ensured a representative distribution of site-level training rates.

After sorting the sites within each region on those three characteristics and then randomly after that (using computer-generated random numbers), the team implemented PPS sampling by first "duplicating" site observations based on the site's size measure (for example, a site with 200 customers contributed 200 observations to the ordered dataset). The team then selected a random starting number for each ordered list. The team first selected for the study the site corresponding to the starting number, and then sequentially selected every Nth site thereafter, where N depended on the desired number of sites to be selected in the region and the total number of observations in the ordered list. For example, if the ordered list for a region had 1,000 site observations, four sites were to be selected, and the 50th observation was the random starting point, then the team selected the sites corresponding to observations 50, 300, 550, and 800 (where N=250).

Using simulations to test the site selection approach. To determine the likelihood that the site selection strategy might fail to generate an adequately representative sample of sites along the desired characteristics, simulations of the site selection approach were conducted prior to sampling. Each simulation was a test run of the sampling procedure, implemented exactly as it would be for the actual selection of study sites. These simulations entailed drawing 2,000 different sets of 30 sites and examining the distribution of sites across the regions that resulted. The distribution of the training rate was also calculated each time. Table B.1 shows the results of the simulations. The second column shows the share of the population in each DOL region and the training rate in the population. The third column shows the mean share of the sample in each region and the mean training rate across the 2,000 simulations. The final three columns show the 10th, 50th, and 90th percentiles in the distributions for each region. (Because the percentiles are shown separately for each region, the columns do not reflect results for a single simulation. Thus, the percentages in each of these columns do not always sum to 100.) The final three columns also show the 10th, 50th, and 90th percentiles for the training rate.

As Table B.1 shows, the distribution of possible site characteristics closely tracked the population distribution, even when relatively low (10th percentile) or high (90th percentile) points in the distribution were considered. Simulations were also conducted for other site selection rules—including selecting sites at random without stratification, using several other stratification schemes, or using sets of sites matched prior to sampling—however, the approach described above generated the closest predicted match to the distribution of site characteristics in the full population while also maintaining a good distribution of sites across states within regions. Most importantly, this approach performed well even if the draw was "unlucky"—other approaches did well on average but were susceptible to draws that, by chance, did not mirror the population characteristics.

Table B.1. Simulated distributions of site characteristics

Characteristic	Population	Simulated sample distribution			
		Mean	10th Percentile	50th Percentile	90th Percentile
Percentage of Population in Administrative Region					
Region 1 (Boston): CT, MA, ME, NH, NJ, NY, RI, VT	14	13	11	12	14
Region 2 (Philadelphia): DE, DC, MD, PA, VA, WV	7	8	6	8	10
Region 3 (Atlanta): AL, FL, GA, KY, MS, NC, SC, TN	26	25	23	25	28
Region 4 (Dallas): AR, CO, LA, MT, ND, NM, OK, SD, TX, UT, WY	17	19	17	19	21
Region 5 (Chicago): IA, IL, IN, KS, MI, MN, MO, NE, OH, WI	21	21	19	21	23
Region 6 (San Francisco): AZ, CA, ID, NV, OR, WA	14	14	12	14	16
Percentage of Those Who Request Intensive Services Who Receive Training	57	55	50	55	59

Source: WIA Standardized Record Data for adult and dislocated worker exiters between April 2006 and March 2008 projected to 18 months. Only LWIAs in the U.S. mainland were included.

Note: Characteristics are weighted by sample size at selected sites.

The selected sites. Table B.2 shows the 30 selected sites, by region. The sample is balanced across regions and has a mix of sites that are large and small and that had high and low training rates. The 30 sites are spread across LWIAs from 21 states, and the sample has 16 sites from the eight states with the largest WIA funding levels (in PY07), including at least one site in each of those eight states. Seventeen of the 30 sites are large (greater than 600 customers annually) and 18 have a high training rate (greater than 50 percent).

Table B.2. LWIA sites selected for evaluation

Region	State	Size	Training rate	Site name
1	NJ	Small	Low	Essex County Workforce Investment Board
1	NY	Large	Low	New York City
1	NY	Large	Low	Albany/Rensselaer/Schenectady Counties
1	NY	Small	High	Chautauqua County
2	PA	Large	Low	Central Pennsylvania Workforce Development Corp.
2	PA	Small	Low	Southwest Corner Workforce Investment Board
2	PA	Small	Low	Northwest Workforce Investment Board
3	FL	Large	High	Region 8, First Coast Workforce Investment Board
3	GA	Small	High	Atlanta Regional (Area 7)
3	KY	Large	Low	Kentuckiana Works
3	MS	Large	High	Twin Districts Workforce Investment Area
3	SC	Large	Low	Lower Savannah Council of Governments
3	SC	Small	High	Santee Lynches Regional Council of Governments
3	TN	Large	High	East Tennessee Human Resource Agency
4	LA	Small	High	Orleans Parish
4	SD	Large	Low	South Dakota Consortium
4	TX	Large	Low	Gulf Coast Workforce Board-The WorkSource
4	TX	Large	High	North Central Texas Workforce Development Board

Region	State	Size	Training rate	Site name
4	TX	Small	High	South Plains Workforce Development Board
5	IL	Small	High	Du Page County Workforce Investment Board
5	IN	Large	Low	Indianapolis Private Industry Council
5	MI	Large	High	Thumb Area Michigan Works!
5	MI	Small	High	Muskegon County Department of Employment and Training
5	MO	Small	High	Central Region
5	OH	Large	High	WIA Area 7
5	WI	Small	High	WOW Workforce Development Inc.
6	CA	Large	Low	Fresno County Workforce Investment Board
6	CA	Large	High	Sacramento Employment & Training Agency
6	NV	Small	High	Nevadaworks
6	WA	Large	High	Workforce Development Council of Seattle-King County

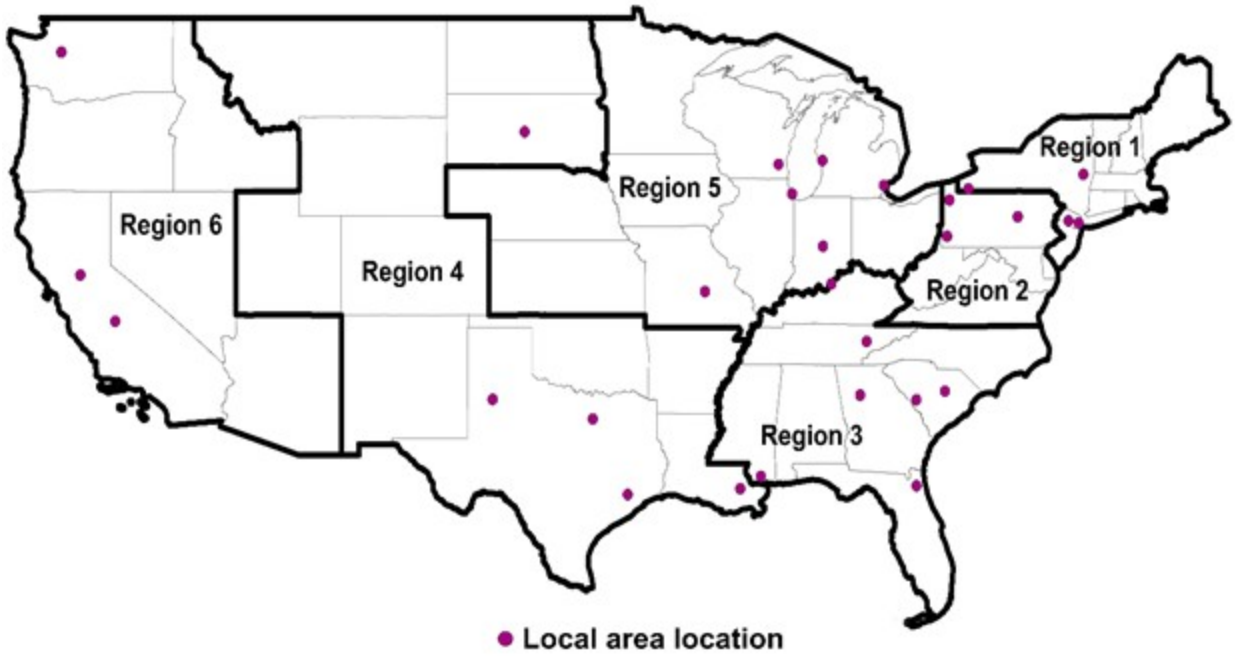
Note: "Small" sites are those with fewer than 600 customers annually, and "large" sites are those with 600 or more annually. "High" and "Low" training rate categorization is based on whether the site's training rate was greater or less than 50 percent.

Recruiting sites. Recruitment activities included letters and calls from the Assistant Secretary of ETA and multiple visits from the evaluators to explain the study. While these visits involved lengthy discussions about the evaluation with senior staff and members of the workforce investment boards, no data were collected during those visits and no WIA customers were contacted.

Following a review of Section 172 of the WIA (see Appendix A) and queries to staff in the Department's Solicitor's Office, ETA concluded that the Department does not have statutory authority to require local workforce investment areas (LWIAs) to participate in the evaluation. Although Section 172 requires the Secretary to "provide for the continuing evaluation of the programs and activities" and directs the Secretary to "conduct as (sic) least 1 multisite control group evaluation," there are no provisions regarding participation in these evaluations by any organization(s). This includes those receiving Federal funding for WIA programs or for providing services to WIA participants.

All but four of the 30 sites that were originally selected (and listed in Table B.2) agreed to participate in the evaluation. Thus 26 of the 30 sites—or 87 percent of the sites, representing 89 percent of the customers in the 30 sites—agreed to participate. The sites that declined to participate in the study were (1) WIA Area 7 in Ohio, (2) Thumb Area Michigan Works!, (3) DuPage County, Illinois, and (4) Nevadaworks. These sites are highlighted in Table B.2. Figure B.1. shows a map of the locations of the LWIAs included in the evaluation.

Figure B.1. Local areas participating in the study



Accounting for sites that choose not to participate. Because the 26 sites that agreed to participate may differ from the four sites that refused to participate in ways that affect the magnitude of the impacts, a potential exists for a bias in the impact estimates. Hence, we conducted a comprehensive sensitivity analysis to address potential nonresponse biases on the impact estimates due to the noncooperation of some sites.

We proposed two approaches for dealing with nonresponse. Our primary approach for assessing the sensitivity of our impact findings to site nonparticipation called for the selection of "matched replacement" sites for each of the four sites that refused to participate (referred to as "refuser" sites). As discussed further below, for each refuser site, we selected the most closely matched replacement sites based on the stratification variables discussed above. Impacts in the replacement sites could differ from those in the initially-selected refuser sites. However, the replacement sites matched well to the refuser sites based on the observable matching data (see below), and thus, formed a reasonable alternative approach for "imputing" missing impact data for customers in the refuser sites. This approach also had the potential for increasing the precision of the impact estimates by increasing the number of study sites. Finally, the inclusion of additional "matched" sites will allow the evaluation to obtain more precise estimates of specific program features, which is an important evaluation objective.

The secondary approach is to statistically adjust for site nonparticipation using information on the characteristics of the 26 sites that agreed to participate and the four sites that refused. As discussed in more detail below in Subsection 2c, this approach will involve adjusting the sample weights for nonresponse using propensity score methods and using multiple imputation methods.

Selection of replacement sites. Replacement sites were selected to be as similar as possible to the refusing sites using the stratification variables discussed above. To do this, when the sites were selected, ordered lists of five replacement sites were also developed for each site.

Replacements were chosen by searching for sites that were of similar size, in the same region, in the same state, and had similar training rates as the originally-selected site. The criteria were prioritized in the order listed. The size of the site was considered the most important feature to match on to ensure sample size targets could be met without drastically changing the rates at which customers were assigned to the restricted services groups.

Importantly, this selection procedure for the replacement sites is similar in spirit to a simple stratification approach that would have called for the allocation and random selection of replacement sites within strata. Our approach is an extreme form of stratification where replacement sites were matched to original sites using the stratification variables. Under either stratification approach, the inclusion of replacement sites in the analysis sample could yield unbiased estimates to the extent that site nonresponse is independent of impacts within the strata. In this case, it is effectively random whether the original or replacement sites were selected "first."

The main advantage of our stratification approach is that it was more likely to yield replacement and original sites that were better balanced on the stratification variables, especially due to small sample sizes. The analogy of our approach in randomized control trial sampling is the use of propensity scoring to first pairwise match sampling units prior to random assignment and then to select one of each pair to the treatment or control group (see, for example, Murray 1998 and Schochet 2008a) or to use minimization to achieve balance for treatment assignments within strata (see, for example, Pocock 1983).

In essence, our replacement site selection strategy used a "model" that minimized differences between the original and replacement sites using the stratification variables that were available at the time of sampling. The replacement sites were selected at the same time as the original sites due to the considerable amount of uncertainty as to when the original sites would make their participation decisions. Thus, in order to obtain a timely sample, we often contacted replacement sites before the original sites made their final decisions.

Recruitment of Replacement Sites. We recruited two replacement sites. The first replacement site for Thumb Area Michigan Works!—Southeast Michigan—agreed to participate. We were required to go to the second replacement site for WIA Area 7 in Ohio—Chicago Workforce Investment Council. The two other sites that declined—DuPage County and Nevadaworks—declined to participate later in the study and were not replaced. Because of the lateness of their decisions, they were not replaced. Table B.3 summarizes our recruitment success.

Table B.3. Success at site recruitment as of June 2011

Selected to participate/agreed to participate	Number of sites	Number of customers who receive intensive services in 18 months in sites
Sites selected originally to participate in the study	30	68,130
Agreed to participate[a]	26	60,811
Did not agree to participate	4	7,319
Replacement sites agreed to participate[b]	2	4,424
Replacement site did not agree to participate[b]	1	8,937
All sites that agreed to participate in the study	28	65,235

[a]The primary analysis sample.

[b]The second replacement site was used to replace one site that refused.

Our primary analysis will include 28 sites—the 26 sites that were originally selected and agreed to participate in the evaluation and the two replacement sites. An important reason for including the two replacement sites in the study is that *three of the four refuser sites were from the Midwest Region;* only four of the seven original sites in this region remained in the 26-site sample. Standard nonresponse adjustments could be applied to adjust for this serious underrepresentation of the WIA population in the large Midwest Region (for example, by giving larger weights to the four sites in this region that are in the 26-site sample). However, another approach to adjust for this potential site-level nonresponse is to include in the sensitivity analysis the two replacement sites that are both in the Midwest Region.

Table B.4 compares the characteristics of the original 26-site samples with the 30- and 28-site samples using the stratification variables used for sampling. The two replacement sites are from the same Midwest region as the two refuser sites and one is in the same state as the site it is replacing. The replacement sites are of similar size to the attrited sites (about 3,000 customers). The training rate was somewhat lower in the replacement sites than their two matched original sites, however, because a lower priority was placed on the training rate in the matching than on the region and size variables. It is interesting, however, that the training rate in the two replacement sites was similar to the overall training rate in the 30- and 26-site samples.

We conducted a sensitivity analysis for the inclusion of the replacement sites. Before using these two replacement sites in the analysis, we compared the impacts in the two replacement sites with the impacts in the four original Midwest sites to examine whether the impacts in the replacement sites were atypical, and conducted F-tests to gauge whether the differences in the impacts were statistically significant. We also used F-tests to compare the 26- and 28-site impact findings. For both the 26- and 28-site samples, we employed statistical adjustments for site nonparticipation (see Subsection 2c).

Table B.4. Stratum characteristics of sites in different samples

Characteristic	Original	Post-attrition	Attrited sites	Replaced sites	Replacement sites	Post-replacement sites
Number of Sites	30	26	4	2	2	28
Region						
1	13.3%	15.4%	0	0	0	14.3%
2	10.0	11.5	0	0	0	10.7
3	23.3	26.9	0	0	0	25.0
4	16.7	19.2	0	0	0	17.9
5 (Midwest)	23.3	15.4	75	100	100	21.4
6	13.3	11.5	25	0	0	10.7
Size Stratum						
1	10.0%	11.5%	0	0	0	10.7%
2	33.3	30.8	50	0	0	28.6
3	26.7	26.9	25	50	50	28.6
4	10.0	11.5	0	0	0	10.7
5	6.7	7.7	0	0	0	7.1
6	10.0	7.7	25	50	50	10.7
7	3.3	3.8	0	0	0	3.6
Average Number of Customers	2,271	2,339	1,830	2,878	3,066	2,391
Percent in Training	55.9	52.7	76.5	74.5	55.5	52.9

We also used WIASRD and Area Resource File (ARF) data to compare the final set of study LWIAs to the 30 randomly selected and to all LWIAs nationwide. (ARF data are collected by the Health Resources and Services Administration and contain detailed information on local area characteristics by county.) This comparison was used to check the extent to which the sites resemble the LWIAs nationwide on observable characteristics. The WIASRD and ARF data were also used to adjust the weights for site nonresponse and to perform multiple imputations.

To the extent that these adjustment methods did not fully capture unobservable differences between site responders and nonresponders that are correlated with study impacts, the impacts estimated in this study will be biased estimates of the impact of the program nationwide. However, the estimates will still be unbiased estimates of the impacts of the program in the sites that participated in the study.

b. Selection of Individuals Within Sites

At each site, nearly all consenting WIA adult and dislocated worker customers who would, in the absence of the study, be offered intensive services were randomly assigned into one of three research groups just after they expressed an interest in receiving intensive services. The three research groups are the (1) full-WIA group—customers in this group can receive any WIA services, including training, for which they are eligible; (2) core-and-intensive group—customers in this group can receive any WIA services for which they are eligible but not training; and (3) core group—customers in this group can receive only WIA core services and no WIA intensive or training services. Thus, this evaluation is examining the impacts of WIA intensive and training services on customers' outcomes relative to a situation in which customers have access to WIA core services only.

In selecting a point of random assignment, we considered the following criteria: (1) the point had to allow customers to receive core services; (2) the point had to allow us to address a meaningful research question and the intervention studied had to be sufficiently large for us to expect to be able to detect its impacts; (3) the point had to be at a similar point in the service flow in each site so we are addressing the same research question in each site; and (4) random assignment at this point had to be operationally feasible.

Selecting the point of random assignment was challenging in this study because the sites differed in their service provision and in their definitions of intensive services. For example, some sites include nearly all staff-customer interactions as intensive services while others include only substantial interviews with employment counselors. Our approach was to define intensive services as services that require "substantial" staff input irrespective of how it was defined by the site.

While many people who use the American Job Center receive only core services, we are not evaluating core services because (1) few sites would agree to turn a customer away from the American Job Center without the offer of some service; (2) the services are typically co-funded by the Employment Service; (3) some services are accessed on-line making it difficult to deny the services; and (4) the impact of these light-touch services is likely to be too small to detect with the sample size feasible for such study. Hence, we are only evaluating the impact of "intensive" services as defined above and training services.

We worked with each site to define substantial intensive services. Site staff helped to define the point of random assignment based on their understanding that the study is attempting to apply a uniform definition of intensive and training services across sites (to the extent possible).

While the terms core, intensive, and training are clear in the legislation and discussed by policy makers, frontline staff are often unaware of the terms and rarely use the term "intensive" services. In our training of staff, we were careful to describe the point of random assignment in terms of the names staff used for services rather than "intensive" services. This prevented any confusion with the different definitions of the terms "intensive" service. We were not asking sites to make any changes to how they recorded the receipt of services in their management information systems. To conduct random assignment, intake counselors input key identifying information on each customer in the study universe into a web-based computer system that was developed by the evaluation team. The web-based system returned random assignment results within seconds. These results were obtained using pre-programmed randomly-generated strings of random assignment statuses. The string length depended on the sampling rates to the core-and-intensive (CI) and the core groups (C), as one CI and one C code were randomly ordered (using computer-generated random numbers) within each string. This process ensured that the selection of the restricted services groups was evenly spread out over the sample intake period.

Administrative records data—including unemployment insurance (UI) records and state or local WIA management information systems (MIS) data—were collected for the full research sample. However, as discussed later, follow-up surveys were conducted only for random subsets of the full research sample using computer-generated random numbers within explicit strata to ensure a balanced survey sample in terms of key population characteristics. To attain a sufficient sample size, the sample intake period spanned 18 months. Around 36,000 people were randomly assigned. Of these, about 2,000 were later found ineligible or withdrew consent.

Research group assignment rates. About 6 percent of customers were assigned to the core group and another 6 percent assigned to the core-and-intensive group. The remaining 88 percent were assigned to the full-WIA group. Keeping the rates of assignment to the restricted-service groups low was important so as not to change program operations and to be more acceptable to the sites. The implemented approach, which involved restricting access to the full set of WIA services to a small portion of the customers in the study, provides sufficient statistical power for the impact analysis (as shown by the minimum detectable impacts shown in response to question 2 below), and is likely to have helped foster sites' cooperation in the study.

Assignment rates to the restricted-service groups that did not have access to full-WIA services differed by the size of the site; the rates were lower in larger sites than in smaller sites. This was necessary to ensure that the customer sample did not consist mainly of individuals from the largest sites. The sampling rate for each of the restricted-services groups—the core group and the core-and-intensive group—varied from less than 1 percent in the larger sites to 17 percent in the smaller sites. By design, the sample was close to "self-weighting." Smaller sites were less likely to be selected under PPS sampling, but conditional on the site being selected, a higher proportion of customers was included in the research sample, such that any given customer in the WIA population was close to equally likely to be selected into the research study. The sample was largely self-weighting both within and across regions. However, the analysis will use sampling weights to correct for any imbalances arising if selected sites represent a smaller or larger proportion of the expected sample than they would of the population.

Sampling for the surveys. Because some important outcomes are not available from administrative sources, two follow-up surveys were/are being conducted with about 6,000 customers. The surveys collect a rich amount of information on sample members' training, training program characteristics and employment and self-sufficiency outcomes.

All adult and dislocated workers randomly assigned to the core-and-intensive or core groups were included in the survey sample. However, only a random subset of about 2,000 full-WIA group members was included. Thus, the survey sample is balanced across the three research groups, with 2,000 people in each of the three groups, yielding more precise impact estimates than would other allocations of the 6,000 customers. The random selection of full-WIA members for the survey sample is stratified by site; within each site, the survey sample size of full-WIA members is the same as the sample sizes for the core-and-intensive and core groups. Stratification on other characteristics was performed to ensure that the sample is balanced in terms of adult/dislocated worker status, sex, and race/ethnicity and was well matched to the core and core-and-intensive services groups on these dimensions.

Sample attrition and response rates. The first potential source of attrition was the refusal of sites to participate (Table B.5). As discussed above, 26 out of the 30 initially-selected sites agreed to participate. The participation rate in terms of individuals is 89 percent (first row of Table B.5).

Table B.5. Assumptions about sample attrition in the evaluation

1. Proportion of all customers in the 30 initially-selected sites that are in the 26 sites that agreed to participate	89%
2. Proportion of all customers in the 26 participating sites who consent to participate in the study and remained eligible for the study	97%
3. Proportion of consenting customers who will respond to the second follow-up survey	82%
4. Proportion of all customers (both consenting and nonconsenting) in the 30 sites who respond to the 30-month follow-up survey	71%
5. Proportion of all consenting customers for whom we receive administrative data	100%
6. Proportion of all customers (both consenting and nonconsenting) in the 30 sites for whom we receive administrative data	86%

The second potential source of attrition from the sample of customers in the participating sites occurred in obtaining consent to participate in the study or customers becoming ineligible for the study. However, we found that 97 percent of all customers agreed to participate in the study and remained eligible for the study (second row of Table B.5).

The third source of attrition is nonresponse to the follow-up surveys. We expect to achieve an 82 percent response rate to the 30-month follow-up survey (third row of Table B.5). (We discuss our approach to obtaining a high response rate in the follow-up surveys in Section 3 that follows.)

The fourth row in Table B.5 shows that we expected that 71 percent of all customers in the 30 initially randomly selected sites would respond to the 30-month follow-up survey. It is calculated as the response rate (82 percent) times the percentage of customers who consent to the study and remain study eligible (97 percent) times the percentage of customers in the 30 sites who are located in the 26 participating sites (89 percent).

Sample attrition in the traditional sense will not occur in the collection of the UI wage records because of the interpretation of nonmatching records. We have sent the social security numbers of all participants in our study to the U.S. Department of Health and Human Services. They have matched the social security numbers with their National Directory of New Hires, which includes data on quarterly earnings collected from state UI agencies. If they find a match, they will return the information about earnings for the quarter on that study participant. If they do not find a match, we will assume that the study participant was not employed and had no earnings in that quarter. Hence, we will have information for every study participant (fifth row of Table B.5). The sixth row in Table B.5 shows the percentage of all customers in the 30 initially randomly selected sites for whom we expect to receive administrative data.

We recognize, however, that the information obtained from UI records could be incorrect. They could be incorrect for several reasons including: (1) the study participant's earnings are not covered by the system (because for example, the participant is self-employed, an independent contractor, or a federal government worker); (2) the employer incorrectly reports the participant's earnings (employers have an incentive to under-report the amount of reported earnings because they affect the payroll tax); or (3) the study participant has given an incorrect social security number. Despite the potential concerns with these data, we proposed to collect

them because when reported, the amount of earnings may be more accurate and there is the potential to collect data for a longer follow-up period without additional burden to the study participants.

Unusual problems requiring specialized sampling procedures. There are no unusual problems requiring specialized sampling procedures for either the WIA Evaluation.

Periodic cycles to reduce burden. The follow-up surveys were conducted twice, at 15- and 30-months after study intake and random assignment to maximize the length of the period in which we can analyze impacts, while also maximizing recall to enhance data quality and reducing the time burden on respondents for each survey[1]. The collection of data on program costs to support the benefit-cost analysis occurred only once.

2. Procedures for the Collection of Information

The evaluation will estimate impacts using a finite-population, design-based approach. Accordingly, study inferences will be generalized to the customer universe from which the research groups will be selected (not to a "superpopulation" of WIA programs and customers). We adopt this approach because WIA services, customer populations, and the local area context (such as unemployment rates) change somewhat over time; thus, policymakers can assess whether the evaluation findings for the full sample and key subgroups pertain more broadly to program superpopulations. The estimated variances of the impacts under this approach will be adjusted for design effects due to clustering and weighting.

The central feature of the evaluation is the random assignment of customers who are eligible to receive intensive services to one of three research groups within each study site. Experimental statistical methods will yield unbiased estimates of the net impacts of WIA as it operated during the study period. For adults and dislocated workers, the net impacts of each service tier can be estimated by comparing outcomes of the (1) full-WIA treatment group and the core-and-intensive group, (2) the full-WIA group and the core group, and (3) the core-and-intensive group and the core group. Impacts will be estimated not only for the full sample, but also for important subgroups defined by customer, program, and site characteristics.

a. Estimating Impacts for the Full Sample

With a random assignment design, there should be no systematic observable or unobservable differences between research groups except for the services offered after random assignment. Thus, for each customer population (adults, dislocated workers, or both combined), simple differences in the mean values of outcomes between customers assigned to any two research groups will yield unbiased estimates of program impacts, and the associated t-tests (adjusted appropriately for design effects due to weighting and clustering) can be used to assess statistical significance.

The study will also use regression estimators to control for residual differences between the treatment and comparison groups and to construct more efficient estimators than the simple difference-in-means estimators. The next sections discuss the variance formulas for these impact estimators under a design-based approach that will be employed for the study.

[1] The 30-month survey is still being conducted.

Differences-in-means estimators. The design for the evaluation is a two-stage stratified design, where n_h sites (referred to as primary sampling units, or PSUs) were selected within region h with probabilities proportional to size, and m_{hig} customers from region-*h* site-*i* were then randomly assigned to research group *g* with the site-specific assignment probabilities discussed above. As discussed, site sample sizes will be selected to yield a sample that is largely self-weighting (but not completely), and there will be no poststratification. Thus, weights for customer *j*, denoted, by w_{hij} will be used to correct for the sample design and for site and survey nonresponse as discussed below.

Under this design, the simple differences-in-means impact estimate for comparing two research groups (g and g') to each other for a continuous or binary outcome, *y*, will be calculated as follows:

(1) $$I_1 = \bar{y}_g - \bar{y}_{g'}$$

where:

$$\bar{y}_g = \frac{\sum_{h=1}^{H}\sum_{i=1}^{n_h}\sum_{j=1}^{m_{hig}} T_{hij} w_{hij} y_{hij}}{\sum_{h=1}^{H}\sum_{i=1}^{n_h}\sum_{j=1}^{m_{hig}} T_{hij} w_{hij}}, \quad \text{and}$$

$$\bar{y}_{g'} = \frac{\sum_{h=1}^{H}\sum_{i=1}^{n_h}\sum_{j=1}^{m_{hig'}} (1 - T_{hij}) w_{hij} y_{hij}}{\sum_{h=1}^{H}\sum_{i=1}^{n_h}\sum_{j=1}^{m_{hig}} (1 - T_{hij}) w_{hij}},$$

where T_{hij} is a binary variable equal to 1 for customers in group g and 0 for customers in group g'.

The study will use the Taylor linearization method to calculate the variance of I_1. To highlight the features of this method, suppose that we are interested in estimating the variance of a population parameter $\beta=F(x_1,x_2,\ldots,x_n)$, where $F(.)$ is a nonlinear function of the observed data vector *x*. Suppose next that we perform a Taylor expansion of β around $(\mu_1,\mu_2,\ldots,\mu_n)$ where $\mu_p = E(x_p)$, where the $E(.)$ operator is the expected value of x_p averaging over repeated sampling from the sample universe. This Taylor expansion yields the following expression for the variance of β:

(2) $$\text{var}(\beta) \approx \text{var}\left(\sum_i Z_i\right), \text{ where}$$

$$Z_i = \frac{\partial F}{\partial x_i}(\mu_1, \mu_2 \ldots, \mu_n) x_i.$$

Consequently, to estimate the variance of β, the linearized covariates, Z_i, can be used in formulas for calculating variances for population *totals* under clustered designs.

To apply this method for the impact estimator in equation (1), we note that the mean outcomes for the two research groups in equation (1) are *ratios* of two sums (denoted by R_g and $R_{g'}$, respectively). Thus, using equation (2), the corresponding linearized variables for these ratio estimators can be expressed as follows:

$$Z_{hijg} = \frac{w_{hij}(y_{hij} - \hat{R}_g)}{\sum_{h=1}^{H}\sum_{i=1}^{n_h}\sum_{j=1}^{m_{hig}} T_{hij} w_{hij}} \text{ for group } g, \text{ and}$$

$$Z_{hijg'} = \frac{w_{hij}(y_{hij} - \hat{R}_{g'})}{\sum_{h=1}^{H}\sum_{i=1}^{n_h}\sum_{j=1}^{m_{hig'}} (1 - T_{hij}) w_{hij}} \text{ for the group } g'. \quad (3)$$

As discussed next, the way in which the study will use these linearized *Z* variables in the variance calculations will differ for those in the certainty and noncertainty sites.

Certainty sites. As discussed in Section 1a of Part B, two sites were selected with certainty (because these sites had selection probabilities greater than one). The customer samples in each of these sites can be treated as a simple random sample from each site. This is because the certainty sites were not "sampled," and hence, each certainty site is effectively its own stratum. Consequently, the variance of the impact estimates in the certainty sites do not need to account for between-site variability but only within-site variability.

The study will estimate the variance of the impact estimates in the certainty sites as follows:

$$\text{var}(I_{1,\text{certainty}}) = \sum_{h}\sum_{i} 2 m_{hi} S_{hi}^2, \text{ where}$$

$$S_{hi}^2 = (1 - f_{hi})(S_{hig}^2 + S_{hig'}^2)/2$$

$$S_{hig}^2 = \sum_{j=1}^{m_{hig}} (Z_{hijg} - \bar{Z}_{hig})^2/(m_{hig} - 1)$$

$$S_{hig'}^2 = \sum_{j=1}^{m_{hig'}} (Z_{hijg'} - \bar{Z}_{hig'})^2/(m_{hig'} - 1)$$

$$\bar{Z}_{hig} = \sum_{j=1}^{m_{hig}} Z_{hijg}/m_{hig}$$

$$\bar{Z}_{hig'} = \sum_{j=1}^{m_{hig}} Z_{hijg'}/m_{hig}, \quad (4)$$

and where f_i is the sampling fraction in site *i*. It is important to note that, for simplicity, the formulas are not indexed by "*certainty,*" although this index is implied, because these

calculations will be performed using data on only those customers in the certainty sites. This convention is followed for the remainder of this section.

Noncertainty sites. The variance of the impact estimates in the noncertainty sites must account for clustering due to the sampling of sites. A key feature of these variance calculations is that the research groups were selected from the *same* sites, thereby creating a potential correlation between the mean outcomes of customers across the research groups.

The formulas that the study will use to calculate the variance of the impact estimates in the noncertainty sites will differ depending on whether it is assumed that the sampling of sites was performed with replacement (WR) or without replacement (WOR). Under the WR assumption, the variance formula is very simple:

$$\text{var}(I_{1,\text{Noncertainty WR}}) = \sum_{h} n_h S^2_{h,\text{impact}}, \text{ where}$$

$$S^2_{h,\text{impact}} = \sum_{i=1}^{n_h} (I_{hi} - \bar{I}_h)^2 / (n_h - 1)$$

$$I_{hi} = Z_{hig} - Z_{hig'}$$

$$Z_{hig} = \sum_{j=1}^{m_{hig}} Z_{hijg}$$

$$Z_{hig'} = \sum_{j=1}^{m_{hig'}} Z_{hijg'}$$

$$\bar{I}_h = \sum_{i=1}^{n_h} I_{hi} / n_h. \quad \text{(5)}$$

This variance expression represents the extent to which estimated ***impacts*** vary across sites (and thus, accounts for the covariance between the mean outcomes of the research groups within the same site).

One problem with the WR assumption is that it is likely to produce conservative variance estimates because it does not incorporate the finite sample correction at the site level. One way to adjust for this problem is to include the finite population correction in the variance expression in equation (5) as follows:

$$\text{var}(I_{1,\text{Noncertainty WOR 1}}) = \sum_{h} (1 - f_h) n_h S^2_{h,\text{impact}}, \quad \text{(6)}$$

where f_h represents the sampling rate in stratum h. This approach is the formula for a WOR design where PSUs (sites) are sampled with *equal* probabilities within each stratum (region), and where second-stage sampling rates are small (which will be the case for the evaluation).

Another approach is to assume WOR sampling with unequal first-stage state selection probabilities and to use the Yates-Grundy-Sen variance estimator:

$$\text{var}(I_{1,\text{Noncertainty WOR 2}}) = \sum_{h=1}^{H} \sum_{i=1}^{n_h} \sum_{i>i'}^{n_h} (\gamma_{hii'}(I_{hi} - I_{hi'})^2 + \sum_{h=1}^{H} \sum_{i=1}^{n_h} \pi_{hi}(1 - f_{hi}) m_{hi} S_{hi}^2$$

where

$$\gamma_{hii'} = (\pi_{hi}\pi_{hi'} / \pi_{hii'}) - 1, \quad (7)$$

π_{hi} are state selection probabilities, and $\pi_{hii'}$ are joint inclusion probabilities for each *pair* of sites in the stratum. This method is somewhat cumbersome, because of the large number of joint inclusion probabilities that need to be calculated. Thus, the study will explore using this approach, but will rely more on the methods shown in equations (5) and (6).

Combined variance estimates. The study will calculate overall variance estimates by combining the variance estimates from the certainty and noncertainty sites as follows:

$$\text{var}(I_1) = p_c^2 \text{ var}(I_{1,\text{certainty}}) + (1 - p_c)^2 \text{ var}(I_{1,\text{Noncertainty q= WR, WOR1, or WOR2}}), \quad (8)$$

where p_c is the population share in the certainty sites.

Test statistics. To assess the statistical significance of the impact estimates, the study will compute t-tests by dividing the estimated impacts in equation (1) by the square root of estimated variances from equation (8). The number of degrees of freedom for these tests will be approximated as the number of sites in the sample minus the number of strata minus 1.

b. Regression Estimators

To obtain regression-adjusted impact estimates, the study will estimate variants of the following regression (ANCOVA) model:

$$y = \alpha + \gamma T + Q\delta + \varepsilon, \quad (9)$$

where y is an outcome variable at a specific time point, T is an indicator variable equal to 1 for customers in group g and 0 for customers in group g', Q are baseline explanatory variables that are associated with key outcome measures, ε is a mean zero disturbance term, and α, γ, and β are parameters to be estimated. The estimate of γ represents the regression-adjusted impact estimate of WIA on the outcome variable, and the associated t-statistic can be used to gauge the statistical significance of the impact estimate.

The study will use generalized linear model methods to estimate regression-adjusted impacts and their variances to account for the sample design. These methods generalize the Taylor series linearization method discussed above for parameters that are defined as *implicit* functions of linear statistics or estimating equations. These methods can be used to estimate linear models for continuous outcome measures as well as nonlinear logistic models for binary outcomes (the two main types of outcomes for which impacts will be estimated in the evaluation).

The theoretical assumptions for generalized linear models are as follows:

$$E(y_{hij}) = \mu_{hik}, \quad (10)$$

$$(11) \quad Var(y_{hij}) = Var(\mu_{hik}),$$

and *g* is a link function such that:

$$(12) \quad g(\mu_{hij}) = x'_{hij}\beta \text{ and } \mu_{hij} = g^{-1}(x'_{hij}\beta).$$

Note that the *X* variables in equation (12) contain both the *T* and *Q* variables in equation (9), and that the *kx*1 parameter vector β contains both the γ and δ parameters.

The estimating equations for the exponential family of distributions (of which linear and logistic regressions are special cases) can be derived by setting to zero the derivatives of the log likelihood function with respect to β. These estimating equations can be expressed as follows:

$$(13) \quad \frac{\partial \log L}{\partial \beta} = S(\beta) = \sum_{h=1}^{H} \sum_{i=1}^{n_h} \sum_{j=1}^{m_{hi}} \frac{\partial \mu_{hij}}{\partial \beta} w_{hij} V(\mu_{hij})^{-1} (y_{hij} - \mu_{hij}) = 0,$$

where *S(β)* is the score function.

Estimates of β in equation (13) can be obtained using Newton-Raphson (Taylor Series) methods. The variance of these estimates can be calculated as follows:

$$(14) \quad \text{var}(\hat{\beta}) = (J_0)^{-1} Var[S(\hat{\beta})](J_0')^{-1},$$

where J_0 is a *k*-by-*k* matrix of derivatives of the score function with respect to β, and *Var[S(β)]* is the *design-based* variance of the score function.

An estimate of *Var[S(β)]* can be obtained using the Taylor linearization method discussed in the previous section. This is because the score function is a *sum* of linearized *kx*1 *Z* vectors, where the Z vector for each individual is of the form:

$$(15) \quad Z_{hij} = \frac{\partial \mu_{hij}}{\partial \beta} w_{hij} V(\mu_{hij})^{-1} (y_{hij} - \mu_{hij}).$$

Consequently, similar procedures to those described in the previous section for the differences-in-means estimators can be used to compute *Var[S(β)]* using the linearized Z vectors. For instance, under the WR assumption, the variance estimate in the noncertainty sites can be computed as follows:

$$(16) \quad Var[S(\hat{\beta})] = \sum_h \frac{n_h}{n_h - 1} \sum_i (Z_{hi} - \bar{Z}_h)(Z_{hi} - \bar{Z}_h)'$$

$$Z_{hi} = \sum_j Z_{hij}$$

$$\bar{Z}_h = \frac{1}{n_h} \sum Z_{hi},$$

and under the WOR assumption with equal state sampling probabilities, the variance estimate can be obtained by multiplying equation (16) by $(1-f_h)$.

Linear and logistic regression procedures are special cases of the above generalized linear model formulation. For linear regression, the β parameters can be estimated using the following weighted least squares formula:

(17) $$\hat{\beta} = (X'WX)^{-1}X'WY,$$

where W is a matrix of weights. Design-based variances for these regression coefficients can be estimated using the formulas in equations (13) to (15) where:

(18) $$\mu_{hij} = x'_{hij}\beta \text{ and } Var(\mu_{hij}) = \sigma^2.$$

For logistic regression models, the assumptions are:

(19) $$\mu_{hij} = \frac{\exp(x'_{hij}\beta)}{1+\exp(x'_{hij}\beta)} \text{ and } Var(\mu) = \mu(1-\mu).$$

The estimated impacts using the regression approach should be similar to the differences-in-means impact estimates, because the covariates should be uncorrelated with treatment status due to random assignment. However, the standard errors of the impact estimates should be smaller using the regression models because the covariates are likely to be correlated with the outcome measures, and hence, are likely to reduce intraclass correlations.

c. Estimating Impacts for Participants and Adjusting for Crossovers

The experimental framework will provide unbiased estimates of the impact of the *opportunity* to receive specific WIA services (intent-to-treat [ITT] effects). However, since some sample members may decide not to use the offered WIA services, the net impacts on just those who participate in the program (treatment-on-the-treated [TOT] effects) are also of interest.

Crossovers occur if customers assigned to one research group receive WIA services for which they are ineligible given their study assignment to the core or core-and-intensive group. Our main approach to crossovers is to prevent them. Site staff were carefully trained on the importance of not undermining the experiment. We monitored the extent of crossovers by collecting administrative data on service receipt from the sites. In all groups, crossovers were less than 5 percent.

Methods to adjust for nonparticipation and research group crossovers are complex because research groups were offered different combinations of services. Thus, both the full-WIA and the core-and-intensive services research groups under investigation had nonparticipants and crossovers. This problem becomes more tractable under certain assumptions, in which case policy-relevant TOT estimates can be generated, although they must be interpreted carefully. TOT impacts will be estimated using two potential approaches.

First, assuming the treatment has no impact on those who did not receive the service, the Bloom adjustment will be used to calculate the impact of the treatment on those who did receive the service. The TOT impact is calculated by dividing the estimated ITT impact from the full sample by the proportion of the relevant group that received services (Angrist et al. 1996; Bloom 1984). In our case, a participant will be defined as a customer who receives any intensive or training services. Bloom adjustment procedures will be applied to the various contrasts:

- **Impacts of the receipt of intensive services.** These impacts can be obtained by dividing the difference between the mean outcomes of those in the core-and-intensive services and core groups by the percentage of core-and-intensive services group members who received intensive services.
- **Impacts of the receipt of training beyond core and intensive services.** These impacts can be obtained by dividing the difference between the mean outcomes of the full-WIA and core-and-intensive services groups by the difference between the participation rates for the two groups. These TOT estimates must be interpreted carefully because they will reflect both the receipt of training services as well as differences in the amount of intensive services received by the two groups.

The second approach for obtaining TOT estimates uses counselors' predictions on how likely each customer would be to receive intensive services and training, if offered. The study registration form (SRF) requests that the counselor, using check boxes, indicate the likelihood that each customer eligible for random assignment will receive training services. This information will be obtained prior to random assignment, and thus, will be available for all members of the FW, CI, and C research groups. The accuracy of these predictions will be assessed by comparing predicted and actual training receipt designations for members of the FW group.

If these predictions are highly accurate, we will estimate TOT impacts on the actual receipt of intensive services and training services by comparing the mean outcomes of predicted trainees in the FW and C groups. To assess TOT impacts of the actual receipt of training, beyond intensive services, we will compare the mean outcomes of predicted trainees in the FW and CI groups and divide this impact by the proportion of the CI group that receives intensive services (to account for some customers in the CI group who do not receive intensive services).

We will also use additional baseline data from the study registration forms along with propensity scoring methods to obtain more precise training predictions and impacts (Schochet and Burghardt 2007). This will be done in three stages, which we discuss using the FW and C groups. In the first stage, we will use the FW group only to estimate a logit model that regresses an indicator variable that equals 1 for those who actually received training and 0 for those who did not on indicators of the counselor training predictions and other baseline covariates. In the second stage, we will compute predicted probabilities (propensity scores) for both FW and C members using the parameter estimates from the model. Because of random assignment, the parameter estimates pertain not only to the FW group but also to the C group.

There are two options for the third stage. One option—the traditional method—is to use the estimated propensity scores to match a C member to each FW member (with replacement) using nearest neighbor, caliper, or kernel matching. Trainee impacts would then be obtained by

comparing the outcomes of actual trainees in the FW group to their matched C members. The second option—the cutoff method—obtains a "predicted" trainee group by selecting FW and C members with propensity scores larger than a cutoff value. Trainee impacts would then be estimated by comparing FW and C members in the predicted trainee group. Under this approach, it is natural to select the cutoff value so that the proportion of all FW members in the predicted trainee group is the same as the proportion of all FW members who actually received training (see Schochet and Burghardt, 2007 for more details).

d. Estimating Impacts for Subgroups

Subgroup analyses will address the question of whether access to a certain tier of WIA services is more effective for some subgroups than others. Analyses will be conducted for subgroups defined by customer characteristics and for subgroups defined by program and community characteristics. The first set of subgroup analyses will determine the extent to which specific services benefit customers with different baseline characteristics, such as age, sex, race/ethnicity, education level, and employment history. The second set of subgroup analyses will determine the extent to which key LWIA characteristics, such as performance on DOL's common measures, quality of implementation, site size, and local area characteristics, are related to observed impacts.

Impacts for each subgroup will be estimated in turn using a straightforward modification to equation (9), where, for simplicity of exposition, an analysis contrasting two research groups is assumed and the subgroup indicator Q_s is defined at the individual level and has two levels (for example, $Q_s=1$ for females and $Q_s=0$ for males):

(20) $$y = \alpha + \gamma T + Q_{-s}\delta_{-s} + Q_s\delta_s + (Q_s{*}T)\theta + \varepsilon.$$

Equation (20) differs from equation (9) because of the inclusion of the interaction term, $Q_s{*}T$, and where Q_{-s} represents the vector of baseline covariates that excludes Q_s. The regression-adjusted impact for those with $Q_s=1$ (for example, females) is $(\gamma+\theta)$, and for those with $Q_s=0$ (for example, males), it is γ. The parameter θ represents the *difference* in the impacts across the two subgroup levels. Equation (20) can be generalized to subgroups with more than two levels (such as race/ethnicity) by including additional treatment-by-subgroup indicator variables and using *F*-tests to assess whether differences in impacts across subgroup levels are statistically significant.

e. Construction of Weights and Nonresponse Adjustments

All impact analyses will be conducted using sample weights that adjust for the sample design and for site and customer nonresponse, so that the design-based impact estimates can be generalized to the customer universe for the evaluation. The primary analysis sample will include the 26 originally-selected sites that agreed to participate in the study. A secondary analysis sample for the sensitivity analysis will also include the two replacement Midwest sites. For this secondary analysis using the 28-site sample, we will construct weights assuming that the two replacement sites were "original" sites.

For both the primary 26-site sample and the secondary 28-site sample, the survey weights will be obtained by first calculating the following selection probability for each survey respondent:

$$(21) \quad p_{hijg} = q_{hi} a_{hi} c_{hijg} r_{hijg},$$

where p_{hijg} is the probability that customer *j* in region *h*, site *i*, and research group *g* completes a follow-up interview; q_{hi} is the probability that site *i* in region *h* is selected for the study; a_{hi} is the probability that a selected site agrees to participate in the evaluation; c_{hijg} is the probability that a customer within a participating site is selected for follow-up interviews; and r_{hijg} is the probability that the customer is a survey respondent. The weight for a customer, w_{hij}, will then be computed to be inversely proportional to p_{hijg}.

Calculating q_{hi} and c_{hijg}. The probability that a site is selected for the study (q_{hi}) will be computed using the sampling probabilities discussed above that are based on recent WIASRD data on the number of LWIA customers who received intensive services. Similarly, values for c_{hijg} will be obtained using the customer sampling probabilities to the various research groups from above.

Calculating a_{hi}. As discussed, 30 sites were randomly selected for the study, 26 agreed to participate, and two Midwest sites were selected as replacements for two refuser Midwest sites. Sites which refused to participate may differ from more cooperative sites in ways that are potentially related to customer outcomes and impacts. If not corrected, the effects of site nonresponse could lead to biased impact estimates.

To examine the effects of site nonresponse, the contractor will first conduct statistical tests (chi-squared and t-tests) to gauge whether the characteristics of responding sites are fully representative of the 30 sites. These analyses will be conducted using the following data: strata indicators used for site selection (region, size, and training rate), WIA funding levels, additional customer characteristics in the WIASRD data, and local area data (such as the unemployment rate) in the ARF data.

Our primary approach for adjusting for site nonresponse will be to calculate a_{hi} using the following propensity score matching procedure:

- **Estimate a logit model predicting site nonresponse**. A binary variable—equal to 1 for a participating site and zero for a nonparticipating site—will be regressed on the variables listed above.
- **Calculate a propensity score for each site.** This score is the predicted probability that a site is a respondent, and will be constructed using the parameter estimates from the logit regression model and the site's covariate values. Sites with large propensity scores are more

likely to be respondents, whereas sites with small propensity scores are more likely to be nonrespondents.

- **Construct response probabilities (the a_{hi} probabilities) using the estimated propensity scores.** The response probability for a site will be calculated as the site's estimated propensity score. It is important to note that the propensity score procedure adjusts only for *observable* differences between site respondents and nonrespondents. The procedure does not adjust for potential unobservable differences between the two groups. Thus, this procedure only partially adjusts for potential nonresponse bias.

Calculating r_{hijg}. Survey nonresponse can also bias impact estimates if outcomes of survey respondents and nonrespondents differ. To assess whether survey nonreponse may be a problem for each follow-up survey, three general methods will be used:

- **Comparing the baseline characteristics of survey respondents and nonrespondents within research groups.** We will conduct statistical tests to gauge whether those in a particular research group who respond to the interviews are fully representative of all those in that research group. The statistical tests will use baseline data from the SRF (which will be available for the full research sample). For each baseline characteristic, we will test whether there are significant differences between customers who responded to the follow-up survey and those who did not respond to the follow-up survey, using *t*-tests to test for significant differences in univariate characteristics (such as age) and chi-square tests to test for significant differences in categorical variables (such as educational attainment). These tests will be conducted separately for each research group. Noticeable differences between respondents and nonrespondents could indicate potential nonresponse bias and limit the generalizability of the study results if not taken into account.
- **Comparing the baseline characteristics of respondents across research groups.** Tests for whether the baseline characteristics of respondents across research groups differ from each other will be conducted. Similar to the comparisons between respondents and nonrespondents, for each baseline characteristic on the SRF, we will test whether there are significant differences in baseline characteristics for respondents in each of the three research groups, again using *t*-tests for univariate characteristics and chi-square tests for categorical variables. Statistically significant differences between respondents in different research groups could indicate potential nonresponse bias and limit the internal validity of the study if not taken into account.
- **Comparing impacts for respondents and nonrespondents using administrative data.** Administrative outcome data will be available for both survey respondents and nonrespondents. To gauge the extent to which survey nonresponse may be a problem, statistical tests will be conducted to assess whether estimated impacts based on administrative outcome data differ for survey respondents and those in the survey sample who did not respond to the survey. This will be done in the same framework as the subgroup analysis described in Equation (3) and the accompanying text, where the subgroup is follow-up survey response status. The parameter estimate for λ represents the estimated difference in the impacts for survey respondents and nonrespondents.

Two approaches for correcting for potential survey nonresponse bias will be used in the estimation of program impacts based on survey data. First, adjustments for any observed differences between respondents across the various research groups will be performed by including baseline characteristics of the respondents in all the regression models. Second, because this regression procedure will not correct for differences between respondents and nonrespondents, we will construct values for r_{hijg} so that the weighted observable baseline characteristics are similar for respondents and the full sample that includes both respondents and nonrespondents. For each survey instrument and research group, the study will construct r_{hijg} using the propensity score methods discussed above, where (1) a logit model will be estimated that predicts interview response using baseline data, and (2) r_{hijg} will be calculated as the predicted propensity score.

This propensity score procedure will yield large weights for those survey respondents with characteristics associated with low response rates (that is, for those with small propensity scores). Similarly, the procedure will yield small weights for those respondents with characteristics that are associated with high response rates. Thus, the weighted characteristics of respondents should be similar, on average, to the characteristics of the entire research sample.

Poststratification. The study will not poststratify the sample for several reasons. First, the study initially selected the sample using stratified random sampling methods, and thus, will obtain proportionate representation within key subgroups of the WIA customer population. Second, because of large sample sizes, stratified random selection will tend to generate proportionate sample sizes even across customer subgroups that are not used to define the initial strata. Finally, the study will not obtain additional key data items on individual sample members and the full sample universe after sampling that will be useful for adjusting the means of the treatment and comparison groups using poststratification methods. Thus, the sample weights for the study will not be adjusted for poststratification.

Multiple Imputations. To test the sensitivity of our results to this propensity score procedure, we will also use multiple imputation procedures (Rubin 1976) that replace missing customer outcomes with a set of plausible values that represent the uncertainty about the correct imputed value. We will generate five multiply imputed data sets, analyze them using standard procedures for complete data, and combine the results from these analyses. This multiple imputation technique has become quite commonly used in experimental evaluations of social policy interventions (Puma et al. 2009; Rubin 1987).

Specifically, we will use the regression method where a regression model is fitted for each variable with missing values, with the previous variables as covariates. The models will include both site-level and customer-level baseline variables. Based on the fitted regression coefficients, a new regression model will be simulated from the posterior predictive distribution of the parameters and will be used to impute the missing values for each variable. This process will be repeated sequentially.

We will estimate impacts using each of the five data sets and using the sampling weights. Let β_i be the estimated impact for data set *i*. The final estimate for the treatment effect will be the mean of the β_i (that is, $\bar{\beta} = \sum_{i=1}^{5} \beta_i / 5$).

The standard error of the combined estimate will be calculated from (1) a within-imputation variance component, (2) a between-imputation variance component, and (3) an adjustment factor for the number of repetitions (D=5 in our case). Let W_i be the estimated variance of the parameter from repetition i. Then the within-imputation variance is $\bar{W} = \sum_{i=1}^{5} W_i / 5$, the between-imputation variance component is $B = \sum_{i=1}^{5} (\beta_i - \bar{\beta})^2 / 4$, and the total variance is $T = \bar{W} + (6/5)B$, which will be used for significance testing.

Degree of accuracy for the impact estimation. A sample size that is adequate to detect any net impacts that are large enough to be policy relevant is key to the success of the evaluation. This section presents minimum detectable impacts (MDIs) on quarterly earnings—one of the key outcomes of the evaluation—for both the survey and administrative record samples for the sample of 26 sites (Table B.6). In calculating the MDIs, a five percent significance level and two-tailed test are assumed. The power calculations incorporate design effects stemming from the clustering of individuals within sites and the use of sampling weights, as well as multiple comparison adjustments.

Variances under a clustered design. To consider sources of variance under a clustered design, a hypothetical unclustered simple random assignment design in which customers would be randomly assigned to each research condition across all LWIAs is considered first. Under this design, the variance of the estimated impact on an outcome measure (that is, the difference between the mean outcomes of those assigned to two research groups being compared) must account for between-customer variance only and can be expressed as follows:

$$(4)\quad Var(impact) = \sigma^2 [\frac{1}{k_1} + \frac{1}{k_2}]$$

where k_1 is the number of customers in the first research group, k_2 is the number of customers in the second research group, and σ^2 is the variance of the outcome measure.

Under the two-stage design proposed for the evaluation, study sites were first randomly selected from the universe of LWIAs, and then study-eligible customers within the study sites were randomly assigned to the research groups. Under this design, there is clustering at the site level. Intuitively, if sampling were repeated, a different set of sites would be selected, which introduces additional variance to the impact estimates relative to the simple random sample design discussed above. Mathematically, the variance expression becomes

$$(5)\quad Var(impact) = (1 - f)\frac{2\sigma^2\rho(1 - c)}{s} + \sigma^2(1 - \rho)[\frac{1}{k_1} + \frac{1}{k_2}]$$

where s is the number of study sites ($s = 30$), ρ is the between-site variance as a proportion of the total variance of the outcome measure—the intraclass correlation—and f is the finite population correction at the site level. If there is no between-site variance (that is, if mean customer outcomes are the same in every LWIA), then $\rho = 0$ and equation (5) reduces to equation (4). Even if ρ is small, design effects from clustering can be large because the site-level term in the variance expression is deflated by the number of sites, not the much larger number of customers. However, if the sites in the selected sample represent a large proportion of the total WIA customer population, then the finite population correction reduces the site-level term in proportion to the share of the population represented by the sample. For example, if half of the customers are represented by the sampled sites—that is, $f = 0.50$—then the site-level variance term is half of what it would have been otherwise.[2] If all of the sites were selected—that is, $f = 1$—then the site-level term would disappear. The within-site correlation between the outcomes of those assigned to the two research groups is captured by the parameter c and is likely to be positive. Thus, this correlation will likely reduce the variance and, hence, the design effects, due to clustering.

An equivalent way of expressing equation (5) is as follows:

$$(6)\quad Var(impact) = \frac{\sigma_I^2}{s} + \sigma^2(1-\rho)\left[\frac{1}{k_1}+\frac{1}{k_2}\right]$$

where σ_I^2 is the variance of the *net impacts* across sites. Thus, design effects will be small if impacts are similar across LWIAs, which would occur if c is close to 1 or ρ is close to 0 in equation (5). Based on data from recent employment-related impact evaluations on populations similar to the WIA population, the value of c is set to 0.7 and ρ is equal to 0.04 in the MDI calculations. Estimates of rho and c come from three sources: (1) DOL's National Evaluation of the Trade Adjustment Assistance Program that included a national sample of workers filing for UI benefits across 26 randomly selected states and hundreds of local workforce areas, (2) DOL's Evaluation of the Individual Training Account Demonstration; and (3) DOL's National Job Corps Evaluation which contained national samples across 100 Job Corps centers nationwide. In the simulations used to test the sampling procedure, as discussed in Subsection 1a above, design effects from clustering and weighting were calculated in each of the simulated random draws. On average, design effects that incorporate both clustering and weighting effects are expected to be about 1.51 for impacts based on the follow-up interview sample—that is, the variance is about 51 percent larger compared to an unclustered, self-weighting design—and this estimated design effect did not vary much across the simulations. For the administrative records sample, the site-level term is a larger proportion of the total variance, and as such, the design effect for the administrative records sample is larger, 2.25, mostly due to a greater relative effect of clustering on the variance.

Multiple comparisons problems and solutions. The evaluation randomly assigned adult and dislocated workers to three research groups. Thus, there are three possible contrasts for analysis:

[2] The sampling strategy is designed to generalize to the full population of WIA sites at the time of the study (excluding small sites and sites not on the U.S. mainland), so the finite population correction is appropriate for the site-level term in the variance formula.

1. Comparisons of the full-WIA group to the core-and-intensive group
2. Comparisons of the full-WIA group to the core group
3. Comparisons of the core-and-intensive group to the core group

Suppose separate *t*-tests were conducted for each contrast to test the null hypothesis of no impacts, where the type I error rate (statistical significance level) is set at α = five percent for each test. This means that the chance of erroneously finding a statistically significant impact is five percent. However, when the hypothesis tests are considered together, the "combined" type I error rate could be considerably larger than five percent. For example, if all null hypotheses are true, the chance of finding at least one spurious impact across the three tests would be 14 percent (assuming that the tests are independent). Thus, without accounting for the multiple comparisons being conducted, there is a greater chance that the study will erroneously conclude that some particular treatment is preferred over others. A similar issue arises when considering estimating program impacts on many outcome measures or for many different subgroups of customers—the probability of finding spurious impacts increases greatly.

At the same time, statistical procedures that correct for multiple testing typically result in hypothesis tests with reduced statistical power—the probability of rejecting the null hypothesis given that it is false. Stated differently, these adjustment methods reduce the likelihood of identifying real differences between the contrasted groups because controlling for multiple testing involves lowering the type I error rate for individual tests, with a resulting decrease in the power to detect statistically significant impacts when the program is indeed effective (Schochet 2008b).

The MDI calculations for the full sample adjust for multiple comparison testing. One MDI adjustment approach, based on the Bonferroni method, is to calculate MDIs in which the usual significance level (α = five percent) is divided by the number of tests (three in the case of the main contrasts). This approach is conservative because it assumes independent tests, even though the tests are correlated because of the repetition of each research group sample across tests. Instead, the less conservative Tukey-Kramer method that accounts for the repetition of research groups in each comparison will be used (Kramer 1956; Tukey 1953).

The multiple comparisons problem also occurs when tests of intervention effects are conducted across multiple outcomes. To address this issue, outcomes for which the analysis is *confirmatory* versus outcomes for which the analysis is *exploratory* will be distinguished. The confirmatory analysis will focus on priority outcomes—average quarterly earnings and employment—and provide estimates whose statistical properties can be stated precisely. The goal of this analysis will be to present rigorous tests of the study's central hypotheses; for these analyses, significance levels will be adjusted for multiple testing. Confirmatory analyses will be limited to estimates based on the full sample of customers.

The purpose of exploratory analysis, on the other hand, will be to examine other outcomes of interest, such as participation in training and receipt of public assistance, for which impacts might exist. The aim of this analysis will be to identify hypotheses that could be subject to more rigorous future examination. For the exploratory analysis, multiple comparison adjustments will not be made.

Finally, the multiple comparisons problem also arises when considering many subgroups for which separate impacts are estimated. Therefore, all subgroup analyses will be treated as exploratory. We will conduct F-tests of the differences in impacts within categories of subgroups. For example, we will conduct an F-test of whether the impact on older customers is different than the impact on younger customers. We will note in our report that with an alpha threshold high enough to account for the multiple comparisons among all the subgroups (not just those in a category), it is likely that no impact on a subgroup would be found significant.

Minimum detectable impacts. For the overall participant sample, we can expect to detect a significant quarterly earnings impact for each comparison if the true program impact were $161 or more using the survey sample and $127 or more using the administrative records sample (Table B.6). The MDIs are lower for the administrative records sample as we will collect administrative data on everyone in the full-WIA group and not just the 2,000 selected for the survey sample.

Table B.6. Minimum detectable impacts on quarterly earnings, for adults and dislocated workers in 26 sites that agreed to participate

	Full-WIA vs. Core	Full-WIA vs. core-and-intensive	Core-and-intensive vs. core
	Quarterly earnings (dollars)	Quarterly earnings (dollars)	Quarterly earnings (dollars)
Survey Data			
Adult and dislocated workers	161	161	161
WIA training participants	316	316	NA
Adults only	169	169	169
Dislocated workers only	198	198	198
50% subgroup of customers	181	181	181
50% subgroup of sites	200	200	200
Administrative Data			
Adult and dislocated workers	127	127	151
WIA training participants	249	249	NA
Adults only	127	127	127
Dislocated workers only	144	144	157
50% subgroup of customers	134	134	168
50% subgroup of sites	159	159	188

Notes: The MDI formula used for the calculations is as follows:

$$factor \times \sigma \sqrt{(1- R^2_{across})(1- f)(2\rho(1- c)/ s)+(1- \rho)\left(\frac{1- R^2_{within}}{r}\right)\left(\frac{1}{k_1}+\frac{1}{k_2}\right)}$$

where σ is the standard deviation of quarterly earnings ($1,250) based on results from previous similar studies, f is the finite population correction (0.247), r is the response rate (0.82 for the survey, 1.00 for administrative records), R^2 is 0.20 both within and across sites, the intraclass correlation ρ is 0.04, the correlation of treatment and control groups within sites c is 0.70, k1 and k2 are pertinent sample sizes for groups 1 and 2, and s is the total number of sites (26). The MDI calculations assume two-tailed tests, 80 percent power, and a five percent significance level that is adjusted for multiple testing using the Tukey-Kramer approach, yielding a factor of 3.19. For subgroup estimates, no multiple testing adjustment is made, yielding a factor of 2.80. To calculate the MDI on those who participate in training, the MDI for the full sample is divided by the estimated training rate of 51 percent.

NA = not applicable.

MDIs can also be calculated for customers who participate in training, which is an important, and often expensive, component of WIA services. About 51 percent of WIA customers who receive intensive services also participate in training. Using the Bloom adjustment, it is estimated that the MDI for full-WIA group members who participate in training —the estimate of TOT—is $316 for the survey sample and $249 using administrative records data when compared to the core-and-intensive services group. (Since only the full-WIA group is eligible for WIA-funded training, the estimated MDIs for training participants for the core-and-intensive versus core comparison are not calculated.)

MDIs as measured by the survey data are about $181 for a subgroup including 50 percent of customers. The design will also be slightly less effective at detecting impacts for subgroups of sites than for subgroups defined by customer characteristics, because of larger clustering effects, but it can still reliably detect impacts on quarterly earnings that are $200 or larger for the survey sample and $159 for the administrative sample.

The MDIs are comparable to the inflation-adjusted quarterly earnings impacts found for adults in the National JTPA Study (Bloom et al. 1993). The MDIs also suggest that the study will have sufficient precision to assess whether the impact of the WIA services are sufficient to justify the costs. The ITA Experiment found that the cost of WIA-funded training on average was about $3,200 per customer (McConnell et al. 2006). Hence, for the benefits from increased earnings to outweigh the costs of training, earnings would need to increase by more than $320 per quarter on average over the 30-month period. The MDIs are sufficiently small that we will be able to detect an impact as small as $320 per quarter for the full sample with either the survey or administrative data.

3. Methods to Maximize Response Rates and Data Reliability

The contractor will use well-established methods to maximize response rates and data reliability for the follow-up surveys. We have used the methods described below during the administration of the 15-month survey and the ongoing 30-month survey, as well as in other data collection efforts, such as the Trade Adjustment Assistance (TAA) Study (OMB number 1205-0460) and the Individual Training Account (ITA2) Follow-up Study (OMB number 1205-0441). For this evaluation, we achieved a 79 percent response rate to the 15-month follow-up survey.

The strategy for maximizing response to the follow-up surveys began with the survey development and carries through the entire survey process. The methods employed mitigate all types of individual nonresponse, from failure to locate the sample member to a refusal to participate in the survey. Using the methods for the two follow-up surveys, we obtained a 79 percent response rate for the first follow-up survey and expect to obtain a response rate of at least 82 percent for the second follow-up survey.

Survey development. Elements of the survey development and administration itself will support high response:

- **Survey language and length.** The two follow-up questionnaires were designed to be easy to complete. The questions are written in clear and straightforward language. The average time required for the respondent to complete the survey was 40 minutes for the 15-month follow-up and is 30 minutes for the 30-month follow-up.

- **Multilanguage survey administration.** During telephone contact, interviewers identify Spanish-speaking respondents and connect or schedule them to speak with a bilingual interviewer. Also, if the study intake documents (consent, study registration, or contact information forms) were completed in Spanish, a bilingual interviewer is automatically assigned the case. When necessary, translators for languages other than Spanish are used. Mathematica employs staff who speak a wide range of languages and have experience conducting interviews in a number of languages.

Methods to enhance locating efforts, promote positive contacts with sample members, and sustain outreach efforts over time and with the toughest sample members also support high survey response:

Locating sample members. An essential step in a successful survey effort is the ability to locate as much of the survey sample as possible. The locating process begins with the use of an independent vendor that will check the full sample against current address databases prior to any initial outreach. This first step is critical given that some sample members may have moved since the time of their entry into the study. For any mail that is returned as undeliverable after the initial advance letter (described below), the evaluation team will begin a series of extensive tracking and locating procedures that have proven successful in other Mathematica studies. These procedures include using other independent address databases and searching social networking sites. When these attempts fail to locate the sample member during the survey period, the contractor turns to checking with neighbors and family members. At the time of study intake, customers completed a contact information form to provide contact information for up to three friends or relatives who might know how to get in contact with them at some future date (Appendix B, OMB clearance number 1205-0482). When talking with these contacts, the specific purpose of the call is not disclosed, but Mathematica locators convey that the effort to reach the sample member is for an important study being sponsored by the government.

If all centralized efforts to locate and interview a sample member have been exhausted, with no completed interview, the evaluation team prepares locating packets to send to local interviewers. These local interviewers are trained on the project's goals and the questionnaire, but their main purpose is to find the sample member and have that person call into the telephone interviewing center. The field locator may also discover other information about the sample member, including that they have moved, have been incarcerated, entered the military, or are deceased. In some cases, the field interviewer may receive an adamant refusal from the customer to participate in the survey. All information gleaned by the field staff is sent back to the contractor for further determination on how to proceed.

Initial contact with sample members. Establishing the authenticity of the survey effort with sample members from the start lays an important foundation in promoting a high response. To provide sample members with an initial, official introduction to the survey, including its purpose, content, and length, the evaluation team sends an advance letter on DOL letterhead (including the evaluation project logo) shortly before fielding of the survey begins (Appendix D). This letter (1) explains the voluntary nature of participation and their privacy protection, (2) extends the incentive offer, and (3) gives a toll-free number for telephone calls. The envelope is printed with the DOL logo to capture the sample member's attention and to communicate the legitimacy of the study. (However, Mathematica's return address will be used to facilitate the processing of returned mail and locating procedures.)

Gaining and maintaining cooperation. The evaluation team makes multiple attempts to reach all sample members through a series of outreach methods by mail and telephone. Also, the team sends out a reminder postcard to sample members who remain difficult to reach one week after the initial advance letter (discussed above) is sent. The postcard provides a toll-free number to use to call in and complete the survey at their convenience and prominently displays the incentive amount for survey completion. Based on the experience in past survey efforts, we have found that the incentive amount captures the attention of anyone who receives the postcard—if it is not the sample member directly, then the mail recipient is more likely to pass along the postcard to its intended recipient. Another postcard providing similar information is sent out to sample members after another two weeks of nonresponse. Two more reminder letters are sent out to nonrespondents at the midpoint of the data collection and again three to four weeks prior to the end of data collection. The advance letter and reminder postcards are provided in Appendix D.

Getting a respondent on the phone is clearly an important step, but gaining their cooperation to begin and ultimately complete the survey is paramount. Mathematica's interviewers are highly trained in establishing rapport with respondents, gaining their cooperation, and avoiding refusals. During project-specific training, interviewers focus on skill development and role-playing to secure respondents' cooperation and avert and convert refusals. Sample members who still refuse to participate once reached are sent a tailored refusal-conversion letter that addresses their specific concerns. Following the letter, an expert refusal-conversion interviewer makes follow-up calls to try to gain the sample members' cooperation.

Incentives for survey participants. Offering an incentive for completion of the follow-up surveys is important for obtaining the desired response rates and reducing overall survey costs without affecting data quality. There is substantial evidence on the benefits of offering incentives. According to Singer et al. (2000), incentives can help to achieve high response rates by increasing the propensity of sample members to respond. By doing so, incentive payments have been found to contain evaluation costs by significantly reducing the number of calls required to resolve a case. Studies offering incentives show decreased refusal rates and increased contact and cooperation rates. Incentives also increase the likelihood of participation from subgroups with a lower propensity to cooperate with the survey request. This is an important component of ensuring the representativeness of the survey respondents and the quality of the data being collected. For example, Jäckle and Lynn (2007) find that incentives increase the participation of sample members more likely to be unemployed. There is also evidence that incentives bolster participation among those with lower interest in the survey topic (Schwartz et al. 2006; Jäckle and Lynn 2007; Kay 2001), resulting in data that are more nearly complete. Furthermore, paying incentives does not distort responses and impair the quality of the data obtained (as reflected in item nonresponse or the distribution of responses) from groups that would otherwise be underrepresented in the survey (Singer et al. 2000).

For the 30-month follow-up survey, we are using a tiered approach to incentives. The approach is to: (1) offer sample members who were paid a $40 or $75 payment for completing the 15-month survey a $75 payment for completing the 30-month follow-up survey; (2) offer sample members who either did not respond to the 15-month survey or were paid $25 for completing the 15-month survey a $25 incentive to complete the 30-month survey and increase this to $75 only for sample members who are unresponsive to outreach attempts for three months. This incentive approach was approved by OMB on March 12, 2015 (ICR reference

number 201502-1205-001). Part A of this clearance package provides additional justification for the incentives used for the 30-month follow-up survey.

Data reliability. The two follow-up surveys are unique to the current evaluation and will draw from a sample of participants from across all the evaluation sites, ensuring consistency in the collected data. The surveys have been extensively reviewed by project staff and staff at DOL, and have been thoroughly tested in a pretest involving six individuals from nonparticipating sites.

Evaluation sample members are interviewed by trained members of Mathematica's survey operations staff who are experienced working on previous studies conducted for DOL as interviewers, supervisors, and monitors. Most of these staff members are familiar with similar questionnaire content and sensitive to the difficulties faced by job seekers and unemployed individuals. All survey operations staff assigned to the study participate in both general training (if not already trained) and extensive project-specific training. Interviewers do not work on the survey until they have been certified as prepared. The project-specific training included role-playing with scenarios and other techniques to ensure that interviewers are ready to respond effectively to questions from sample members about the study and the survey in order to illicit complete and accurate responses from respondents. A list of frequently asked questions and answers (FAQs) (Appendix C) was developed and included in the operational procedures manual for the survey administered via computer-assisted telephone interviewing (CATI). Interviewers can also access the FAQs at any time during the survey.

When the survey administration is completed, an analysis that compares response rates in the full WIA, core-and-intensive and core groups will be conducted to assess whether there are systematic differences between the groups in the likelihood of nonresponse and in the characteristics of individuals responding to the survey. This analysis will use data from the SRF, which will be available for all sample members. These data will include the same variables used to monitor the random assignment process. If it appears that the survey respondent sample is not representative of the study sample, weights to adjust for nonresponse will be developed using propensity scoring methods. (The details of these methods are discussed above in Part B, Section 2.)

4. Tests of Procedures or Methods

The follow-up survey instruments were thoroughly tested with six individuals from nonparticipating sites. Mathematica employed an iterative pretesting approach; that is, survey staff administered three pretests and incorporated lessons learned before proceeding with the remaining pretests. For the initial pretests, Mathematica incorporated cognitive interviewing techniques in which respondents were encouraged to think through their responses out loud. Survey researchers encouraged respondents to identify any words and phrases that were confusing as the questions were asked rather than waiting for an end of interview debriefing. These techniques were applied to the survey introduction, answers provided to frequently asked questions, as well as to questionnaire items. The survey researchers used non-leading probes to minimize bias (for example, "*I noticed you hesitated. Tell me what you were thinking*") when administering the interviews.

After the first three pilot tests were completed as cognitive interviews, the final three pretest interviews provided timing estimates. Project staff debriefed those respondents using a standard debriefing protocol to determine if any words or questions were difficult to understand and answer. Respondents in the pilot test of the follow-up surveys were given an incentive for their time completing the survey. Following the pretest and OMB approval (clearance number 1205-0504), the 15-month survey was administered successfully to the entire survey sample and yielded a 79 percent response rate. The 30-month survey administration began in June 2014 and is currently ongoing.

5. Individuals Consulted on Statistical Methods

For the evaluation itself, consultations within the evaluation team on the statistical methods have been used to ensure the technical soundness of the study.

The following individuals were consulted on the statistical methods discussed in this submission to OMB:

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