# Estimated Impacts for Participants in the Trade Adjustment Assistance (TAA) Program Under the 2002 Amendments

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# EXECUTIVE SUMMARY

The Trade Adjustment Assistance (TAA) program is the linchpin of Federal efforts to assist America's manufacturing workers rebound from job separations that they experienced as a consequence of foreign competition. The program's goal is to help trade-affected workers obtain reemployment at a suitable wage by providing training, temporary income support, and other services. Historically, TAA has also played a compensatory role, by assisting workers harmed by the relaxation of trade barriers.

In the wake of amendments to the TAA program, enacted as the Trade Act of 2002,[1] the U.S. Department of Labor's Employment and Training Administration (ETA) funded a comprehensive *Evaluation of the TAA Program*, designed to document the program's implementation and assess the ability of the program to achieve its goal of helping participants find rapid and suitable reemployment. This report, one of a series produced as part of the evaluation, focuses on the latter objective, by describing results from an impact analysis, using a statistically-matched comparison group and a four-year follow-up survey. The report addresses four central research questions:

- How effective was the 2002 TAA program in improving access to reemployment services and education and training services and in helping participants obtain educational credentials?

- How effective was the 2002 TAA program in boosting participants' employment and earnings and in improving their access to jobs that offer better pay and fringe benefits? In particular, does TAA boost participants' employment and earnings in the fourth year after job loss (the last year of the observation period, and the primary outcomes for the evaluation)?

- How effective was the 2002 TAA program in promoting access to health care coverage and in reducing receipt of other forms of government assistance?

- How did the 2002 TAA program's impacts differ among participants with different demographic and local area characteristics and among those who access different types of TAA services?

The impact analysis was conducted using samples from 26 randomly selected states, of TAA participants and matched comparison group of workers in the manufacturing sector and from the same local areas. Both groups were Unemployment Insurance (UI) claimants who were separated from their jobs over the same period in 2005 and 2006. The matching was initially conducted using

[1] The TAA program has since been twice amended (in 2009 and 2011) and now differs in significant ways.

demographic information in UI claims files and data on local area characteristics, and was subsequently refined using detailed baseline information from telephone surveys. While the TAA participant sample and its comparison group were statistically similar on a wide array of observable baseline measures, unobservable differences could remain between the two groups that may have biased the estimated impacts.

The main impact findings described in the report used a matched sample of UI claimants as the comparison group for TAA participants, with key outcomes measured through a follow-up survey. However, to test the robustness of the study's conclusions, alternative samples and model specifications were used and data were drawn from alternative sources. In particular, as part of sensitivity analyses, we estimated impacts using matched samples of UI exhaustees and administrative UI wage records. Key findings regarding impact estimates for the 2002 TAA program can be summarized as follows:

- *TAA led to increased receipt of reemployment services*. According to survey data, more than 94 percent of TAA participants received at least one reemployment service, while 77 percent of comparison group members reported doing so. Furthermore, TAA increased access to reemployment services of all types, including those designed to help workers find jobs immediately—with resume assistance or job searches, for example—and those focused on longer-term career planning. Overwhelmingly, the One-Stop Career Center system was the primary source that both TAA participants and their comparisons used to access these services, but TAA participants were much more likely to report that they found the services helpful. TAA also substantially increased the extent to which participants received staff-assisted services from programs funded through the Workforce Investment Act (WIA).

- *Participation in TAA was associated with large increases in receipt of education and training and the attainment of educational credentials*. Nearly 66 percent of TAA participants received training, compared to 27 percent of comparisons, and the average TAA participant spent about 8 times as many weeks in education and training as the average comparison group member (49 weeks, compared to 6 weeks). Impacts on participation in education and training programs were largest during the first two years of the follow-up period, but persisted in the third and fourth years. Furthermore, TAA increased the educational attainment of program participants—about 51 percent had received educational credentials or degrees by the end of the follow-up period while only 21 percent of comparisons did, an impact of 30 percentage points.

- *Impacts of TAA on engagement in any productive activity were small*. To a large degree, TAA participants engaged in training in the period just after job loss, in lieu of seeking and obtaining employment. Thus, impacts on being employed *or* in training—that is, engaging in any productive activity—were not statistically significant, except in the first year, when effects were negative and statistically significant but small.

- *The main impact study findings used the comparison sample of UI claimants and showed that, in the final year of the follow-up period, TAA participants had lower earnings than the comparison group, but worked about the same number of weeks*. As was hypothesized, during the first two years of the observation period, when many TAA participants were in training, the labor market outcomes for

participants were significantly worse than for their matched comparison group members who were not eligible for TAA. During the subsequent two years, the gap between participants and comparisons narrowed. In the final year of the follow-up period, TAA participants earned about $3,300 less than their comparisons, but both groups worked about the same number of weeks (33 weeks for TAA participants, compared to 35 weeks for comparisons). Slightly larger percentages of TAA participants were still in training or had retired at the end of the observation period. The impact findings using the UI claimant sample were very similar when we (1) used UI wage records (rather than survey data) to measure outcomes (though these data were available only up to the 12th quarter), (2) limited the sample to TAA participants who were certified for TAA prior to job loss, (3) excluded workers who were recalled to their jobs, (4) used samples of TAA participants drawn in alternative ways, and (4) used alternative statistical methods to match the TAA participants and comparisons.

- *Impacts for employment and earnings were more positive comparing participants to UI exhaustees*. As a sensitivity test, an alternative specification used a matched set of UI exhaustees as the comparison group. Using this specification, TAA participation was estimated to have a positive effect on employment but no effect on earnings, in the last quarter of the follow-up period. These can be thought of as an upper-bound estimate, because this specification assumes that the decision to exhaust UI is not influenced by the availability of TAA services. The main impact estimates are based on a comparison group with both non-exhaustees and exhaustees, a more conservative approach.

- *Impacts on employment and earnings may be more favorable for TAA participants who received training than for those who received income support without training*. The results for the service receipt subgroups are only suggestive because of potential sample selection biases that make it difficult to identify quality comparison group matches for each service group. The estimated impacts of TAA on average weeks worked and earnings in the fourth year of follow-up did not statistically differ for the two service groups. However, the impacts across the two groups did differ in the final quarter of the follow-up period. In that quarter, the impact on the employment rate was not statistically significant for the trainees, but remained negative and significant for those who had received income support without training. Furthermore, for the main impact study findings, the impact on earnings was less negative for the trainees than those who received income support without training, although it was still statistically significant for both groups.

- *Impacts on employment and earnings became statistically insignificant by the end of the follow-up period for younger participants, but were persistently negative for older participants*. Younger TAA participants, the group with the largest positive training impacts, had the largest negative employment and earnings impacts during the first two years of the follow-up period. However, employment results for them showed steady improvements in the last two years of follow-up, and impacts became statistically insignificant starting by the middle of the third year. By contrast, the employment and earnings impacts for the older age groups remained negative and statistically significant throughout the four-year follow-up period.

- *When TAA participants returned to work, they had lower wages and were less likely to have access to fringe benefits than their comparisons.* In keeping with the overall impacts on employment and earnings discussed above, the gap between participants and comparisons was largest early in the follow-up period. By the end of the follow-up period, the gap in job quality had decreased, but had not completely closed. Trainees fared better than participants who received only income support, although trainees still earned significantly lower average hourly wages than their matched comparisons in their most recent jobs in the final follow-up year. It is possible that trainees could not take full advantage of their new skills, because many re-entered the labor market during the peak of a recession, whereas their matched comparisons (many fewer of whom undertook training) mostly re-entered the labor market earlier.

- *Among TAA participants who received occupational skills training, 37 percent were employed in the occupations for which they trained*. The likelihood that an occupational trainee was employed in his or her training field varied by the occupational focus of the training program. More than 50 percent of trainees in the fields of healthcare practitioner, production, or transportation and material moving were likely to be employed in those fields. By contrast, about one third of trainees who enrolled in programs for office and administrative support, healthcare support, or installation, maintenance and repair found employment in their training fields. Comparison group members were significantly more likely to return to work in production occupations than were TAA participants.

- *Participation in TAA was associated with changes in income from sources other than the workers' own earnings*. Participants collected more in UI payments and were more likely to exhaust their benefits, most likely because TAA provided additional income support while participants completed training or sought employment. However, TAA had a negative impact on total income, suggesting that these additional income payments did not fully compensate for the lower earnings that participants experienced during the study period when many were in training.

- *Participation in TAA was associated with decreased health insurance coverage in the period following job loss*. Despite the availability of a tax credit for health insurance for TAA participants, the loss of employment-based health insurance coverage reduced coverage among TAA participants overall, especially for trainees. This negative impact decreased somewhat later in the study period.

- *Participation in TAA did not impact family structure*. Participants and their comparisons tended to retain their family structures and housing situations throughout the study period. In addition, TAA had very small effects on worker mobility.

Overall, although TAA participation substantially increased the receipt of reemployment and education and training services, these impacts had not yet translated into labor market gains during the four-year period following job loss.

## A. ABOUT THE TAA PROGRAM

To be eligible for TAA benefits and services, a worker must be covered by a petition certified for TAA. Petitions are filed by an eligible entity (employers, unions, One-Stop operators or partners, among others) with ETA, and, once it receives the petition, ETA certifies the affected worker group for TAA if it determines that the job losses occurred as a consequence of foreign competition. The date on which a petition is certified for TAA is the *determination date*. Recognizing that layoffs may have occurred before the petition was certified, or may occur after certification, TAA allows workers to be covered by the certified petition if they have experienced full or partial separations within a date range defined as beginning with the *impact date*, which is usually one year before the date the petition was filed, and ending with the *termination date*, which is usually two years after the determination date. This range represents slightly more than a three-year period of participant eligibility.

Once the petition is certified, members of the affected worker group are eligible for training, income support, and various other services. The precise nature of allowable services has changed as TAA's enabling legislation has changed, as it did most recently with the Trade Adjustment Assistance Extension Act of 2011 (TAAEA). However, for the sample whose impacts are estimated in this report, the rules of the Trade Act of 2002 held sway. Under this legislation, training for occupational skills can be paid by TAA for up to two years, and can include classroom training, on-the-job training (OJT), and other customized training. Remedial training can also be provided if it is required for the worker to successfully complete occupational skills training or to take full advantage of his or her existing occupational skills.

Trade Readjustment Allowances (TRA) are weekly cash payments made to eligible workers in amounts equivalent to their UI benefits. TRA is paid once UI benefits are exhausted and is of two main types, basic TRA and additional TRA. Basic TRA is available to workers for 26 weeks following the exhaustion of UI and can be paid if the worker is in training, has completed training, or has been granted a waiver from the training requirement. Additional TRA, which has a weekly payment amount identical to basic TRA, can be provided for an additional 52 weeks once basic TRA ends and is only available to workers while they are in training (no waivers are allowed). TRA can be provided for up to an additional 26 weeks for workers in remedial training.

In the interest of promoting rapid reemployment, and because training may not pay off for older workers before they retire, the Trade Act of 2002 established a wage supplement program, Alternative Trade Adjustment Assistance for Older Workers (ATAA). ATAA, available for workers ages 50 and older, pays up to 50 percent of the difference between the wage in the worker's new job and the wage at separation. Benefits can be paid for up to two years, up to a maximum payment amount of $10,000.

The Health Coverage Tax Credit (HCTC) is another new benefit established with the Trade Act of 2002. The tax credit equals 65 percent of the cost of health insurance coverage for the individual and qualified family members.

TAA also provides a range of other services, although they are not very common. If suitable local-area employment is not available, a job search allowance can reimburse a worker for the costs associated with conducting job search out of the local area, and relocation allowances provide partial reimbursement to cover costs of relocating to take advantage of job opportunities elsewhere.

Supplemental assistance is available to cover travel and subsistence expenses for workers who need to live away from home to participate in training. Case management services are also available, although on a limited basis, because, under the Trade Act of 2002, it is intended that TAA participants will receive most case management assistance through TAA's partners in the One-Stop system, including through the Employment Service (ES) and WIA.

## B. RESEARCH DESIGN

Random assignment, the ideal design for estimating program impacts, was not feasible for the evaluation, because TAA is an entitlement program and, hence, services cannot be denied to workers determined eligible. Consequently, the evaluation used a comparison group (propensity score matching) design to obtain nationally representative estimates of the impact of TAA on participants' employment and other outcomes.

The evaluation samples of eligible TAA workers were selected in two stages. In the first stage, 26 states were randomly selected with probabilities proportional to the expected number of TAA participants in the state. All 26 states agreed to participate in the study, and together they contain about 90 percent of all TAA-eligible workers nationwide.

In the second stage, the following two samples of eligible TAA workers were selected from each of those 26 states:

1. ***The "certified worker sample."*** This sample, which was the one primarily used for the analysis, includes workers whose names appeared on lists of workers covered by petitions certified for TAA. These lists, referred to in this study as certified worker lists, were obtained by states from employers as part of TAA's worker notification requirements. In drawing the sample, we required that workers on these lists received a first UI payment from the state in which the firm named on the petition was located. Because only about one half of workers eligible for TAA services received significant program services, we subdivided the certified worker sample into two groups:

   - *TAA participants*—those who received a significant TAA service.

   - *TAA nonparticipants*—those who did not receive significant TAA services, but who might have received light-touch TAA services or One-Stop core services provided through WIA or ES.

   Note that TAA *participants* and TAA *nonparticipants* are both considered treatment groups, but impacts are estimated separately for each group. (Impacts for TAA nonparticipants are described in detail in a separate report.)

2. ***The "TRA-beneficiary sample."*** This supplementary sample consists of workers who received TRA after they exhausted their regular UI benefits. These workers had similar UI claim dates as the TAA certified worker sample.

This report focuses on impacts for those who received significant TAA services and thus makes use of the *TAA participant* and *TRA-beneficiary* samples. An important evaluation design feature was the selection of these two separate TAA treatment samples. The two samples received TAA services at roughly the same time, but the samples were drawn in different ways, thus allowing an

assessment of the robustness of study findings when impacts were estimated for the two groups separately.

To select the certified worker samples, we collected certified worker lists and UI/TRA claims data from each of the 26 study states. The latter source contains demographic information on UI claimants and information on their TRA and UI benefit receipt. The claims data were merged with the certified worker lists for each state, and the certified worker sample frame was then defined to include the following workers:

- *Workers on the certified worker lists covered by petitions that became certified for TAA between November 1, 2005 and October 31, 2006*. We specified a one-year window to account for potential seasonal layoff patterns.

- *Those whose UI benefit years started between September 1, 2004 and October 31, 2008*. Workers are covered by a certified petition if they experience job separation between one year prior to the petition filing date and two years after the petition certification date. Because it typically takes ETA one to two months to make certification determinations once it receives a petition, the slightly more than four-year benefit period was selected to include workers covered by the petition certification window specified above.

The sample frame was further restricted to workers who were between the ages of 16 and 80 at the time of the UI claim and had non-missing values for key data items, including telephone numbers and variables that the study required for matching. The sample frame includes about 55,000 TAA-eligible workers nationwide.

This group was divided into TAA participants and TAA nonparticipants. TAA participants were initially defined as those who received TRA benefits according to the initial extracts of UI/TRA claims data that the states provided, and TAA nonparticipants were initially defined as those who had not received TRA benefits. Using these designations, we randomly selected the following certified worker samples for data collection:

- 2,875 participants and 1,506 nonparticipants for initial telephone interviews (the "survey sample"); and

- A larger sample of 7,546 participants and 12,452 nonparticipants for whom only administrative records data would be collected (the "administrative data sample").

The initial TAA participant and nonparticipant designations were subsequently updated using additional administrative data we collected from states, including TAA participant data from the Trade Act Participant Reports (TAPRs) and updated TRA benefit information from additional UI/TRA claimant extract files. Participation status was also updated based on information about TAA service receipt elicited from survey respondents (for the survey sample only). About 25 percent of nonparticipants were reclassified as participants using these sources.

To assess the robustness of the impact findings, we also selected a nationally representative sample from the universe of TRA beneficiaries. The TRA-beneficiary sample frame includes those in the UI claims files who received their first TRA payments anytime between January 1, 2006 and December 31, 2006 (regardless of whether they appeared on one of the certified worker lists that the

states provided us), and who satisfied the other age and data requirements discussed above for the certified worker sample. The TRA-beneficiary sample frame includes about 30,000 workers.

Using the UI/TRA claims data, we selected separate comparison samples for: (1) participants in the certified worker survey sample, (2) participants in the certified worker administrative records sample, (3) the TRA-beneficiary sample, (4) TAA-eligible nonparticipants in the certified worker survey sample, and (5) TAA-eligible nonparticipants in the certified worker administrative records sample. Thus, in total, we selected 130 matched comparison samples for the five treatment groups across the 26 study states.

We identified the pool of potential comparison group members from the UI/TRA claims data as follows:

- We aligned the treatment and comparison samples by limiting the comparison groups for the certified worker samples to those who started collecting regular UI benefits (or, for the TRA-beneficiary comparison group, TRA benefits) during a period comparable to the treatment groups.
- Because TAA eligibility is restricted to those in the goods-producing sector, we limited the comparison sample to those in the manufacturing industry.
- We excluded workers from the comparison group who were on worker lists certified for TAA or who received TAA services.
- We limited the potential comparison pool to workers who lived in the same local areas within each state as the treatment group members, and to those between the ages of 16 and 80 who had non-missing values for key variables.

The variables used in the initial matching process were constructed from the UI/TRA claims data, and included demographics (gender, age, race/ethnicity), job characteristics (base period earnings), and UI claim and benefit information (benefit year begin date, date of UI first payment, and UI maximum benefit amount). Using geographic identifiers on the UI/TRA claims files, additional indicators relating to the workers' local area characteristics were also used (unemployment rate, poverty rate, percentage of workers in manufacturing, average earnings per job, percentage population growth, and urbanicity). Matching on geographic factors was critical, because many TAA participants were dislocated from jobs in rural communities that were heavily trade impacted.

We used propensity score matching methods to select the study comparison groups. For the survey sample, we endeavored to complete interviews with two comparison workers for every treatment worker. Operationally, we matched each treatment worker to the five comparison group members with the closest propensity scores and released the first two best matches for interviewing (three matches were held in reserve in case future releases were necessary). Nearest neighbor matching was performed with replacement so that each comparison group member could be matched to multiple treatment group members. Conducting initial interviews with a comparison sample that was twice as large as the treatment sample allowed for a second stage of matching that used the richer variables from the initial survey. We also selected two comparisons for each certified worker or TRA beneficiary in the administrative data samples.

The TAA participants and comparisons came from similar areas (with the exception of a small percentage of participants found in rural areas), and tests of the matching confirmed that TAA participants in the survey and administrative data samples were very well balanced with their comparisons on the array of demographic, job history, UI claims, and geographic matching characteristics used. (These matching tests are described in the report and, more fully, in a companion methodological report.) Nonetheless, differences between the groups on unobservable characteristics, including elements of the jobs or geographic areas from which workers were displaced, could remain.

## C. DATA AND METHODS

Outcome data for the impact analysis were obtained from several sources:

- *Initial interviews.* These interviews represented our first contact with the sample, and were conducted an average of 29 months after the UI claim date.
- *Follow-up interviews.* These interviews were conducted an average of 51 months after the UI claim date.
- *Administrative records.* Data of the following types were collected from the states:
  - UI wage records data
  - UI/TRA claimant data for the period subsequent to the job loss
  - TAPR data, showing TAA services received
  - Data from the Workforce Investment Act Standardized Record Data (WIASRD), showing services received by TAA participants and comparisons funded through WIA.

Initial and follow-up interviews were conducted by telephone with the participant sample and its matched comparisons. About 63 percent of the TAA participants selected for interviewing for the initial survey completed the follow-up survey, and the survey was completed with 2,054 TAA participants and 1,796 comparisons. Administrative records data were collected for all samples, and data are available for 10,476 certified worker TAA participants and 16,282 of their comparisons, as well as for 9,877 TRA beneficiaries and 15,266 of their comparisons.

The primary sample used for the impact estimates was comprised of the follow-up survey respondents in both the participant treatment group and the comparison group. To construct this analysis sample, we "re-matched" (i.e., statistically adjusted) the comparison group to be like the TAA participant group, using the initial matching variables and the richer data on pre-layoff characteristics available from the initial survey. This was important, because the initial survey revealed some differences between the two groups in pre-layoff characteristics that were not known in the initial matching (such as job tenure and recall status). The survey data items used for the re-matching included (among other characteristics): highest grade completed, marital status and family composition, health status, home ownership, household income, total earnings in the year prior to the claim, number of jobs held in the three years prior to the claim, reason for job loss, expectation of being recalled, union status, occupation of the job of dislocation, receipt of fringe benefits on the

job of dislocation, firm size, hourly wage on the job of dislocation, and job tenure on the job of dislocation.

We used a "kernel" matching algorithm for the re-matching process, where each TAA participant was compared to all comparison group members in the follow-up sample. This process led to matched participant and (weighted) comparison survey samples that were balanced on all the matching variables, including the pre-layoff variables from the initial survey. We found no statistically significant participant-comparison differences in the distributions of any of the matching variables. Differences between the two groups on unmeasured characteristics could nonetheless remain, including on variables on which we ideally would have liked to match, such as detailed information on the availability of jobs in the industry from which the worker was dislocated.

The UI claim date associated with a worker's trade-related dislocation (and the comparable claim date for the comparison) demarcates the beginning of the follow-up period for the impact analysis. Thus, outcomes were measured in each quarter following the "trigger" UI claim. The follow-up period was 16 quarters for most analyses, but was 12 quarters for analyses based on the UI wage records. Some outcome measures pertain only to the time of the interview.

We estimated impacts using regression methods, where each study outcome was regressed on a treatment status indicator variable and a fixed set of baseline covariates. The outcomes of the comparison group represent the counterfactual for the study—that is, the outcomes that the TAA participants would have experienced in the absence of the TAA program. Baseline covariates were used in the analysis to improve the precision of the impact estimates. All impact estimates were calculated using sample weights (which adjusted for survey nonresponse for the survey sample) and the standard errors of the impact estimates were inflated to account for design effects due to unequal weighting and state-level clustering.

The *confirmatory* analyses for the evaluation pertain to TAA impacts on employment and earnings in the fourth year of follow-up. These analyses address the key research questions that were specified in the study protocols, and are the impacts that the study was powered to detect. The impact findings for other study outcomes with weaker a priori hypotheses about expected program impacts and for population subgroups are deemed as exploratory.

## D. THE DECISION TO PARTICIPATE IN TAA

For purposes of this study, we defined a TAA participant to be an individual who received a significant TAA service, including TAA-funded training, TRA, ATAA, HCTC, a job search allowance, a subsistence or travel allowance (for those in training), or a relocation allowance. This definition excludes the substantial number of workers who received only waivers; they were excluded on the grounds that waivers are inconsistently recorded in the states' TAPRs and because these individuals effectively received nothing beyond One-Stop core services.

Using this definition, about 50 percent of those eligible for TAA became TAA participants. This estimate is essentially the same regardless of whether it is calculated from administrative data that states provided or from baseline survey data. However, the TAA participation rate differed markedly from state to state—in some states, no more than 30 percent of eligibles participated, while in other states more than 80 percent did. About 70 percent of persons who became TAA

participants cited their interest in training or schooling as a reason for applying to the program. Other reasons, such as an interest in receiving TRA or HCTC, were much less commonly cited.

## E. PARTICIPANTS' EXPERIENCES IN TAA

Among TAA participants, 93 percent received TRA and about one half received TAA-funded training. Receipt of ATAA, HCTC, and job search, relocation, subsistence, and travel allowances were much less common. However, states differed markedly from each other in the extent to which their participants accessed training.

Given that TRA and training were the most common service categories, we defined two major TAA subgroups: those who received TRA without training (which we refer to as the TRA-only subgroup) and those who received training, most of whom also received TRA (which we refer to as the trainee subgroup). Younger workers were significantly more likely to be in the training subgroup than those who were older.

Once eligible for TAA, workers who became participants tended to access services quickly, with about half doing so within the first six weeks. Their duration of participation—defined as time elapsed between the date of first service and date of last service—was quite variable; participants can be divided into roughly three equal groups, with about one-third participating for up to one year, another third participating for between one and two years, and another third participating for more than two years. The average duration of participation was considerably longer for trainees than it was for those accessing other services, with trainees receiving an average of 89 weeks of service.

About 27 percent of participants filed their claims more than 90 days before they became eligible for TAA. Thus, it took longer for participants to access services from the date of the trigger UI claim than from the date of their TAA eligibility. This situation primarily occurred when a worker experienced a job separation and then filed a UI claim before the petition under which the worker was covered was certified, or when a worker experienced a separation and filed a claim, was later recalled, and then experienced a subsequent separation under which he or she received TAA services, all within the same UI benefit year.

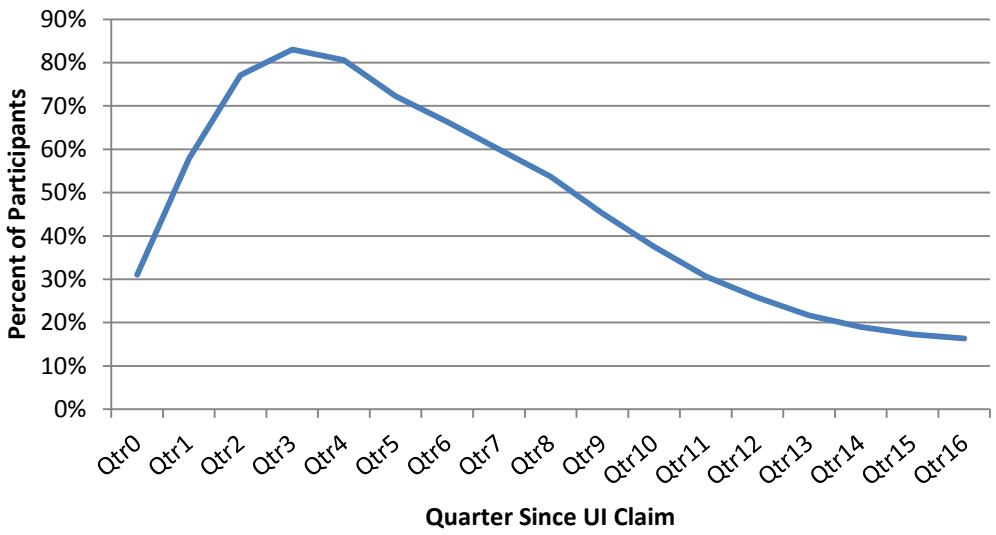
The percent of TAA participants enrolled in TAA in any quarter following the UI trigger claim quarter displays the pattern shown in Exhibit 1. About 30 percent of participants were participating in TAA beginning with the quarter in which their claims occurred, and the participation rate rose sharply in the several quarters thereafter, reaching a peak of 83 percent by the third quarter after the trigger quarter. The participation rate dropped steadily over subsequent quarters, but, even by quarter 16, more than 16 percent were still participating.

Participation rates were higher for trainees than for the TRA-only subgroup in every quarter and the average number of weeks of participation was greater for them—the average duration in the program was 89 weeks for trainees and 64 weeks for the TRA-only subgroup. The duration for trainees included an average of 54 weeks that they were participating in training, plus additional time before or after training while they were receiving other services (e.g., pre-training assistance in selecting a training program and post-training placement assistance).

## F. IMPACTS ON REEMPLOYMENT SERVICES

The Trade Act of 2002 and ETA's accompanying guidance emphasize that trade-affected workers should be provided access to early intervention and reemployment services as soon as a petition is filed, even before a certification decision has been made. Furthermore, ETA notes that TAA, operating in the context of the One-Stop Career Center system, should not attempt to duplicate services that are already available in the One-Stop system. Therefore, the co-enrollment of workers in WIA might be the most efficient means of delivering necessary reemployment and case management services, even if the workers become TAA participants.

**Exhibit 1: Percent of TAA Participants Utilizing TAA Services since UI Claim Date**




Source: Administrative data.

Comparison group workers came from the same local areas as the TAA participants, however, and thus were likely to have been in similar proximity to One-Stop system access points. We found that, indeed, more than three quarters of those in the comparison group accessed reemployment services since their job losses, and almost all of them accessed these services through the One-Stop system. These facts suggest the widespread accessibility of these services to the unemployed.

Nonetheless, TAA substantially increased receipt of reemployment services—94 percent of TAA participants received at least one reemployment service, while 77 percent of comparison group members reported doing so, a statistically significant gain of 17 percentage points. According to the initial survey data, TAA increased access to reemployment services of all types, including those designed to help workers find jobs quickly—with resume assistance or job searches, for example—and those focused on longer-term career planning. Overwhelmingly, the One-Stop Career Center system was the primary source that TAA participants and their comparisons used to access these services. TAA participants were much more likely to report that they found the services helpful.

TAA also substantially increased the extent to which participants received WIA staff-assisted services. Whereas only 4 percent of comparisons received WIA staff-assisted services, 38 percent of

TAA participants did so, a statistically significant increase of 34 percentage points. The comparison group's high rates of receipt of reemployment services through the One-Stop system identified in the survey data, coupled with much lower rates of enrollment in WIA staff-assisted services, suggest that many comparisons accessed only One-Stop self-services or informational services, and were doing so perhaps as part of their registration for UI.

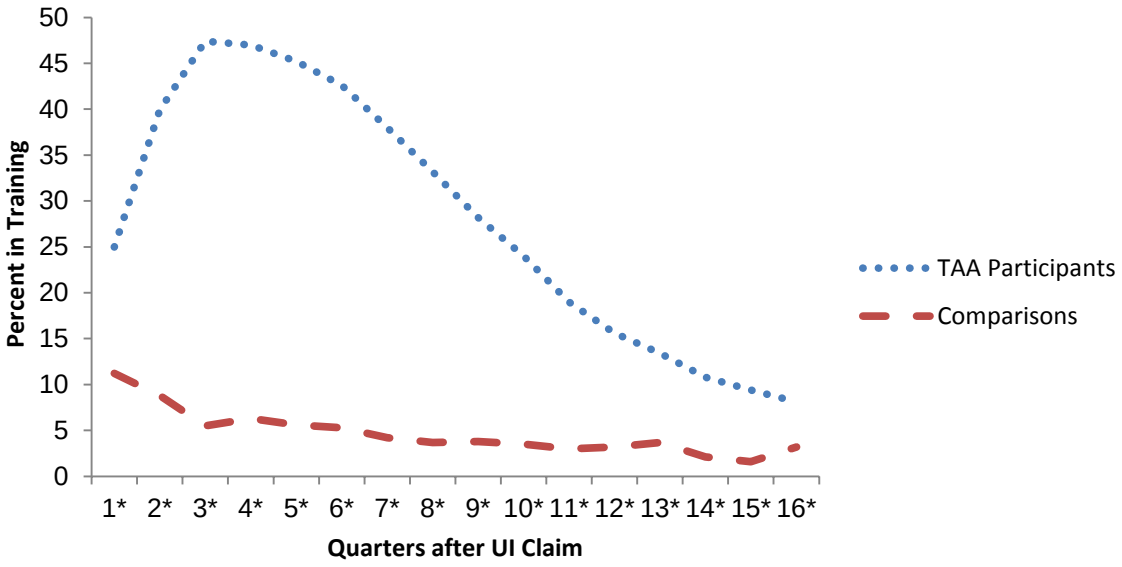
## G. IMPACTS ON EDUCATION AND TRAINING

TAA aims to improve workers' marketable skills to help them rebound from their job losses. A chief way it does so is by providing generous support for education and training. Therefore, we would expect that participation in TAA should increase enrollment in education and training programs and increase educational attainment.

We found that indeed it does. The TAA program had a substantial impact on the amount of education and training received by participants. Nearly 66 percent received training of some type (funded through TAA or other sources) compared to 27 percent of comparisons, a statistically significant impact of 39 percentage points. Furthermore, the average TAA participant spent about 8 times as many weeks in education and training as the average comparison group member (49 weeks, compared to 6 weeks). TAA participants were significantly more likely to have received remedial education or non-occupational higher education, but the largest program impacts were on the receipt of occupational skills training.

Impacts on participation in education and training programs were largest during the first two years of the follow-up period, but persisted in the third and fourth years (Exhibit 2). Even in quarter 16, TAA participants were still significantly more likely to be enrolled in training than comparisons (8 percent versus 3 percent).

**Exhibit 2: Participation in Education and Training, by Quarters after UI Claim**




*Source*: TAA Initial and Follow-up Surveys.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

TAA also increased the educational attainment of program participants. More than 50 percent of participants received educational credentials or degrees, an impact over comparisons of 30 percentage points. This increase in educational attainment was also evident in participants' reported highest levels of education. By the final interview, TAA participants were significantly more likely to report having received vocational certificates or associate's degrees, and were significantly less likely to report that high school diplomas were their highest levels of education. In addition, TAA participation significantly increased the attainment of a GED for workers without a high school diploma at program entry.

TAA substantially increased the receipt of training for participants of all ages. However, the program's impacts on training were typically larger for the younger participants than for the older ones, especially with respect to occupational training programs. Younger workers in general (in both the participant and comparison samples) were more likely to take part in training than those who were older.

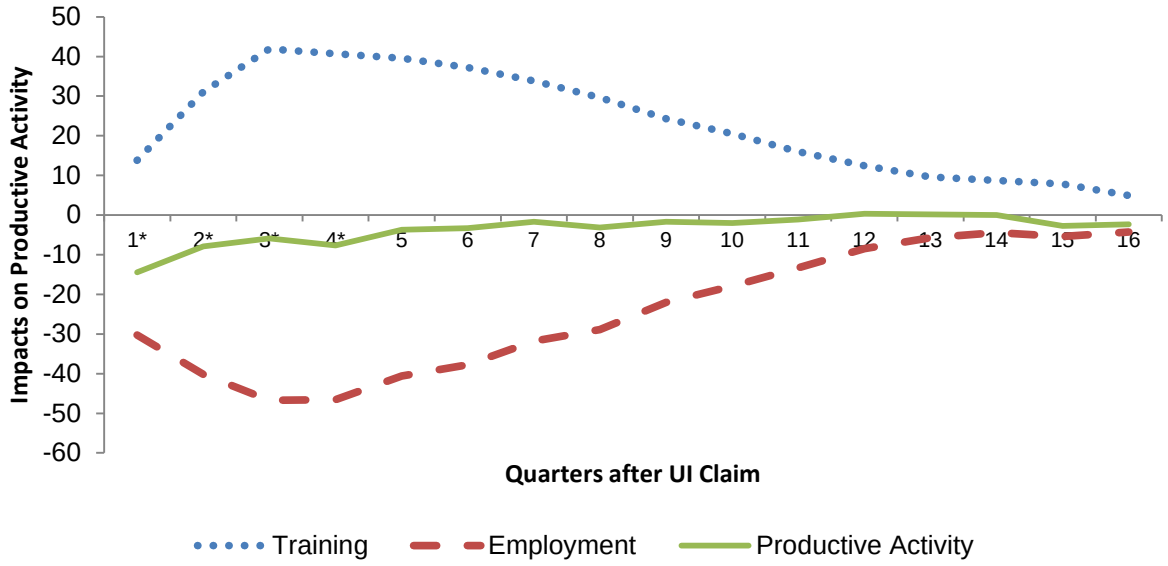
## H. IMPACTS ON EMPLOYMENT AND EARNINGS

Because of TAA's impacts on the receipt of reemployment services and time spent in education and training programs, the program could be expected to *decrease* participants' employment and earnings in the short run but increase their productivity, marketability, and employability in the longer run, as measured by increases in their eventual labor force participation and earnings.

Using survey data, we first estimated quarterly impacts on being either employed or in training—that is, engaging in *any* productive activity. As shown in Exhibit 3, these impacts were small throughout the four-year follow-up period—small and negative in the first year following job loss, and not statistically significant in subsequent quarters. These negligible impacts on any productive activity came about because the positive estimated impacts on training participation that were described above were largely offset by negative estimated impacts on employment.

We next looked at impacts on employment and earnings more closely. In our main impact study findings, we found that TAA participants suffered steep decreases in employment and earnings relative to their comparisons in the two years following their trigger claims. However, by the fourth year of the follow-up period, the gap between the participants and the comparisons had narrowed—there was no significant difference in weeks of employment between TAA participants and comparisons by the fourth year of follow-up, but TAA participants still earned about $3,300 less, on average, than comparisons, a statistically significant difference. Table 1 shows the impacts on employment and earnings.

## Exhibit 3: Impacts on Productive Activity




*Source*: TAA Initial and Follow-up Surveys.

* Impact of TAA on productive activity is significantly different from zero at the 0.05 level, two-tailed test.

## Table 1: Impacts on Employment and Earnings

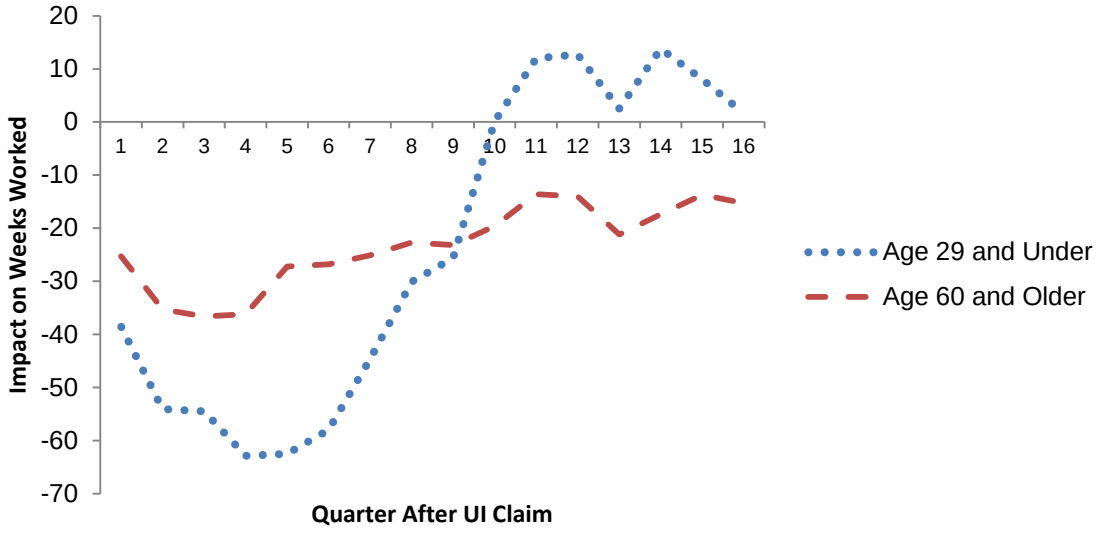
| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Weeks of Employment | | | | |
| Quarters 1 – 4 | 6.0 | 25.5 | -19.4*** | 0.9 |
| Quarters 5 – 8 | 18.9 | 37.8 | -18.9*** | 1.2 |
| Quarters 9 – 12 | 28.1 | 37.1 | -9.0*** | 1.1 |
| Quarters 13 – 16 | 33.0 | 35.0 | -2.0 | 1.3 |
| Annual Earnings (2006$) | | | | |
| Quarters 1 – 4 | 3,053 | 15,728 | -12,674*** | 795 |
| Quarters 5 – 8 | 9,574 | 22,561 | -12,987*** | 909 |
| Quarters 9 – 12 | 13,548 | 20,999 | -7,451*** | 871 |
| Quarters 13 – 16 | 15,917 | 19,189 | -3,273*** | 883 |
| Total Earnings, Quarters 1 – 16 | 42,939 | 80,072 | -37,133*** | 3,289 |
| Sample Size, Quarters 1 – 12 | 1,989 | 1,622 | | |
| Sample Size, Quarters 13 – 16 | 1,340 | 1,089 | | |

*Source*: TAA Initial and Follow-up Surveys.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

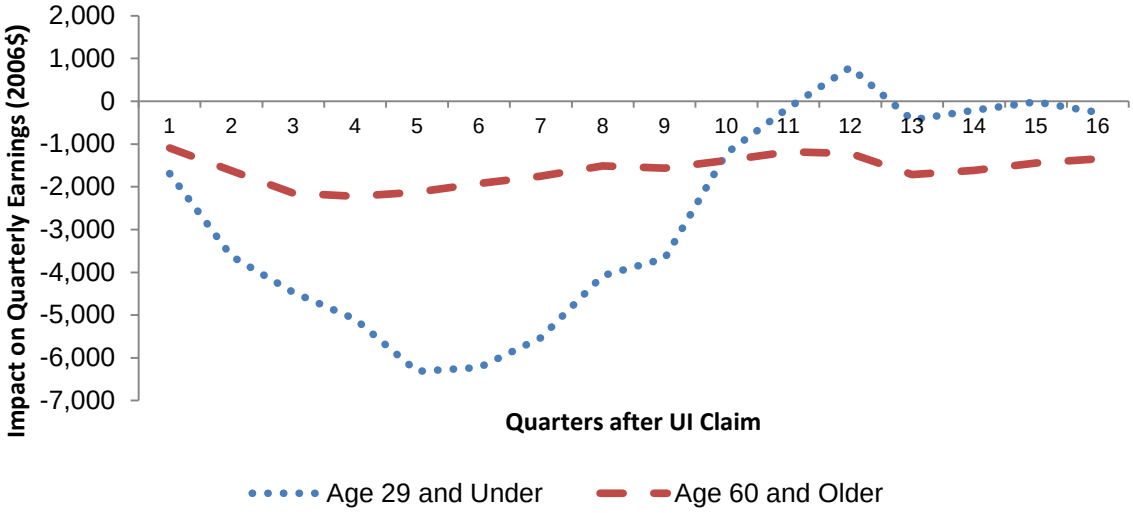
This pattern of findings was particularly strong for younger TAA participants, the group with the most positive training impacts. Among all age groups, these younger workers had the largest negative employment and earnings impacts during the first two years of the follow-up period, but the impacts became statistically insignificant starting in quarter 10. In contrast, the impacts for the older age groups remained negative and statistically significant throughout the follow-up period. Exhibits 4 and 5 show these findings, for weeks worked and earnings, respectively.

**Exhibit 4: Impacts on Weeks Worked, by Age at UI Claim**




*Source*: TAA Initial and Follow-up Surveys.

**Exhibit 5: Impacts on Earnings, by Age at UI Claim**

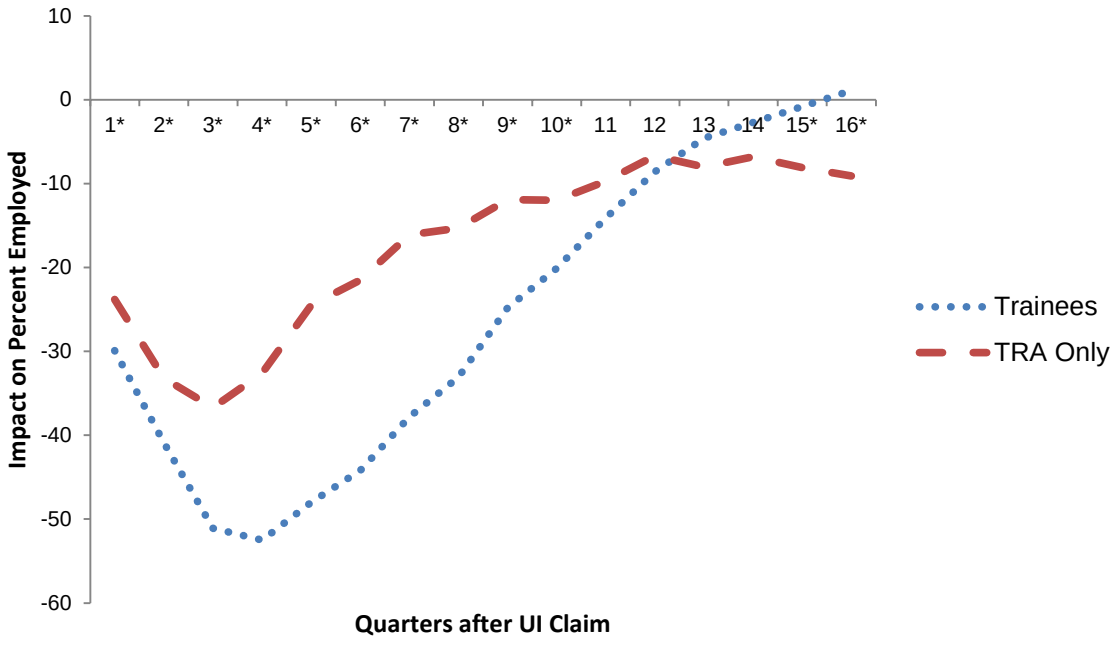



*Source*: TAA Initial and Follow-up Surveys.

We also estimated impacts by service receipt subgroups. For those participants who undertook training, we anticipated that TAA would reduce employment and earnings during the period of training, because it is likely that some of these workers would have held jobs if TAA-funded training had not been an option. However, as TAA trainees leave their training programs, we expected their employment and earnings to rise after a period of adjustment. Expectations were less clear for the TRA-only subgroup. On the one hand, TAA-induced increases in the receipt of reemployment services could increase the reemployment rates of TRA-only workers soon after job loss; on the other hand, the offer of TRA benefits could induce some workers to extend their unemployment spells and exhaust their UI benefits without increased job search effort, which could lead to short- and, perhaps, long-term earnings reductions.

While the results for the service receipt subgroups are only suggestive because of potential sample selection biases that could have led to comparison group matches that are of questionable quality, we found that by the end of the follow-up period labor market impacts were more favorable for the trainees than the TRA-only participants. The impact on average weeks worked in the fourth year of follow-up was not statistically significant for the trainees, but remained negative and significant for the TRA-only participants. Furthermore, the impact on earnings in the fourth year was less negative for the trainees than for the TRA-only group (although it was statistically significant for both groups). Exhibits 6 and 7 show these results, for employment rates and earnings, respectively.

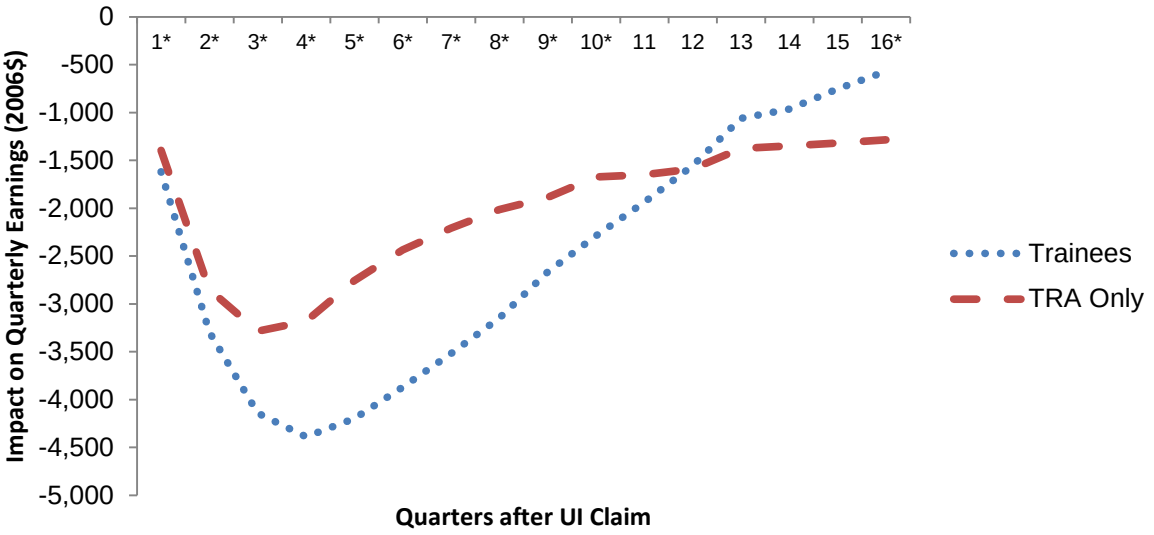
**Exhibit 6: Impacts on Employment Rates, by TAA Service Receipt**




*Source*: TAA Initial and Follow-up Surveys.

* Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

**Exhibit 7: Impacts on Earnings, by TAA Service Receipt**



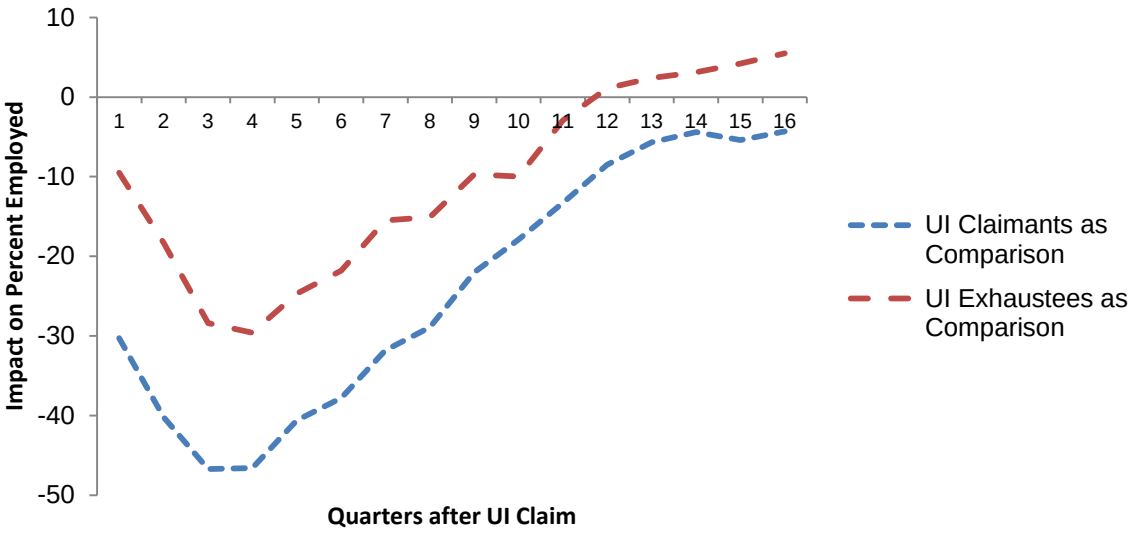

*Source*: TAA Initial and Follow-up Surveys.

* Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

We conducted a series of sensitivity tests to examine the robustness of the impact findings. In one such test, we estimated impacts by limiting the TAA participants and matched comparison samples to UI *exhaustees*. The strong advantage of using UI *claimants* for the analysis, as we did originally, is that all events that occurred after the job separation date should arguably be thought of as outcomes and could be a consequence of the offer of TAA services. For example, those eligible for TAA could have exhausted UI because they decided to take up the offer of TAA-funded training and become TRA recipients. In fact, more than 80 percent of the TAA participants exhausted UI (and two-thirds enrolled in training), compared to a UI exhaustion rate of about 45 percent for matched comparisons from the same local areas. A counterargument is that many TAA eligibles exhausted UI and became TAA participants when their job search efforts immediately after job loss proved unsuccessful. In this case, UI exhaustion should not be thought of as an outcome, but rather as a proxy measure for the workers' employability. We endeavored to control for their employability through the extensive baseline characteristics we used in the matching when selecting comparison group members. However, there could be some remaining unobservables that give rise to negative selectivity among the TAA participant sample members relative to their comparisons.

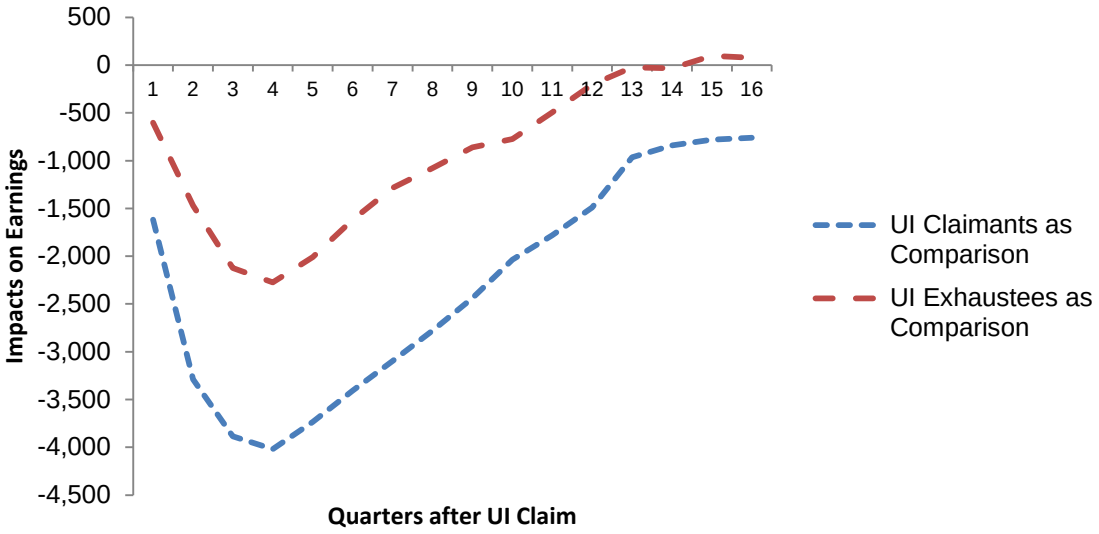
As a sensitivity test, we therefore limited the TAA participant and comparison workers to UI exhaustees. We view this specification as representing an upper-bound estimate of the effects of TAA, because it assumes that the decision to exhaust UI is not influenced by the availability of TAA services. By contrast, the full comparison sample with both exhaustees and non-exhaustees is a more conservative approach, typical of much social science research. Although the "true" impacts cannot be known, it is plausible that they lie somewhere between the two sets of estimates. Using this alternative specification, by the last quarter of the follow-up period TAA had a positive impact on employment and no effect on earnings (see Exhibits 8 and 9).

## Exhibit 8: Impacts on Employment Rates Using Samples of UI Exhaustees versus UI Claimants




*Source*: TAA Initial and Follow-up Surveys and UI Claims data.

## Exhibit 9: Impacts on Earnings Using Samples of UI Exhaustees versus UI Claimants




*Source*: TAA Initial and Follow-up Surveys and UI Claims data.

Among other sensitivity tests, we estimated impacts using UI wage record data rather than survey data to measure outcomes; restricting the sample to TAA participants who were certified for TAA prior to their trigger claim dates; using the TRA-beneficiary or certified worker administrative data samples (rather than the certified worker survey sample, which is our benchmark); restricting TAA participants and comparisons to those who were not recalled; and using alternative matching algorithms. In all these specifications, results were substantively unchanged from those reported for the survey sample using UI claimants as the comparison group.

TAA participants and their comparisons were all employed in manufacturing jobs prior to filing their UI claims, but we did not match on the workers' three-digit industry classification *within* the manufacturing category due to small cell sizes. However, late in the project, ETA obtained state-level data from the Quarterly Census of Employment and Wages (QCEW) that enabled us to compute the percent change in private industry employment between 2004 and 2009 by three-digit industry. We used these data to construct an industry growth rate measure and constructed revised kernel weights with this additional matching variable. We estimated very similar employment and earnings impacts using the original and these revised kernel weights.

As an additional sensitivity test, we found zero impacts on employment and earnings using the sample of TAA *nonparticipants* and their matched comparisons. This lends credibility to the impact findings for the TAA participants, because the direction of the selection biases (if there are any biases) for the nonparticipants is likely to be *opposite* that for the TAA participants (that is, the nonparticipants may have decided not to participate in the program because they found jobs quickly and, thus, may have been more "employable" than their matched comparisons).

We also estimated impacts for the sample of TAA participants and TAA nonparticipants combined—that is, for TAA eligibles whether or not they accessed a significant TAA service. The estimation of impacts for this combined sample has methodological appeal, because potential sample selection biases discussed earlier for the separate participant and nonparticipant samples might be largely offsetting using the combined sample. Because impacts estimated for nonparticipants on employment and earnings were near zero, and TAA participants and nonparticipants each made up about one-half of the total sample of eligibles, impacts for the combined sample are about half of what they were for the TAA participant sample, with few changes in the levels of statistical significance.

We also estimated impacts for additional subgroups. We found no significant differences in impacts by subgroups defined by gender, family composition, or education level at program entry. We did, however, find some impact differences by race/ethnicity, health status, and key local area characteristics. The negative earnings impacts in the fourth year were limited to whites, those in good health, and those living in non-metropolitan and high unemployment areas at the time of job loss.

We did not find differences in impacts for participants who co-enrolled in WIA and those who did not—both groups earned significantly less than their comparisons throughout the follow-up period, but the gap narrowed somewhat over time for both groups. We found some highly suggestive evidence that ATAA improved the earnings of age-eligible participants, although very few participants in our sample enrolled in this program component.

Finally, we found differences in impacts on earnings in the fourth year based on the time of post-program exposure to the economic recession that started in December 2007. For TAA participants who filed for UI in 2006 or later, there was no impact on employment or earnings in the final follow-up year. In contrast, TAA participants who filed prior to 2006 had significantly worse employment and earnings outcomes than comparisons throughout the follow-up period. Interestingly, the employment rates and earnings of the TAA participants were almost equivalent for these two cohorts, and the difference in impacts across them was driven entirely by the employment outcomes of comparisons. Comparisons who filed their UI claims in 2004 or 2005 were able to return to the labor market before the economic conditions deteriorated, whereas comparison workers who lost their jobs in later years and TAA participants who engaged in training faced more challenging economic conditions in which to find work. We cannot say, however, whether the impacts of the TAA program and the economic returns to training would have been different if the economic conditions had been better.

## I. IMPACTS ON JOB CHARACTERISTICS

The results pertaining to employment and earnings discussed above take a broad-brush approach to looking at employment impacts. But TAA participants and comparisons could differ with regard to more fine-grained dimensions of employment, such as the timing of reemployment, hourly wages, and receipt of fringe benefits. We found impacts of TAA on these aspects of employment as well.

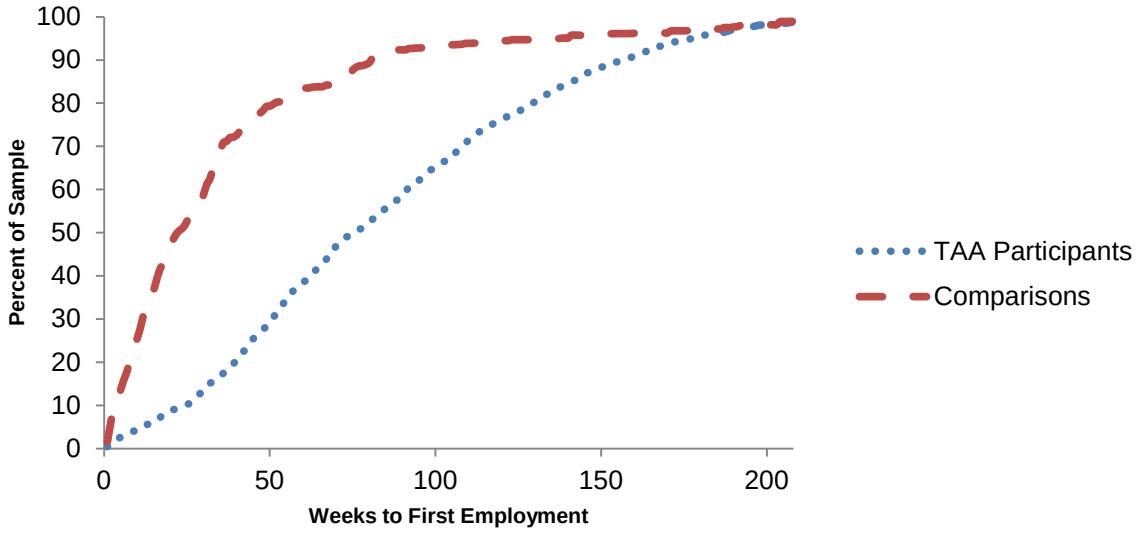
Among those who became reemployed, comparisons returned to work much more quickly than TAA participants. For comparison workers, an average of 36 weeks elapsed between their UI claim and initial reemployment dates, compared to 86 weeks for TAA participants, a significant difference of 50 weeks. Similarly, within the first year after their UI claims, 80 percent of the comparisons who ever became reemployed during the follow-up period had returned to work, compared to about 30 percent for TAA participants.

While the share of comparisons who had found employment increased steeply in the first year, the time pattern of reemployment for TAA participants was quite different. As Exhibit 10 shows, the growth of the participants' reemployment rates was more evenly spaced across the first three years after job loss. Both TAA trainees and TRA-only participants took significantly longer than their matched comparisons to become reemployed, but the difference was much greater for trainees.

TAA participants also had lower hourly wages in their jobs at first reemployment than comparisons. TAA participants earned $11.70 on average, compared with $13.20 for comparisons, for a difference of -$1.50. These differences in hourly wage rates led to a significant difference in the wage replacement rate. In the first post-UI job, the average reemployed comparison earned 91 percent of his or her pre-UI wages, compared to 83 percent for the average reemployed participant.

The availability of job benefits is another important indicator of job quality and, again, TAA participants fared less well. A majority of comparisons found initial reemployment in jobs that offered health insurance (71 percent), paid time off (75 percent), and retirement benefits (57 percent). TAA participants were significantly less likely to have each of these benefits available to them in their first post-UI jobs, with rates of 57 percent, 60 percent, and 44 percent, respectively.

**Exhibit 10: Cumulative Frequency of Timing of Reemployment (Survey Data)**




*Source*: TAA Initial and Follow-up Surveys.

By the end of the follow-up period, the gap in job quality had narrowed, but had not completely closed. TAA participants still had lower average hourly wages than comparisons in their most recent jobs in the fourth year of follow-up, but the gap had narrowed from -$1.50 to -$0.80 per hour. TAA participants also increased their access to employer-provided benefits, although they continued to lag behind comparison workers. With respect to job quality, trainees fared better than TRA-only participants, although they still earned significantly lower average hourly wages than their matched comparisons in the final follow-up year.

We also found that, among TAA occupational trainees who were working in the final year of follow-up, 37 percent were employed in the occupations for which they had trained, but the likelihood that a trainee was employed in his or her training field varied by the occupational focus of the training program. For example, approximately one third of trainees who enrolled in programs for office and administrative support, healthcare support, or installation, maintenance and repair found employment in their training fields. In contrast, more than 50 percent of trainees in the other three most common programs—healthcare practitioners, production, or transportation and material moving—were likely to be employed in those fields.

Although these results need to be interpreted cautiously due to potential sample selection issues, we found that, relative to the full population of trainees, program effects on job quality were more favorable for TAA participants who enrolled in occupational training and became employed in their training fields. For occupational trainees working in their respective fields, there was no significant difference in the average hourly wages between the trainees and their matched comparisons, nor was there a difference in access to fringe benefits.

## J. IMPACTS ON OTHER OUTCOMES

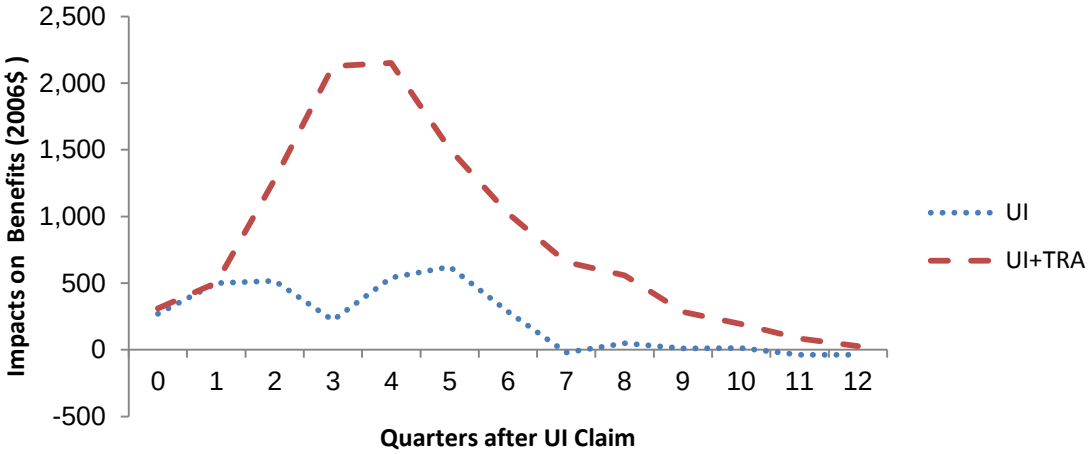
Job loss has the potential to affect many aspects of workers' lives. Family members of displaced workers may begin new jobs or work longer hours in order to offset the reduction in household income. Workers may also be eligible for various public benefits or pensions, while health insurance coverage, frequently obtained through an employer, may become difficult to maintain after job loss.

With regard to sources of income other than the sample members' own earnings, we found that participants collected more in UI payments, and 83 percent of participants exhausted their benefits, compared with 45 percent of comparisons. Moreover, as Exhibit 11 shows, participants collected more in UI and in combined UI and TRA payments than comparisons. The inclusion of TRA benefits nearly doubled the total unemployment payments that participants received, to almost $20,000 over 12 quarters, or nearly $11,000 more than the total UI benefits that were collected by comparisons (who, of course, were not eligible for TRA).

However, even with these increased UI/TRA amounts, TAA had a negative impact on participants' average total household income, suggesting that TRA payments did not fully compensate for the lower earnings that participants experienced during the study period when many were completing training. In both 2007 and 2009, TAA reduced total household income by about $5,000 (expressed in 2006 dollars).

We also looked at family structure and housing, and found no significant impacts of TAA on these outcomes. Although it is likely that both TAA participants and comparisons underwent substantial life changes associated with their job losses, neither participants nor comparisons experienced changes in their marital status, household sizes, or rates of home ownership in comparison to the pre-layoff period.

**Exhibit 11: Impacts on UI and Combined UI/TRA Benefits Received**




*Source*: Administrative data.

Moreover, TAA had no impact on improvements or declines in self-reported health status, and two thirds of both participants and comparisons reported being in good or excellent health at the time of their follow-up interviews. However, TAA seemed to increase the fraction of individuals reporting work-limiting conditions in the follow-up interview by three percentage points (16 percent for participants, compared to 13 percent for comparisons), and, among those with this type of condition, participants were more likely than comparisons to report having had the condition for less than two years.

TAA also had a negative impact on health insurance coverage. Despite the availability of HCTC benefits for TAA participants, only 14 percent took advantage of this benefit, and this usage could not compensate for the loss of coverage through employment for the sample of participants as a whole, and especially for trainees. This negative impact on health insurance coverage decreased somewhat later in the study period.

## K. CONCLUSIONS

The TAA program is designed to ensure that trade-affected workers are provided with quick access to One-Stop core services to hasten their return to work, and, when necessary for securing suitable reemployment, with potentially lengthy education and training services. TAA appears to be having significant and strong effects in providing these services to its customers. TAA participants in our sample were much more likely to access reemployment services than their comparisons, including services with the objective of helping workers find new jobs quickly, as well as those focused on longer term career planning. Moreover, participants were significantly more likely to access education and training, spending about 8 times as many weeks in these activities as the average comparison group member, and obtaining educational credentials more than twice as often.

Given the substantial duration of their program participation, we would expect TAA to delay participants' onset of reemployment, and indeed that was the case. During the first two years after they lost their jobs—during what was essentially a period in program services for many of them—TAA participants were significantly less likely to be employed than comparisons and they earned substantially less. The hypothesized rebound after services ended did not fully materialize, however. As their participation in training and other TAA services drew to a close, participants began to catch up with their comparisons, but, even four years after job loss, they had not yet closed the gap. These broad patterns of results are remarkably consistent with those reported by Corson and his colleagues in their study of the TAA program nearly two decades ago (Corson et al. 1993; Decker and Corson 1995).

Beyond these broad stroke findings, impacts for subpopulations of TAA workers suggest important lessons for program improvement. First, there is strong evidence that the program is ineffective for older workers; employment and earnings impacts were large and negative throughout the follow-up period for this group, and did not materially decrease over time. Furthermore, we found that TAA significantly increased older workers' retirement rates. This and our impact results on ATAA offer suggestive evidence that ATAA would seem to be an appropriate strategy for improving the short- and longer-term earnings of older participants. However, this program component is little used. Qualitative findings from the broader evaluation suggest reasons why: eligibility guidelines are restrictive (D'Amico et al. 2010) and this program component has not been strongly promoted (D'Amico et al. 2011). Thus, consideration might be given to strategies that

would broaden program access and promote take up among older workers. An important focus for future research should be to provide a clearer understanding of ATAA's effects.

Second, there is little evidence that the TAA program has positive economic benefits for those who receive TRA in the absence of training. The TRA-only subgroup fared relatively poorly in contrast to their comparisons, and impacts on their employment and earnings in the fourth year were less favorable than for TAA trainees. Furthermore, the economic gains they realized through receipt of TRA did not wholly offset the income that they lost through their lower earnings. Thus, the provision in the Trade Act of 2002 that TRA in the absence of training should only be allowed under limited waiver conditions seems appropriate (TAAEA reduced waiver conditions still further).

Third, and by contrast, earnings effects were more encouraging for trainees, and, although our conclusions on this must be tentative, they seem especially promising for those who found jobs in occupations for which they received training. In light of these findings, ETA's continued efforts to promote training in occupations that are in high demand, coupled with program staff members' diligent efforts to place training completers in jobs that match their skills, should both be further emphasized. Prior research (e.g., Leigh 2000) has found scant evidence that retraining in general is effective for dislocated workers, but some prior studies (e.g., Jacobson et al. 2002) and our own findings provide tantalizing clues that training can be effective if the training is carefully chosen and linked to fields in high demand. Obtaining more rigorous evidence of the link between occupational training and post-program earnings impacts is an important area for future research that could lend further credence to these suggestions.

Although these strategies taken together might improve program performance, it is worth speculating as to why the positive economic returns to the services TAA participants received are not already more fully in evidence. We suggest several reasons. First, about one third of TAA participants received income support without undertaking training of any type. As we have just noted, impacts were particularly unfavorable for study participants who fell into this group, and there are compelling reasons why this finding might have been expected. Most obviously, it has been shown that the availability of UI can delay the return to employment among the unemployed (see, for example, Katz and Meyer 1990; Card and Levine 2000; Feldstein 2005; Card et al 2007; and Elsby et al. 2010), and the same effect can be expected to hold true for TRA.[2] In fact, under the Trade Act of 2002 workers can receive waivers from the training requirement if their retirement is expected, which explicitly acknowledges that some in the TRA-only group are not expected to return to work (about nine percent of those in the TRA-only subgroup were listed as having received waivers from the training requirement due to their being within two years of retirement). Furthermore, case management and job placement assistance targeted to TAA participants not enrolled in training seem weak (Mack 2009). It appears, therefore, that the attraction of TRA

[2] Although a number of studies (including the ones cited here) report a significant effect of UI on the duration of unemployment, the magnitude of the effect is not clear. Decker (1997) reviews the literature and concludes that a one week increase in the potential duration of UI extends unemployment by between 0.1 and 0.5 weeks.

benefits increased the duration of unemployment for many TRA-only workers, without an associated increase in their job search activities and eventual ability to obtain better quality jobs.

Even so, impacts on employment and earnings were only somewhat more favorable for the service subgroup that received training—by the fourth year of follow-up there were no positive impacts for them on average weeks worked, and impacts on earnings, although small, remained negative and statistically significant. The earnings of trainees were trending in a direction that indicates they might overtake their comparisons, and it is quite possible that we would have seen positive returns to TAA participation for the trainees if we had a longer follow-up period. But, by the fourth year, these positive impacts were not yet in evidence.

If there are to be eventual positive impacts, why are they taking so long to emerge? In studies of workforce investment programs it is quite common to see the treatment group record much lower earnings than comparisons during an "in-program" period, but their earnings then rebound and overtake the comparisons' earnings shortly after the period of program participation ends. For example, in the national evaluation of the Job Training Partnership Act (JTPA), JTPA participants realized positive returns to their program participation by the thirtieth month after random assignment (Bloom et al. 1994), and Job Corps was similarly found to generate positive impacts on earnings for participants by about the beginning of the third year after random assignment (Schochet et al. 2001). Why, then, is it taking so long for TAA trainees to overtake their comparisons?

One possible reason is that the onset of the outcome measurement periods in the JTPA and Job Corps evaluations was the point at which program applicants were requesting services, whereas, in our study, it was at the point of job loss, which was often many months before TAA participants began program services. In the Job Corps study, for example, participation rates for the treatment group were at their peak in the first quarter after random assignment (i.e., the beginning of their outcome measurement period). By contrast, we found that peak participation for TAA participants was not reached until the third quarter of their outcome measurement period. This finding reminds us that, after job loss, it takes TAA participants time to be notified about their eligibility for program services, attend orientation sessions to have services explained to them, and make decisions about whether to participate. Moreover, as we have noted, about 27 percent of participants were not yet eligible for TAA at the time of their trigger claims, which is when we began recording their outcomes.

Further, the duration of participation was typically much longer for our sample of TAA participants than it was for the JTPA and Job Corps participant samples. In the Job Corps study, the average duration of participation was eight months. For TAA trainees in our study, by contrast, the duration of participation was almost two years (including their time in training and receiving post-training services). Both the slower onset and longer duration of services, then, can help explain why it has taken longer for TAA participants to catch up to their comparisons than it did for JTPA or Job Corps participants.

Also relevant is a large body of literature that suggests that job loss can have lasting effects on workers' earnings (see, for example, Wachter 2011). Although medium-term estimates of the cost of job loss range somewhat across studies, the literature suggests that workers who lose their jobs typically earn about 20 percent less than similar workers who do not lose their jobs. We found this to be the case for our comparison group, who earned about 88 percent of their pre-UI hourly wage in their most recent jobs in the final follow-up year. Because TAA training substantially increases

the time workers spend out of the labor force, it might not be surprising that the effects of job displacement take somewhat longer to resolve for TAA participants than for the typical displaced worker. Further compounding their problem of readjustment, TAA participants were significantly more likely than their comparisons to have switched industries and occupations, and in particular, were less likely to have been employed in the production industries from which they came. Thus, trainees may have been more likely than their comparisons to have started new careers, and therefore we might expect it to take a while for them to begin to show evidence of career advancement.

Finally, TAA trainees completed their training and re-entered the labor market when the nation's economy was mired in its worst economic recession since the Great Depression. About one half of TAA trainees completed their training—and, hence, presumably began their job searches—after the onset of the Great Recession. On the other hand, because they spent less time in training, comparisons were more likely to have returned to the labor market before economic conditions deteriorated. Trainees may begin to see positive returns to their training investment when the labor market begins to rebound and they can make better use of the new job skills they have acquired. Only additional data covering a longer follow-up period will tell us for sure.

As a final note, the impacts presented in this report do not address the possible benefits of the TAA program in making free trade politically feasible. Historical evidence suggests that free trade agreements are enacted on the condition that the most affected workers are provided access to enhanced benefits and services that give them a transition period to recover from their job losses. We will address this important issue in a companion report that presents a benefit-cost analysis, where we present estimates of the value of free trade and discuss assumptions about the extent to which the TAA program makes free trade politically feasible.

# I. INTRODUCTION

The Trade Adjustment Assistance (TAA) program is the linchpin of Federal efforts to help America's manufacturing workers rebound from job separation experienced as a consequence of foreign competition. The program's goal is to help trade-affected workers obtain reemployment at a suitable wage by providing training, temporary income support, and other services. Historically, TAA has also played a compensatory role, by assisting workers harmed by the relaxation of trade barriers. In 2010, the program served 199,238 participants.[3]

In 2004, the U.S. Department of Labor's Employment and Training Administration (ETA) funded Social Policy Research Associates (SPR) and its subcontractor, Mathematica Policy Research (Mathematica), to conduct a comprehensive study—the *Evaluation of the TAA Program*—that included (a) a quasi-experimental impact evaluation, (b) a cost-benefit study, and (c) an implementation study. This report represents the culmination of the evaluation's nearly eight-year effort to estimate the impacts of the TAA program on participants' employment, earnings, and other outcomes. In particular, this report addresses four central research questions:

- How effective is TAA in improving access to reemployment services and education and training services and in leading to the attainment of educational credentials?
- How effective is TAA in boosting participants' employment and earnings and in improving their access to jobs that offer better pay and fringe benefits?
- How effective is the program in promoting better health and access to health care coverage, and in reducing receipt of other forms of government assistance?
- How do the TAA program's impacts differ among participants with different demographic characteristics and among those who access different types of TAA services?

Additionally, to establish the context for the interpretation of the impact findings, the report presents the TAA participation rate among those eligible and describes the services that TAA participants access while in the program.

To provide the necessary background for the subsequent chapters, the remainder of this chapter describes the TAA program in more detail, summarizes the overall design of the evaluation, and reviews the recent literature on the effectiveness of job training programs for dislocated workers.

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[3] U.S. Department of Labor, Employment and Training Administration, *Workforce System Results: December 31, 2010 (http://www.doleta.gov/Performance)*

## A. RECENT HISTORY OF TAA

Although beneficial to the economy as a whole, the expansion of international trade exposes some U.S. firms to a level of increased foreign competition that can harm them financially and cause them to lay off significant numbers of their workers (Kletzer 2002). U.S. government policy recognized such potential for localized harm and incorporated escape-clause provisions into U.S. trade laws in the 1940s. These provisions included the institution of trade barriers if trade-related injuries to U.S. producers could be clearly demonstrated. This approach protected U.S. firms and workers, but it meant forgoing some of the potential economy-wide gains that could result from trade liberalization.

TAA represents an alternative strategy. Rather than blocking or reversing trade liberalization, TAA compensates workers and firms that have suffered trade-related injuries, providing services that help them adjust to changes in market circumstances. TAA's first antecedent was the Trade Expansion Act of 1962, which offered financial payments and other adjustment services to affected workers. However, strict eligibility requirements kept take-up rates low. In subsequent years, ensuing legislation and amendments, including the Trade Act of 1974, expanded eligibility guidelines (though with eligibility still restricted to those affected by trade in goods-producing industries) and changed the program's orientation from financial compensation to adjustment through training and reemployment services.

The Trade Adjustment Assistance Reform Act of 2002 (hereafter referred to by its short title, the Trade Act of 2002) represents another significant milestone in the evolution of the TAA program; described in more detail below, it constituted the programmatic environment when this evaluation project was launched. As the evaluation was nearing its end, the Trade and Globalization Adjustment Assistance Act (TGAAA), enacted in 2009 as part of the American Recovery and Reinvestment Act (ARRA), amended the TAA program yet again. However, TGAAA included a sunset provision and its amendments to TAA expired on February 12, 2011.[4] Further changes then were mandated under the Trade Adjustment Assistance Extension Act of 2011 (TAAEA), enacted on October 21, 2011. Among its key provisions, TAAEA expanded eligibility to trade-affected workers in services, required that case management services be made available to TAA participants, and significantly expanded certain program benefits.

However, participants whose impacts were assessed as part of this evaluation were served by TAA prior to the enactment of TGAAA and TAAEA, so the programmatic framework established by the Trade Act of 2002 is of the most direct relevance to this report. Therefore, the key provisions of this legislation are discussed below.

[4] TGAAA was originally set to sunset on December 31, 2010. On December 29, 2010, Congress enacted the Omnibus Trade Act of 2010, extending the sunset date until February 12, 2011.

## B. KEY PROVISIONS OF THE TRADE ACT OF 2002

The Trade Act of 2002, and ETA's accompanying implementation guidance, promotes three key principles regarding how the TAA program should operate:

- *Increase the focus on early intervention, upfront assessment, and reemployment services.* Recognizing that TAA had often been thought of as a training and income support program, ETA's operating instructions for the Trade Act of 2002[5] note that program operators should not lose sight of the importance of fostering rapid re-employment for adversely affected workers, so long as the goal of obtaining suitable employment is not sacrificed. In this context, providing trade-affected workers with timely access to upfront services might help identify their marketable skills and, with the provision of job search assistance, can assist them in obtaining suitable employment quickly, potentially obviating their need for retraining.

- *Use One-Stop Career Centers as a focal point of participant intake.* In keeping with the fact that the Workforce Investment Act (WIA) identified the TAA program as a required One-Stop system partner, the Trade Act of 2002 promotes collaboration of TAA with its partners in the One-Stop delivery systems by designating One-Stop Career Centers as the main points of TAA participant intake. Furthermore, the focus on the Career Center system is designed to promote the coordination and efficient delivery of services.

- *Maintain fiscal integrity and promote performance accountability.* ETA's operating instructions include a statement of the importance of maintaining fiscal integrity and note that program operators should be mindful of achieving strong participant outcomes.

With these tenets as the backdrop, the next sections review the process by which eligibility for TAA is established and describe the program's benefits and services.

### 1. Eligibility and Worker Notification

To be eligible for TAA benefits and services, a worker must be covered by a petition certified for TAA. Petitions are filed by an eligible entity (employers, unions, One-Stop operators or partners, among others) with ETA. Once it receives a petition, ETA has 40 days to make a determination. The petition is certified for TAA if ETA determines that the displacement occurred (or is expected to occur):

- Because of import competition of "like or directly competitive articles," or

[5] Training and Employment Guidance Letter (TEGL) 11-02, issued October 2002.

- Due to a shift in production to a foreign country so long as the country has a trade agreement with the U.S.

The Trade Act of 2002 also expanded eligibility to secondarily-affected workers; that is, those who are "upstream suppliers" or "downstream finishers" of products produced by a firm that has itself been certified for TAA (the primary firm), so long as the supplier or finisher firm experiences a loss of business that was importantly caused by the loss of business from the primary firm.

The date on which a petition is certified for TAA is the *determination date*. Recognizing that layoffs may have occurred before the petition was certified, or may occur in the future (i.e., after certification), TAA allows workers to be covered by the certified petition if they have experienced full or partial separations within a date range defined as beginning with the *impact date*, which is usually one year before the date the petition was filed, and ending with the *termination date*, which is usually two years after the determination date. This range represents slightly more than a three-year period of participant eligibility.

Once a petition is certified, states, operating as ETA's agents, are required to notify affected workers of their potential eligibility for TAA. To do so, a state elicits from the affected employer a list of all workers who have suffered (or will suffer) full or partial separation due to the cause listed in the petition, along with the workers' contact information. Within this report, we refer to these as *certified worker lists*.

States provide notification by sending a letter to each affected worker, letting the worker know of the TAA program's enrollment deadlines. Notification is also provided during Rapid Response meetings and at TAA orientation sessions, conducted at either the work site or a One-Stop Career Center.

## 2. Program Administration and Early Intervention Services

In most states, TAA is a state-administered program run by the state Employment Service (ES)/Unemployment Insurance (UI) agency. ES staff in field offices conduct intake, take applications for the program, and forward the paperwork to the State TAA Coordinator or other state program administrators for approval. A few states, by contrast, have devolved substantial authority for TAA to local workforce investment areas (LWIAs), and in some of these states LWIA personnel can approve training plans.

Regardless of the administrative arrangements used, One-Stop Career Centers are the focal points of participant intake and service delivery, and TAA is a mandatory One-Stop system partner. Thus, customers generally access program services through the One-Stop Career Center system. Moreover, workers covered by a petition that has been filed (whether or not a determination has yet been made on it) must be provided access to Rapid Response assistance and One-Stop core services, making it imperative that TAA operate in conjunction with its One-Stop partners.

Much of the funding for TAA training is provided by formula to the states, but ETA holds a substantial amount in reserve at the national level. States can request drawdowns from this reserve after they have used significant amounts of their formula allocation. This two-stage procedure is a way of recognizing that states benefit from having a base amount of TAA funds upfront to facilitate

planning, but that the timing and location of trade-related dislocations cannot be predicted by formula with great accuracy, due to their episodic nature.

## 3. Training

Occupational skills training can be paid for by TAA for up to two years, and can include classroom training, on-the-job training (OJT), and other customized training with an employer. Remedial training can also be provided if it is required for the worker to successfully complete occupational skills training or to take full advantage of his or her existing occupational skills. Training should aim to provide suitable re-employment at an adequate replacement wage and, because TAA benefits are an entitlement, must be approved if all of the following conditions are met:

- The worker cannot find suitable employment otherwise;
- The worker would benefit from the training;
- There is a reasonable expectation of employment following the training;
- The training is available at reasonable cost; and
- The worker is qualified to undertake the training.

## 4. TRA and Waivers

Trade Readjustment Allowances (TRA) are weekly cash payments made to eligible workers in amounts equivalent to their UI benefits. TRA is paid once UI benefits are exhausted and is of two main types:

- **"Basic" TRA**, which can typically be provided for the 26 weeks following exhaustion of regular UI, so long as the worker meets certain qualifications:
  - The worker has had at least 26 weeks of employment with the affected firm at wages of $30 or more per week in the 52 weeks before the worker's separation;
  - The worker has exhausted UI;
  - The worker is in approved training, has completed training, or has a waiver from the training requirement (waivers are described below);
  - The worker is unemployed; and
  - The worker meets the UI work search test, unless the worker is in training.
- **"Additional" TRA**, which has a weekly payment amount identical to basic TRA and can be provided for an additional 52 weeks once basic TRA ends. To receive additional TRA, the worker must be in approved training (no waivers are allowed) and must have filed an application for training with the state within 210 days of either the issuance of

the certification covering the worker or the worker's most recent separation, whichever is later.

In addition to these types of TRA, workers can receive an additional 26 weeks of TRA benefits if they are undertaking remedial training during that time. Thus, workers can generally receive UI plus TRA for a total of 2.5 years (i.e., 26 weeks of regular UI, 26 weeks of basic TRA, 52 weeks of additional TRA, and up to an additional 26 weeks for remedial TRA while the worker is in remediation).

To be eligible for TRA, workers must enter training within 8 weeks after the petition is certified or within 16 weeks after the separation, whichever is later (known as the 8/16 rule), unless they receive a waiver from the training requirement before that deadline. Waivers can be granted for any of the following reasons:

- The worker is expected to be recalled;
- The worker is believed to have marketable skills;
- The worker is within two years of retirement;
- The worker has a health condition preventing participation in training;
- Suitable training is not available; or
- The first available enrollment date for the training the worker wants to undertake falls outside the 8/16 guidelines (but within 60 days from that cut-off date, unless there are extenuating circumstances).

According to ETA's guidance,[6] a person receiving a waiver is considered to be receiving TAA services and this qualifies the individual as a TAA participant. However, not all states have consistently followed this guidance (U.S. Government Accountability Office 2006).

## 5. The Wage Subsidy Program

In the interest of promoting rapid re-employment, and because training may not pay off for older workers before they retire, the Trade Act of 2002 established a wage supplement program, Alternative Trade Adjustment Assistance for Older Workers (ATAA). ATAA pays up to 50 percent of the difference between the wage in the worker's new job and the wage at separation. Benefits can be paid for up to two years, up to a maximum payment amount of $10,000.

To be eligible, the worker must:

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[6] See *Trade Act Participant Report (TAPR): General Reporting Instructions and Specifications* (Revised 2006).

- Be at least age 50;
- Obtain re-employment within 26 weeks of the separation;
- Be employed full-time;
- Earn less than $50,000 in the new job;
- Not be re-employed by the same employer as at separation; and
- Be covered by a petition that was separately certified for ATAA.

Workers who accept an ATAA payment forsake their right to receive TRA or undergo training.

## 6. The Health Coverage Tax Credit

The Health Coverage Tax Credit (HCTC) is another new benefit established with the Trade Act of 2002. The tax credit equals 65 percent of the cost of health coverage for the individual and qualified family members. TAA eligibles can obtain health coverage by continuing their former coverage (if available through the Consolidated Omnibus Budget Reconciliation Act of 1985, or COBRA), by contributing to a spouse's plan as long as the employer does not pay more than 50 percent of the premium, by buying coverage through a state qualified health plan (usually state high-risk pool plans), or by using individually purchased coverage that the worker had for 30 days or more prior to job separation.

To be eligible for HCTC, workers must be covered by a certified petition and be receiving TRA or be eligible to receive TRA if they would have exhausted UI. Determining whether a worker would be receiving TRA but for receipt of UI requires a decision about eligibility in the face of a counterfactual. To provide guidance, ETA issued TEGL 11-02 Change 1, which suggested that states issue waivers to workers still receiving UI so that their eligibility for TRA, and hence HCTC, could be established. In an earlier report, the evaluation team found that this guidance was leading some states to issue waivers on a widespread basis, greatly increasing their administrative burden.[7] Accordingly, ETA subsequently issued TEGL 11-02 Change 3, rescinding its earlier guidance and asserting that HCTC eligibility could be established even for workers not receiving TRA or on a waiver, so long as the 8/16 deadlines required for entry into training had not passed and other conditions were met.

[7] Once issued, waivers must be reviewed on a monthly basis, to ensure that the waiver conditions still apply. For a discussion of the problems associated with the widespread use of waivers, see D'Amico et al. 2009.

### 7. Other Services

TAA also provides a range of other services. Job search allowances reimburse workers for 90 percent of the costs associated with conducting a job search out of their local areas—up to a maximum of $1,250—if suitable local-area employment is not available. Similarly, if suitable local employment is not available, relocation allowances provide workers with 90 percent of reasonable expenses, plus a lump sum payment up to a maximum of $1,250, for the costs of relocating to take advantage of job opportunities in other areas. Supplemental assistance is available to cover travel and subsistence expenses for workers who need to live away from home to participate in training: travel reimbursement covers the lesser of the actual cost of transportation between a worker's home and the training site or a cost per mile at the Federal mileage rate; subsistence payments cover the lesser of an individual's actual per diem expenses or 50 percent of the Federal per diem for the training locale.

Case management services are also available, although on a restricted basis. TEGL 5-00 lays out ETA's vision for how TAA should be integrated into the broader One-Stop delivery system and how partner programs' services can be leveraged to provide comprehensive services to TAA participants. According to this guidance, helping customers develop a reemployment or training plan and providing them with follow-up services are allowable and appropriate uses of TAA administration funds.[8] However, assessment, career counseling, and staff-assisted job searches, to the extent they occur, should be funded through partner programs, including Wagner-Peyser or the WIA Dislocated Worker Program.

## C. OVERALL DESIGN OF THE EVALUATION

The *Evaluation of the TAA Program* was designed to address key research questions focused on how the TAA program operates, what its effects are on participants' outcomes, and whether the benefits of the program outweigh the program's costs. In this section we provide a brief overview of the components of the evaluation, the data sources used, and the evaluation's schedule of deliverables.

### 1. Components of the Evaluation

To address the questions described above, the evaluation includes a net impact analysis, a benefit-cost analysis, and an implementation study. The net impact analysis uses quasi-experimental methods to compare program outcomes for treatment groups of TAA participants and TAA-eligible

[8] Congress appropriates $220 million for TAA each year, which states are to use primarily for funding TAA participants' training. States receive an additional 15 percent of this amount, which they can use for TAA administrative expenses. Developing a training plan is classified as an administrative expense, and is therefore funded through the 15 percent allowance for administrative expenses that states are provided.

nonparticipants to comparison groups of those not eligible for TAA. Following best practices in the field (Heckman, LaLonde and Smith 1999), comparison group members were chosen to be like their treatment group counterparts in that they have comparable demographic characteristics and employment histories and are drawn from the same local labor markets. Furthermore, their outcomes are measured in the same way, using the same data sources used for those in the treatment groups. Samples of TAA eligibles were drawn for the treatment group from 26 states and are representative of the national population of TAA eligibles. Further details of the design of the impact analysis are presented in the next chapter.

The benefit-cost analysis is designed to measure how the benefits of the TAA program compare with the program's costs, with both benefits and costs measured in monetary terms. From a societal standpoint, potential benefits include increased output that may result from the increased employment of program participants and their reduced use of alternative employment and training services or other public assistance benefits. Additionally, there may be gains to society that result from the freer trade that the TAA program makes politically tenable. By contrast, the costs are associated with benefits and services provided to participants and the administrative costs of operating the program. Details of the benefit-cost analysis are presented in a separate report.

The implementation study was designed to document how the TAA program is being administered, how program services are being provided, and what challenges were encountered as states endeavored to implement provisions in the Trade Act of 2002 and, later, TGAAA. Data for the implementation study were collected through site visits to 34 states, carried out in waves from 2004 through 2011. Results from the implementation study are detailed in a series of separate reports and briefing papers.

Exhibit I-1 shows the 26 states included in the impact analysis. The implementation study was carried out in all but one of these states, as well as in the nine other states also shown in the exhibit.

## 2. Data Sources

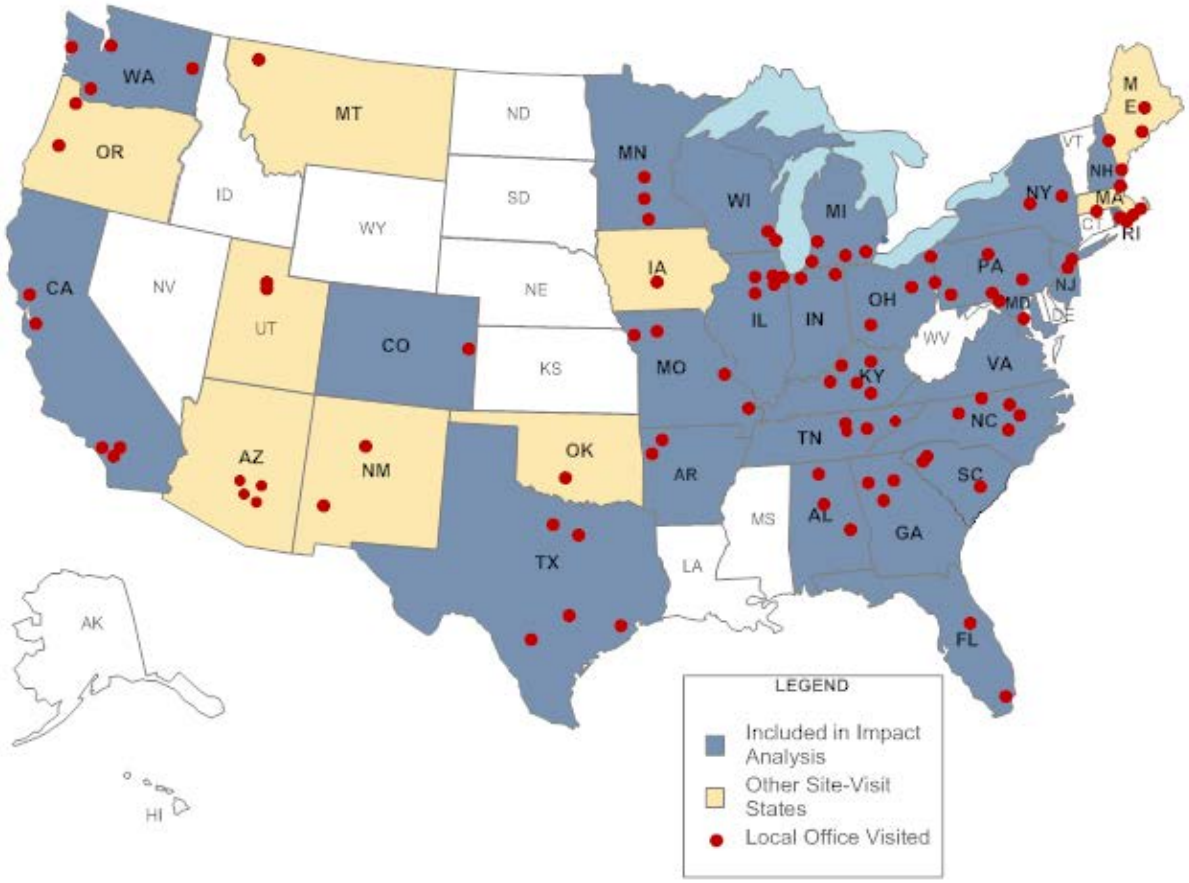
The analyses described above have been carried out using data of three primary types: administrative data, survey data, and qualitative data collected through site visits.

Administrative data were collected from 26 states and include files of various sorts:

- Lists of workers covered by certified petitions (the certified worker lists), used to define the sampling frame for the impact analysis;
- UI and TRA claimant data, used in conjunction with certified worker lists to define the sampling frame for the impact analysis and to measure receipt of benefits, either as TAA services (TRA payments) or as outcomes (UI payments following separation);
- UI wage data, used in the impact analysis to measure employment and earnings in the quarters before and after workers' job separations;
- TAA participation data, drawn from the Trade Act Participant Report (TAPR), the client-level records maintained by states on TAA participants' characteristics, services, and outcomes; and

- WIA participant data, drawn from the Workforce Investment Act Standardized Record Data (WIASRD), the client-level records maintained by states on WIA participants' characteristics, services, and outcomes.

**Exhibit I-1: States Included in the Evaluation's Data Collection**




Data were also drawn from telephone surveys administered to randomly selected samples of treatment group and comparison group members. Two surveys were administered: an initial survey was administered to TAA eligibles, including both TAA participants and nonparticipants, as well as to their comparison group counterparts, and a follow-up survey was administered to TAA participants and their comparison group counterparts. Further information on the initial and follow-up surveys, including sample sizes and response rates, is provided in Chapter II.

The final primary source of data used in the evaluation consists of qualitative information collected during multiple waves of site visits. The visits were conducted at state agencies charged with carrying out the administration of the TAA program, and, within each state, at several One-Stop Career Center offices where TAA services were being delivered. Each site visit lasted multiple

days and included interviews with TAA program administrators, fiscal staff, administrators of partner programs (primarily WIA), line staff, and program participants. Waves of site visits were conducted approximately annually for six years, beginning shortly after the effective date of the provisions of the Trade Act of 2002, so that the evaluation team could document the Act's initial implementation and the evolution of the TAA program up through early 2011 (which includes the period during which the program was guided by TGAAA).

### 3. Reports and Products

The evaluation team has produced a steady stream of reports and briefing papers over the life of the evaluation. Many of these products draw on qualitative site-visit data to describe the implementation of the Trade Act of 2002 and of TGAAA, and to portray various aspects of the operation of the TAA program in detail, including case management services, linkages with One-Stop system partners, and Rapid Response and early intervention services. Other products focus on results from the surveys, and have documented TAA participation rates (from among those eligible for TAA) and the reasons why workers participated in TAA or chose not to participate.

Other reports focus on impact findings. This report, which describes the impacts of participating in TAA, is in this category. A separate report presents impact results for TAA-eligible nonparticipants.

A complete list of the project's products is included in the appendix.

## D. RELEVANT LITERATURE ON DISLOCATED WORKERS

Dislocated workers are generally understood to be those with stable employment histories and strong attachments to their pre-layoff firms or industries who are separated with only a dim prospect of being recalled (e.g., Leigh 1990). Further, the jobs lost by these workers are often well-paying, particularly when their wage or salary levels are considered in relation to the workers' levels of education and their alternative local labor market opportunities (Dolfin and Berk 2010).

The literature on the consequences of dislocation presents a sobering picture of the future job prospects of workers who experience job displacements. Couch and Placzek (2007) reported that earnings losses in the quarter following separation exceeded 30 percent for those displaced through mass layoff. Moreover, losses remained substantial over the six-year period studied by these authors. Other evidence suggests that earnings losses are particularly large and sustained for older workers, those displaced from manufacturing, those who change industry upon reemployment (Couch et al. 2009), those who have high seniority at the time of job loss (Jacobson et al. 1993a), and those who are displaced during economic downturns (Jacobsen et al. 1993b). Ruhm (1991) goes so far as to suggest that dislocated workers can expect to suffer permanent "scars" to their future earnings potential, although their employment rates seem to eventually rebound.

Active labor market programs attempt to provide redress through targeted job training or other reemployment assistance, but the literature on their effectiveness is sparse. Of most direct relevance to this study is an earlier evaluation of TAA conducted by Corson and his colleagues at Mathematica (Corson et al. 1993; Decker and Corson 1995). They found that TRA recipients experienced longer spells of joblessness than did other UI exhaustees from manufacturing and were employed less and

earned less during the three-year period following their initial UI claims. TRA recipients who received training experienced lower employment rates and average earnings throughout most of the three years compared with non-trainee TRA recipients, though they appeared to fare better than the non-trainee group at the end of the three-year period.

The broader literature on the effectiveness of training programs for dislocated workers is relevant as well. Excellent summaries of this research (Leigh 1990 and 2009; Kodrycki 1997) suggest at best mixed evidence that training is effective for this population. In one of the most rigorous studies (it used experimental methods), Bloom studied the Houston site of the Texas Worker Adjustment Demonstration (WAD), which randomly assigned dislocated workers to be given access to either job search assistance alone or job search assistance with the offer of classroom training (Bloom 1990). The author concluded that classroom training had no incremental positive effect on employment and earnings relative to job search assistance alone. However, the follow-up period was short (just four quarters), and there apparently was a serious mismatch between the occupations that workers were interested in training for and the training slots that were available.

In the New Jersey UI Reemployment Demonstration, workers were randomly assigned to one of three groups: job search assistance only, job search assistance plus the offer of short-duration classroom training or on-the-job training, or job search plus a reemployment bonus. Observing outcomes over a six-year follow-up period, Corson and Haimson (1995) found that, by year six of the study, those who had received classroom training were earning on average $1,000 more than those who had received only job search assistance. However, the difference was not statistically significant.

A recent quasi-experimental study of the impacts of training for WIA dislocated workers also failed to offer solid evidence that providing training for dislocated workers had a positive effect on their long-term earnings (Heinrich et al. 2008). Using administrative data from 12 states covering about 160,000 WIA participants who entered the program from July 2003 to June 2005, the researchers estimated the effects of WIA training by comparing the outcomes of participants who received training to those who received only WIA core or intensive services, after making appropriate statistical adjustments. Researchers found that, for dislocated workers, training did not improve earnings and employment relative to core and intensive services. Dislocated workers in training experienced much lower earnings and employment in the first two years after program entry and then only modest positive gains in the third and fourth year relative to dislocated workers in core and intensive services. The gains did not appear to make up for the earlier losses and the researchers concluded that there was little evidence that training produced substantial benefits.

Other evidence is more encouraging. In a series of recent studies, Jacobson and colleagues (Jacobson et al. 2002, 2003, 2005) reported results from an analysis of Washington State's administrative data on the experiences of dislocated workers, some of whom received training through the state's community college system. Using an observation period of three years following job loss, they concluded that a year of community college credits raised displaced workers' earnings by an average of about 9 percent for men and 13 percent for women, but that the workers' training choices mattered greatly. In fact, all the earnings gains were attributed to workers who undertook training in health-related fields, in technically oriented vocational fields, and in academic math and science classes. Even older workers who took such classes were able to benefit. By contrast, returns to training for those who took less technically oriented classes were small or even negative. The

criticality of training choices is consistent with the results from the Houston WAD study, cited above.

Long-term follow-up results for dislocated workers from the Individual Training Account experiment (Perez-Johnson et al. 2011) appear less clear on occupational or content choices in training but suggest positive results for somewhat longer term training. In this eight-site study, WIA customers (including both adult and dislocated workers) deemed eligible for training were randomly assigned to one of three treatments providing different combinations of counseling and the potential amount of the training voucher. Eligibility for training, which varied across sites, generally required some form of occupational counseling or career exploration as part of WIA intensive services, and excluded all customers with low basic literacy. For dislocated workers, two models—"maximum choice" (which required no further guidance and had a cap on training costs of about $3,000) and "structured choice" (which provided a cap of up to $8,000 and more initial guidance)[9]—led to higher quarterly earnings than "guided choice" (which provided some counseling but also capped training costs at $3,000). Both maximum and structured choice led to at least $500 more per quarter in earnings in the final two years of follow-up (six years after services). Reasons for this are not clear, but individuals in the two groups with higher earnings averaged four additional weeks of training (i.e., 32 weeks), compared to the 28 weeks of participants in guided choice.

Based on this review, the evidence is mixed about the efficacy of training and other interventions for dislocated workers. However, while the literature provides important context for the current evaluation, there are some important differences between the TAA program under investigation and the programs and interventions whose impacts were reviewed above. First, the TAA program provides more generous training benefits than most other programs, which might allow for a wider range of training choices and providers. Second, the TRA income benefits provided by the TAA program might allow and encourage TAA trainees to select longer term programs, to remain in these programs for longer periods of time and to complete them at a higher rate. Third, the TAA program requires an often lengthy initial process of petitioning, certification, and notification, prior to participants accessing training, which is unlike most other programs. Fourth, because the Trade Act of 2002 encourages that TAA customers co-enroll in WIA, TAA participants may receive a wider array of reemployment services than were offered to participants in the previously-studied programs. Finally, the current evaluation uses a nationally representative sample, whereas previous studies have typically examined interventions in only a small number of purposively-selected sites, so that the impact findings may not be generalizable beyond the study samples.

[9] Precise cap amounts varied somewhat across the eight sites.

# II. STUDY DESIGN: SAMPLES, DATA, AND ANALYTIC METHODS

The TAA evaluation included the collection of survey and administrative wage records data on samples of eligible TAA workers and matched comparison groups to obtain unbiased estimates of the impact of TAA on participants' employment-related outcomes. The ideal design—random assignment—was not feasible for the evaluation, because TAA services cannot be denied to eligible workers under current program rules, making it impossible to construct a control group. Consequently, the evaluation employed a comparison group (propensity score matching) design to obtain an estimate of the impact of TAA on participants' employment-related outcomes. Comparison samples of dislocated workers in the manufacturing sector were selected to be as similar as possible to workers in the TAA samples at the time of job layoff. These comparison samples were matched to the treatment sample on key variables, and various analyses were used to assess what the outcomes of treatment group members would have been in the absence of the TAA program (that is, to define the counterfactual outcomes for the evaluation).

This chapter discusses the selection of the TAA samples for the evaluation (Section A), the selection of the matched comparison group samples (Section B), data sources (Section C), the primary outcome measures (Section D), the analysis samples (Section E), and analytic methods for estimating and interpreting program impacts (Section F). These topics are covered in much more detail in the companion report entitled "Methodological Notes on the Impact Analysis," which we hereafter refer to as the "MN report."

## A. SELECTION OF TAA WORKER SAMPLES

The evaluation samples of eligible TAA workers were selected in two stages. In the first stage, 25 states and one back-up state were randomly selected and recruited for the study. In the second stage, the following two samples of eligible TAA workers were selected from each of the 26 states:

1. ***The "TAA certified worker sample."*** This primary analysis sample includes the following workers: (1) those whose names appeared on certified worker lists obtained by states from employers as part of the mandatory worker notification process for petitions certified for TAA, and (2) those who also received a first UI payment from the state in which the firm named on the petition was located.
2. ***The "TRA-beneficiary sample."*** This supplementary sample consists of workers who received TRA payments after they exhausted their regular UI benefits. These workers had similar UI claim dates as the TAA certified worker sample.

An important evaluation design feature was the selection of these two TAA treatment samples. The two samples became eligible for TAA services at roughly the same time, thus allowing impact estimates for each sample to be compared. Specifically, this allowed us to examine the robustness and credibility of study findings under the quasi-experimental design, improving our ability to draw informative conclusions about program impacts.

The remainder of this section discusses the selection of these two nationally representative treatment group samples. Chapter I of the MN report provides additional design details.

## 1. Selection of States

To select the states for the evaluation, we obtained from ETA data on petitions that had been recently certified for TAA. These petition data provided a sample frame from which to select the states, because each petition contains information on the estimated number of trade-affected workers (that is, those who are likely to lose their jobs in the period covered by the certification). Using these petition data, 25 states within geographic strata were randomly selected with probabilities proportional to the expected number of TAA participants in the state, as defined using average state shares in Fiscal Years (FY) 2005 and 2006. The selected states, by ETA region, were as follows:

- *Region 1*: New Hampshire, New Jersey, New York, and Rhode Island
- *Region 2:* Pennsylvania and Virginia
- *Region 3:* Alabama, Florida, Georgia, Kentucky, North Carolina, South Carolina, and Tennessee
- *Region 4:* Texas, Arkansas, and Colorado
- *Region 5:* Illinois, Indiana, Michigan, Minnesota, Missouri, Ohio, and Wisconsin
- *Region 6:* California and Washington

All 25 states agreed to participate, along with one additional state—Maryland—that was recruited as a randomly-selected replacement state due to the initial reluctance of some states to participate in the study. Ultimately, all of the selected states participated; thus, the sample contains 26 states.

We estimate that the 26 selected states contain about 90 percent of all TAA-eligible workers nationwide in the study's certified worker sample frame.

## 2. Selection of the Certified Worker Sample

The primary treatment group sample for the impact analysis was obtained from lists of workers in worker groups covered by petitions certified for TAA (the "certified worker lists"), which states are required, by law, to obtain from the workers' employers. States obtain contact information on these workers and notify them in writing that they could be eligible for program services. The evidence suggests that these lists are reasonably complete (see Chapter I of the MN report).

We collected certified worker lists from each study state, and used them to select nationally representative samples of TAA participants (those who actually received TAA services) and TAA nonparticipants (those who were eligible for TAA but did not receive TAA services). The impact findings for the TAA participants are the primary focus of the evaluation and of this report.

Although not a focus of this report, the TAA nonparticipant sample was included for two main reasons. First, this sample was used to obtain new information on TAA participation rates among eligible workers and reasons for their participation or nonparticipation (see Dolfin and Berk 2010). Second, TAA-eligible workers might receive Rapid Response services, other WIA and ES early intervention services, and One-Stop Career Center core services that could obviate their need for TAA. Thus, TAA could have some effect on the outcomes of eligible nonparticipants; this issue will be explored in a separate study report.

To select the certified worker samples, we collected UI/TRA claims data from each of the 26 study states. These data contain demographic information on UI claimants and information on their TRA and UI benefit receipt. The claims data were merged with the certified worker lists for each state, and the certified worker sample frame was then defined to include the following workers:

- *Workers on the certified worker lists who were laid off from firms that became certified for TAA between November 1, 2005 and October 31, 2006.* Even though states furnished data at different times (see below), the petition certification period for the study was the same for all states. We specified a one-year window to account for potential seasonal layoff patterns.

- *Those whose UI benefit year started between September 1, 2004 and October 31, 2008.* Workers covered by a certification include those laid off between one year prior to the petition *filing* date and two years after the petition *certification* date, and it typically takes ETA one to two months to make certification determinations. Thus, this benefit period was selected to include workers covered by the above petition certification window.

The sample frame was further restricted to the following workers: (1) those between the ages of 16 and 80 at the time of the UI claim, (2) those who received regular UI benefits, and (3) those with nonmissing values for key data items, including telephone numbers and variables that the study required for matching. The sample frame includes about 55,000 TAA-eligible workers nationwide.

States provided the UI claims data at different times throughout 2008, depending on when they agreed to participate in the study and had staff available to provide the data. We requested UI claims data for all workers who received a first UI payment of any type from the first quarter of 2004 to the most recent quarter that UI records were available when the data were extracted. Thus, the UI data did not always cover the approximately three-year layoff window for each petition certified between November 1, 2005 and October 31, 2006. In general, however, coverage rates were high: for more than three quarters of the petitions, the period left uncovered was 12 months or less. Chapter I Section E.2 of the MN report discusses these data coverage issues in more detail, and Chapter VII of that report discusses the adjustment of sample weights to account for the uncovered periods.

The certified worker sample frame was divided into TAA participants and TAA nonparticipants. TAA participants were initially defined as those who received TRA benefits according to the first round of UI claims data provided by the states, and TAA nonparticipants were initially defined as those who had not received TRA benefits. Using these designations, we randomly selected the following certified worker samples for data collection:

- 2,875 participants and 1,506 nonparticipants for telephone interviews and administrative records collection (UI wage records, TAPR data, and WIASRD data)

- A larger sample of 7,546 participants and 12,452 nonparticipants for administrative records data collection only

We used systematic random sampling methods to select these samples from the full study universe: workers were ordered by gender, local labor market area, race/ethnicity, and age to ensure representative samples within key population strata (see Chapter I Section E.5 of the MN report).

The TAA participant and nonparticipant designations were subsequently updated using TAPR records, initial interview information on TAA service receipt, and updated TRA benefit information we later received. For these updates, TAA nonparticipants were reclassified as TAA participants if either of the following two conditions were met:

1. The person appeared in the TAPR data as having received a "high" level of service, through training, TRA payments, ATAA benefits, or TAA allowances (such as job search assistance, subsistence while in training, a travel allowance while in training, or a relocation allowance). This definition excluded those in the TAPR data who only received "light-touch" services such as receiving a waiver, a service plan, or case management services. For the survey samples, we also included workers who received health coverage through the Health Coverage Tax Credit (HCTC; information on HCTC is not available in the TAPR data).

2. The person subsequently received a TRA first payment (based on updated UI/TRA claims data that states provided) within the three-year window covered by the TAA petition that was associated with the worker's layoff.

Using these criteria, about 25 percent of nonparticipants were reclassified as participants for the analysis samples.

## 3. Selection of the TRA-Beneficiary Sample

To assess the robustness of the employment-related impact findings, we also selected a supplementary nationally representative sample from the universe of TRA beneficiaries. The primary advantage of this sample over the certified worker sample is that the UI/TRA records claims data contain information on all TRA beneficiaries in the selected states. By contrast, the certified worker sample may not be fully representative of even TRA recipients to the extent that the certified worker lists that states provided us are incomplete. The main disadvantages of the TRA-beneficiary sample are that it: (1) excluded TAA participants who did not receive TRA benefits but received other TAA services, and (2) could not be used to examine issues pertaining to program take-up rates.

The TRA-beneficiary sample frame includes those in the UI claims files who received a TRA first payment anytime between January 1, 2006 and December 31, 2006 (regardless of whether the worker appeared on one of the certified worker lists that the states provided us), and who satisfied other age and data requirements discussed above for the certified worker sample. The 2006

timeframe was selected to ensure that the certified worker and TRA-beneficiary samples were receiving TAA services at approximately the same time.

The TRA-beneficiary sample frame includes about 30,000 workers. For the analysis, we randomly selected 10,200 workers from this universe (see Chapter I Section F of the MN report). About 20 percent of those in the TRA-beneficiary sample are also in the certified worker participant sample. Administrative UI wage records, TAPR, and WIASRD data were collected for these workers, but *not* survey data due to project resource constraints.

## B. SELECTION OF MATCHED COMPARISON WORKER SAMPLES

Using the UI/TRA claims data, we selected separate comparison samples for: (1) participants in the certified worker survey sample, (2) participants in the certified worker administrative records sample, and (3) the TRA-beneficiary sample. We also selected separate comparison samples for two certified worker nonparticipant samples (one survey and one administrative data sample). Comparison samples were selected separately by state. Thus, in total, we selected 130 matched comparison samples for the five treatment groups across the 26 study states.

Chapter II of the MN report discusses the matching process and results in detail. Here, we highlight key features of this process.

### 1. Identifying the Pool of Potential Comparison Group Members

We identified the pool of potential comparison group members from the UI/TRA claims data as follows:

- We aligned the treatment and comparison samples in terms of their job layoff dates by limiting the comparison group for the certified worker samples to those who started collecting regular UI benefits between September 1, 2004 and October 31, 2008. Similarly, we limited the comparison group for the TRA-beneficiary sample to those who started a UI spell between mid 2005 and December 31, 2006 (which was the period when the TRA-beneficiary treatment sample started collecting UI benefits).

- Using UI/TRA claims data on the industries of the workers' primary employers, we limited the comparison sample to those in the manufacturing industry, by restricting the sample to workers with North American Industry Classification System (NAICS) two-digit industry codes of 31, 32, or 33.

- We dropped workers who received TRA benefits according to the UI/TRA claims data or who were on a certified worker list for a firm that was certified for TAA outside the data range for the study.

- We limited the potential comparison pool to workers who lived in the same local areas as the treatment group members, as defined using the local area indicators discussed below, and to those between the ages of 16 and 80 who received regular UI benefits and who had non-missing values for key variables.

Under our design, treatment and matched comparison groups both consist of *new* UI recipients. A disadvantage of this approach is that some in the treatment group started collecting UI benefits before their firm became certified for TAA. For instance, about 28 percent of TAA participants in the certified worker sample were separated from their jobs more than 90 days before their firm's petition was certified. Some of these participants may not yet have known about TAA at the time of their job loss. Furthermore, some may have ultimately participated in TAA because they could not quickly find jobs (although it is also possible that these workers' job search activities were influenced by the anticipation of being eligible for TAA services). Thus, these TAA participants may have been more likely than their matched comparisons to have unobserved characteristics that were associated with poor labor market outcomes, which could yield impact estimates that are biased downwards.

Despite this potential disadvantage, we believe that our design choice is preferable to the alternatives. One approach would have been to select workers who had exhausted their UI benefits. Such an approach, however, would not account for the potential effects of the offer of training, TRA, and other TAA services For instance, some TAA participants in our sample who exhausted their UI benefits and collected TRA might *not* have exhausted UI if TAA had not been an option. Instead, some of these workers might have more quickly found jobs. [10] Consequently, a comparison group restricted to UI exhaustees might have created a bias towards more favorable estimates for TAA, while a comparison group with both exhaustees and non-exhaustees is a more conservative approach, typical of much social science research. While our main impact estimates are based on the comparison group with both non-exhaustees and exhaustees, we also provide impact estimates using a comparison group of just exhaustees. While the "true" impacts cannot be known, it is plausible that they lie somewhere between the two sets of estimates.

Another option would have been to set "time 0" for the treatment sample to be the later of the petition certification date and the UI claim date. A problem with this approach, however, is that there is no comparable date for the comparison sample. Furthermore, there could be anticipatory behavior by participants as they await their firms' certification decisions. For example, workers laid off from firms who applied for TAA but who have not yet been certified by DOL could forgo job opportunities in the hopes of eventually receiving TAA services.

Consequently, the treatment and comparison group samples for the evaluation include new UI claimants, and the "time 0" or "trigger" date for matching is the UI claim date as a proxy for the job separation date. For the impact analysis, we conducted a host of sensitivity analyses (which are described in Chapter VII) to examine the robustness of the earnings impact findings in the face of

[10] Qualitative findings provided mixed evidence on the extent to which TAA facilitates exhaustion. On the one hand, TAA case managers we spoke with during site visits mentioned that many TAA eligibles were very interested in finding re-employment quickly and only agreed to undertake training when their job search proved unsuccessful. On the other hand, other eligibles were attracted to the prospect of gaining new skills that would increase their employability. Also relevant, survey results show that 65 percent of TAA participants reported having applied for TAA because they were interested in training or schooling, while fewer than 2 percent reported having applied because they "had no choice" (Dolfin and Berk, 2010).

potential selection biases. For example, we estimated earnings impacts using samples that excluded treatments whose UI claim dates were before their firms' petition certification dates (where their matched comparisons were also excluded). In addition, we estimated impacts using matched samples of exhaustees to provide an upper bound on the impact estimates.

Another important sensitivity analysis estimated impacts using TAA *nonparticipants* and their matched comparisons, where we might expect the direction of the selection biases to be opposite to that found between TAA participants and their comparisons. A finding of small or zero impacts for the TAA nonparticipants would provide evidence about the credibility of the impact findings for the TAA participants. In this case, the impact estimates for the participants would be the same as if we divided the estimated impacts using the pooled sample of TAA-eligible workers (participants and nonparticipants) by the TAA participation rate (that is, using the Bloom (1984) approach for obtaining the treatment-on-the-treated parameter from the intent-to-treat parameter in randomized designs). These sensitivity results are discussed in Chapter VII.

## 2. Data Items Used for Matching

The variables used in the matching process were constructed from the UI/TRA claims data, and included the following demographic and job characteristics:

- *Demographic information:* Gender, age, and race/ethnicity
- *Job characteristics:* Base-period earnings
- *UI claim and benefit data:* Benefit year begin date, date of UI first payment, and UI maximum benefit amount

In addition, we used zip codes from UI/TRA claims data to merge, by state, county, and year (if relevant), the following local area characteristics into the UI claims records:

- *The annual unemployment rate in 2000 to 2006* using data from the U.S. Bureau of Labor Statistics (BLS).
- *The poverty rate in 2004* using data from the Area Resource File (ARF).
- *The percentage of workers in manufacturing in 2005* using ARF data.
- *The average earnings per job in 2005* using data from the Inter-University Consortium for Political and Social Research (ICPSR).
- *The percentage population growth between July 1, 2000 and July 1, 2005* using ICPSR data.
- *The U.S. Department of Agriculture, Economic Research Service (ERS) 2003 Rural-Urban Continuum Code* using ICPSR data. These codes form a classification scheme that distinguishes metropolitan counties by the population sizes of their metropolitan areas, and nonmetropolitan counties by degree of urbanization and adjacency to a metropolitan area or areas. There are nine such codes that range from

metropolitan areas with a population of one million or more to rural areas that are not adjacent to any metropolitan area.

- *Local area unemployment statistics (LAUS) area type indicators in 2007* using BLS data. These indicators pertain to labor market areas that are economically integrated geographic areas within which individuals can reside and find employment within a reasonable distance or can readily change employment without changing their place of residence.

Although many of our matching characteristics were continuous variables, we constructed categorical variables to use in the matching models. The categorical variables were constructed for each state and sample after we examined the data and determined natural breakpoints. Note that the matching just described pertains to the *initial* matching; as discussed below, the final comparison group for the survey sample was selected after rematching based on a richer set of matching variables available from the initial survey.

## 3. Propensity Score Matching Methods

We used propensity score matching methods developed by Rosenbaum and Rubin (1983) to select the study comparison groups. For each model, we matched each treatment worker to the five comparison group members with the closest propensity scores. Matching was performed *with replacement* so that a comparison group member could be matched to multiple treatment group members.[11]

Our budget allowed us to complete interviews with two comparison workers for every treatment worker. Conducting initial interviews with a comparison sample that was twice as large as the treatment sample allowed for a second stage of matching that used the richer variables from the initial survey to identify a one-to-one match for the follow-up interviews. While we only planned to use two comparison workers, we identified the five nearest neighbors in case initial survey nonresponse generated a need for additional sample.

To assess each matching model specification, we conducted balancing tests on the categorical matching variables and the underlying continuous variables using methods found in the literature (see Chapter II of the MN report). For the balancing tests, we assessed the overlap in the distribution of propensity scores for treatment and matched comparison workers. We conducted *t*-

[11] In propensity score matching, a logit model is estimated where a binary dependent variable that equals 1 for a treatment group member and 0 for a potential comparison group member is regressed on independent variables measuring baseline demographic or other factors that are used to explain the likelihood or "propensity" that an individual is in the treatment group. The propensity score is the predicted probability from this logit model, and is calculated using the parameter estimates from the logit model and each individual's independent variable values. Matching is then conducted using the propensity scores by pairing treatment and comparison group members who have similar scores.

tests for each variable and *F*-tests of the differences in variances for continuous matching variables. We also conducted an *F*-test on the overall set of matching variables by running a regression of treatment status on all matching variables.

If the initial models failed the balancing tests, we used an iterative process to find the preferred model specification. Our re-estimation approach depended on the initial problem. In some cases, we estimated models separately for subgroups that were unbalanced (for example, age, race/ethnicity, a base wage rate category, or a particular local area characteristic). In other cases, we estimated models that included interaction terms for the problem variable as additional matching characteristics. We continued this process until we found a satisfactory model specification for each state sample.

## 4. Propensity Score Matching Results

Table II-1 displays key summary statistics on the matching variables for participants in the certified worker survey sample—the main sample for this report. The results are similar for the other samples (see MN Chapter II Section F).

The results suggest that the propensity score matching process identified matched comparisons from the full comparison group population whose distribution of baseline characteristics is similar to those of participants in the certified worker sample:

- None of the 26 *t*-tests comparing mean propensity score values across the treatment and comparison groups is statistically significant at the 5 percent level (see MN Chapter II Section F).

- None of the overall *F*-tests of variable similarity is statistically significant (Table II.1).

- Only a small percentage of *t*-tests comparing the demographic and local area characteristics of treatments and their first-best matched comparisons are statistically significant (Table II-1). Across the 26 models, the average percentage of *t*-tests that are significant for the demographic variables is 1.4 percent and the median percentage is zero. The average percentage of *t*-tests that are significant for the local area variables is 4.0 percent. However, this figure is larger in some states with TAA samples that lived in isolated (rural) areas, and where it was therefore sometimes difficult to find appropriate matches in those same areas.

- There are no significant treatment-comparison differences for any of the six base wage categories that were used in the matching models (Table II-1). This result is important because employment and earnings are the main evaluation outcomes.

As discussed in Chapter II Section F of the MN report, across the 130 models, about 13 percent of first-best matched comparison observations were matched to more than one treatment observation. Most of these repeat matches were in the top third of the treatment group propensity score distribution. The prevalence of repeated matches was somewhat uneven across states and samples.

## Table II-1: Summary Statistics for Comparing the Original Matching Variables for Treatments and First-Best Matched Comparisons for the Certified Worker Survey Sample, by State

| Sample and State | No. of Matching Variables | Percentage of *t*-tests Comparing Treatment-Comparison Means That Are Statistically Significant at the 5 Percent Level[a]: All Matching Variables | All Demographic Variables | Base Wage Indicator Variables | Local Area Variables | *p*-Value from *F*-test to Gauge Overall Treatment-Comparison Differences[b] |
|---|---|---|---|---|---|---|
| Alabama | 79 | 3.80 | 0.00 | 0.00 | 6.25 | 0.968 |
| Arkansas | 94 | 20.21 | 0.00 | 0.00 | 32.20 | 0.944 |
| California | 87 | 1.15 | 0.00 | 0.00 | 2.22 | 0.992 |
| Colorado | 62 | 0.00 | 0.00 | 0.00 | 0.00 | 0.997 |
| Florida | 58 | 0.00 | 0.00 | 0.00 | 0.00 | 0.939 |
| Georgia | 94 | 6.38 | 0.00 | 0.00 | 9.23 | 0.930 |
| Illinois | 95 | 2.11 | 0.00 | 0.00 | 3.39 | 0.998 |
| Indiana | 103 | 1.94 | 5.13 | 0.00 | 0.00 | 0.982 |
| Kentucky | 76 | 1.32 | 3.23 | 0.00 | 0.00 | 0.579 |
| Maryland | 66 | 1.52 | 3.03 | 0.00 | 0.00 | 0.997 |
| Michigan | 92 | 2.17 | 0.00 | 0.00 | 3.64 | 1.000 |
| Minnesota | 79 | 0.00 | 0.00 | 0.00 | 0.00 | 0.989 |
| Missouri | 83 | 3.61 | 0.00 | 0.00 | 5.36 | 0.966 |
| New Hampshire | 60 | 0.00 | 0.00 | 0.00 | 0.00 | 1.000 |
| New Jersey | 68 | 0.00 | 0.00 | 0.00 | 0.00 | 1.000 |
| New York | 91 | 7.69 | 0.00 | 0.00 | 15.56 | 0.990 |
| North Carolina | 115 | 0.87 | 2.56 | 0.00 | 0.00 | 0.998 |
| Ohio | 98 | 3.06 | 3.03 | 0.00 | 3.08 | 1.000 |
| Pennsylvania | 108 | 0.93 | 0.00 | 0.00 | 1.49 | 1.000 |
| Rhode Island | 51 | 0.00 | 0.00 | 0.00 | 0.00 | 1.000 |
| South Carolina | 79 | 1.27 | 3.45 | 0.00 | 0.00 | 0.975 |
| Tennessee | 103 | 5.83 | 5.56 | 0.00 | 5.97 | 0.839 |
| Texas | 73 | 5.48 | 8.11 | 0.00 | 2.78 | 0.989 |
| Virginia | 93 | 2.15 | 0.00 | 0.00 | 3.85 | 1.000 |
| Washington | 71 | 2.82 | 0.00 | 0.00 | 5.71 | 0.996 |
| Wisconsin | 80 | 2.50 | 2.86 | 0.00 | 2.22 | 0.977 |

Source: UI/TRA claims files and certified work lists provided by the 26 study states

Note. Figures are based on the first-best comparison group matches. The TAA participation and nonparticipation designations are based on initial designations using the TRA benefit receipt information in the UI/TRA claims data.

[a] The *t*-tests include the full set of potential matching variables and not just those used in the matching models.

[b] The *F*-tests include only the matching variables that were used in the matching models.

*Statistically significant at the 0.05 level, two-tailed test.

## C. DATA

Outcome data for the impact analysis were obtained from initial interviews, follow-up interviews, and administrative records (UI wage data, WIASRD data, and TAPR data). Initial and follow-up interviews were conducted by telephone for the certified worker survey sample and their matched comparisons, but not for the TRA-beneficiary sample or their comparisons. Administrative records data were collected for all samples.

This section summarizes key features of the data collection design. Chapters III to V of the MN report provide more details, including a comprehensive survey nonresponse analysis.

### 1. The Initial Interview

Initial interviewing took place by telephone between March 2008 and April 2009. Across the 26 study states, 13,256 individuals in the certified worker survey samples were released for initial interviews. These workers include 2,875 TAA participants and 5,760 of their matched comparisons, as well as 1,506 TAA nonparticipants and 3,115 of their matched comparisons. We conducted initial interviews with a comparison sample that was twice as large as the treatment sample to allow for a second stage of matching prior to the follow-up interviews, using the richer variables from the initial survey.

Telephone numbers and addresses in the UI claims data provided the main contact information for the survey. The Office of Management and Budget (OMB) initially approved the use of a $25 incentive fee to sample members for completing the survey, but later allowed the incentive to be increased to $50 for comparison group members and TAA nonparticipants to help increase survey response rates.

The survey questionnaire included a battery of questions about workers' experiences with the TAA program, their labor market and training experiences, and other key study outcomes that we hypothesized could be affected by TAA participation. The survey questions covered a period that started with the UI claim date associated with the trade-related job separation. The key categories of survey data items were as follows:

- Information about the job that led to the UI claim
- Information about prior jobs, earnings, and income
- Notification of TAA eligibility
- Knowledge of TAA services
- Application for TAA services
- The receipt of TRA, ATAA, and HCTC services
- The receipt of reemployment services
- The receipt of education and training services

- Information on jobs held since the UI claim date
- Other sources of income
- Household structure
- Health status and health insurance
- Demographic information

The (unweighted) response rate to the initial interview was 68.7 percent for TAA participants and 58.9 for their comparison group. Interviews were completed with 1,974 of 2,875 released TAA participants and 3,394 of 5,760 released matched comparisons. The average number of months between the UI claim date and the initial interview completion date was about 29 months for each research group.

As discussed in detail in Chapter III of the MN report, the survey respondents in the treatment and comparison groups are similar on characteristics in the UI claims data that were used for matching. However, there are some important treatment-comparison differences in baseline characteristics from the initial survey that were *not* used for matching, especially for the pre-UI job characteristics. For example, TAA participants were considerably more likely than their comparisons to be in a union, in larger companies, in production occupations, to have been in their jobs longer, and to have had health insurance and other fringe benefits made available. In addition, TAA participants were significantly less likely than their comparisons to report that they expected to be recalled to their job (35 percent, compared to 52 percent). Consequently, to create the final sample for the impact analysis for the survey sample, we rematched treatments and comparisons using the full set of matching variables that included key variables from the initial survey (see Section E below).

## 2. The Follow-Up Interview

Follow-up interviews were conducted with TAA participants in the certified worker survey sample and with their matched comparisons, but not with TAA nonparticipants or their matched comparisons. Follow-up interviewing took place by telephone between June 2010 and December 2010. Across the 26 study states, 3,000 treatments and 3,000 of their matched comparisons were released for follow-up interviews. Follow-up interviews were typically conducted about 23 months after the initial interviews. OMB approved the use of an incentive fee of $25 for treatments and $50 for comparisons for completing the survey.

The 3,000 TAA participants who were released for follow-up interviews consist of two groups. The first group includes all 2,228 participants who completed the initial survey (including 1,974 completers who were classified as participants at baseline, as well as 254 originally classified as nonparticipants at baseline who, based on their responses to the initial survey and TAPR data we received subsequently, were reclassified as participants). The second group includes a random sample of 772 initially-defined participants who did not complete the initial interview. We released these initial noncompleters to increase the overall survey response rate and to help account for survey nonresponse bias. Chapter IV of the MN report discusses our design for selecting the

comparison samples of 2,228 initial completers and 772 initial noncompleters for follow-up interviewing.

For those who did not complete the initial interview, the follow-up survey questionnaire was identical to the initial survey questionnaire, and the coverage period started with the UI claim date associated with the trade-related job separation. For those who completed the initial interview, the follow-up survey questionnaire was very similar to the initial questionnaire except that it excluded questions about the characteristics of the pre-UI job, background characteristics at the time of job loss, the receipt of Rapid Response services, notification of TAA eligibility, and knowledge of TAA services.

The (unweighted) response rate for the follow-up interview among those who completed the initial interview was 80.9 percent for treatments and 81.7 percent for comparisons. As expected, the response rate for those who did not complete the initial interview was considerably lower, but not trivial: 32.5 percent for treatments and 26.7 percent for comparisons (see Chapter IV of the MN report). Overall, interviews were completed with 1,803 of 2,228 treatments and 1,820 of 2,228 comparisons who completed initial interviews, and 251 of 772 treatments and 206 of 772 comparisons who did not.

The *effective* study survey response rate for TAA participants was 63.3 percent. This response rate reflects the percentage of TAA participants who completed follow-up interviews among the nationally representative sample of participants who were released for initial interviews. Using baseline data items from the UI claims data, we found some differences in the characteristics of follow-up survey respondents and nonrespondents (see Chapter IV of the MN report); thus, we adjusted the follow-up weights to help account for survey nonresponse bias (see Chapter VII of the MN report).

The average number of months between the UI claim date and the follow-up interview completion date was about 51 months for each research group, but varied somewhat across the sample. About 93 percent of treatments and 99 percent of comparisons in the sample had at least three years of follow-up data, and 64 percent of treatments and 69 percent of comparisons had at least four years of follow-up data.

## 3. Administrative Records Data Collection

After the certified worker lists and initial UI/TRA claimant files were obtained from the states and the analysis samples were drawn (as described above), we requested updated UI/TRA claimant data, UI wage records, and TAPR and WIASRD data from each of the study's states. These data were collected to: (1) measure UI/TRA benefit receipt for the several years after the trigger claim date, and (where appropriate) reclassify TAA nonparticipants as participants based on evidence of TRA benefit receipt, (2) measure the employment and earnings of treatment and comparison group members in the several years before and after their trigger claims, and (3) document the TAA and other workforce services that TAA participants received.

Nearly all of the 26 states complied with our request to supply these administrative data extracts. However, for various reasons, a few were unable to contribute data from one source or another; Table II-2 shows which of the 26 states contributed which types of data. Moreover, because Social Security Numbers were required data elements in the extracts that states provided (so

that data extracts from the various sources could be merged), no state contributed data without assurances from the evaluation team regarding the substantial data security precautions that would be taken with any data that the state provided.

## D. OUTCOME MEASURES

Three criteria guided specification of the major outcome measures for the impact analysis: (1) selecting outcomes that were likely to be influenced significantly by TAA participation, (2) selecting outcomes with policy relevance, and (3) measuring outcomes reliably. Next, we discuss the primary outcome measures, our hypotheses about how they were likely to be affected by TAA participation, and their construction.

Table II-3 displays the outcome measures used in the analysis.

**Table II-2: Administrative Data that Each State Contributed**

| | Certified Worker Lists | UI/TRA Claimant Data | UI Wages | TAPR Data | WIASRD Data |
|---|---|---|---|---|---|
| Alabama | X | X | X | -- | X |
| Arkansas | X | X | X | X | X |
| California | X | X | X | X | X |
| Colorado | X | X | X | X | X |
| Florida | X | X | X | X | X |
| Georgia | X | X | X | X | X |
| Illinois | X | X | X | X | X |
| Indiana | X | X | X | X | X |
| Kentucky | X | X | X | X | X |
| Maryland | X | X | X | X | X |
| Michigan | X | X | X | X | X |
| Minnesota | X | X | X | X | X |
| Missouri | X | X | X | X | X |
| North Carolina | X | X | X | X | -- |
| New Hampshire | X | X | X | X | X |
| New Jersey | X | X | X | X | X |
| New York | X | X | X | X | X |
| Ohio | X | X | X | X | X |
| Pennsylvania | X | X | X | X | X |
| Rhode Island | X | X | X | X | X |
| South Carolina | X | X | X | X | X |
| Tennessee | X | X | X | X | X |
| Texas | X | X | X | X | X |
| Virginia | X | X | X | X | X |
| Washington | X | -- | -- | X | X |
| Wisconsin | X | X | X | X | X |
| **N of States** | **26** | **25** | **25** | **25** | **25** |

## Table II-3: Key Service and Other Outcome Measures Defined Over Specific Periods, by Data Source

| Measure | Survey Data | UI Wage Records | UI Claims Data |
|---|---|---|---|
| **Reemployment Services** | | | |
| Received Any Reemployment Services | √ | | |
| Receipt of Seven Key Services | | | |
| Information on education or job training programs | √ | | |
| Job search assistance | √ | | |
| Labor market information about local in-demand occupations | √ | | |
| Information on how to change careers | √ | | |
| Help with resume | √ | | |
| Tests to see what jobs qualified/suited for | √ | | |
| Referrals to jobs or employers | √ | | |
| Receipt of Other Services | | | |
| Counseling on whether training is appropriate | √ | | |
| Counseling to select a training provider | √ | | |
| Number of meetings with counselor | √ | | |
| Receipt of Reemployment Financial Assistance | | | |
| Job search allowances | √ | | |
| Supplemental assistance | √ | | |
| Relocation allowance | √ | | |
| Location Where Reemployment Services Were Received | √ | | |
| Helpfulness of WIA-Related Reemployment Services in Finding Suitable Education or Employment | √ | | |
| **Education and Training** | | | |
| All Programs | | | |
| Ever enrolled | √ | | |
| Number attended | √ | | |
| Weeks attended | √ | | |
| Hours attended | √ | | |
| Specific Programs | | | |
| Ever enrolled in a remedial academic course | √ | | |
| Ever enrolled in a non-remedial academic course | √ | | |
| Ever enrolled in occupational training | √ | | |
| Hours attended academic programs | √ | | |
| Hours attended occupational training | √ | | |
| Occupational focus of training program | √ | | |
| Educational Attainment | | | |
| Completed an education or training program | √ | | |
| Received a credential or degree | √ | | |
| Highest grade completed at time of interview | √ | | |

## Table II-3 (continued)

| Measure | Survey Data | UI Wage Records | UI Claims Data |
|---|---|---|---|
| **Employment, Earnings, and Job Characteristics** | | | |
| Overall Employment | | | |
| Ever employed | √ | √ | |
| Number of jobs | √ | | |
| Weeks employed | √ | √ | |
| Hours worked | √ | | |
| Types of separations (voluntary and involuntary) | √ | | |
| Overall Productive Activity | | | |
| Ever employed or in training | √ | | |
| Weeks of productive activity | √ | | |
| First Post-UI Employment | | | |
| Number of weeks until became employed | √ | | |
| Recalled to employer | √ | | |
| Employed in same industry as the trigger job | √ | | |
| Earnings | | | |
| Quarterly and annual earnings | √ | √ | |
| Annual earnings as a percentage of the annual earnings in the year before trigger job loss | √ | √ | |
| Characteristics of the Most Recent Job | | | |
| Hourly wage | √ | | |
| Hourly wage as a percentage of the hourly wage of the trigger job | √ | | |
| Average hours worked per week | √ | | |
| Job benefits (health insurance, paid leave, retirement benefits) | √ | | |
| Occupation | √ | | |
| Whether occupation matches trigger job occupation | √ | | |
| Whether occupation matches training occupation | √ | | |
| Unionized | √ | | |
| Labor Force Participation at the Interview Date (employed, in training, unemployed, retired, not in the labor force) | √ | | |
| **Other Sources of Income** | | | |
| Unemployment Insurance Excluding and Including TRA Benefits | | | |
| Weeks received | | | √ |
| Amount received | | | √ |
| Pension Benefits | | | |
| Received benefits | √ | | |
| Months received | √ | | |
| Amount received | √ | | |
| Public Assistance | | | |
| Received benefits (SNAP/food stamps, cash assistance) | √ | | |
| Months received benefits, by type | √ | | |
| Amount received, by type | √ | | |
| Lived in public housing at time of interview | √ | | |

## Table II-3 (continued)

| Measure | Survey Data | UI Wage Records | UI Claims Data |
|---|---|---|---|
| Income of Spouse | | | |
| Whether spouse employed at time of interview | √ | | |
| Hours per week worked | √ | | |
| Monthly earnings | √ | | |
| Total Household Income in 2007 and 2009 | | | |
| Average and distribution of income | √ | | |
| **Health and Health Insurance Coverage** | | | |
| Health at Interview Date | | | |
| Health status (excellent, good, fair, or poor) | √ | | |
| Had a health condition that limited the amount of work that could be done | √ | | |
| Type of health condition | √ | | |
| Duration of health problem | √ | | |
| Health Insurance Coverage and Medical Expenditures | | | |
| Ever covered by health insurance | √ | | |
| Months covered | √ | | |
| Primary type of health insurance provider | √ | | |
| Out-of-pocket medical expenses in prior 12 months | √ | | |
| **Living Arrangements and Mobility** | | | |
| Living Arrangement at Time of Interview | | | |
| Marital status (married, living together unmarried, separated, divorced, widowed, or never married) | √ | | |
| Number of children under 18 who are financially dependent upon the worker | √ | | |
| Number of children or other dependents 18 or older who are supported by the worker | √ | | |
| Household size | √ | | |
| Housing status (rental, owner-occupied, or other arrangement) | √ | | |
| Mobility | | | |
| Lived, worked, or went to school or training in another state or country | √ | | |
| Distance moved from zip code at time of job loss and at the interview date | √ | | |

## 1. Primary Outcome Measures

The primary outcome measures for the impact analysis can be grouped into six areas:

**Reemployment Services.** The Trade Act of 2002 emphasizes that workers are to be provided Rapid Response assistance and access to One-Stop Career Center core and intensive services whenever a petition is filed, regardless of whether the petition is ultimately certified for TAA. Furthermore, ETA's operating instructions for the legislation, issued as TEGL 11-02, emphasize the importance of early intervention with the aim of promoting rapid reemployment. We would expect, therefore, that treatment group members (both TAA participants and nonparticipants) should have readier access to reemployment services than their comparison group counterparts, including all the core services commonly made available in One-Stop Career Centers—e.g., assistance with job searches, access to labor market information, help with preparing resumes, and job referrals. Furthermore, access to these services could lead to speedier reemployment than would otherwise occur.

**Education and Training.** A central role of the TAA program is to fund education and training to help trade-affected workers develop marketable skills that enable them to find jobs. The Trade Act of 2002 emphasized that long-term training, which has been the historical focus of the program, may not be the best route to suitable and rapid reemployment for all workers. Nevertheless, it continues to be the TAA service that most attracts eligible workers to participate in the program (Dolfin and Berk 2010). Given the TAA program's generous training benefit, we would expect that TAA participation should increase enrollment in education and training programs (as measured by increases in hours and weeks in occupational and skills training and general education). These increases in education and training could also lead to increases in educational attainment (as measured by the receipt of GEDs, vocational certificates, or college degrees).

**Employment, Earnings, and Job Characteristics.** The primary hypothesis is that, all else equal, TAA participants who receive TAA-provided reemployment and training services will become more productive in the long term and, hence, will have greater employment opportunities and higher earnings than those who do not. This increased productivity is expected to enhance employability (as measured by increases in labor force participation, employment, hours worked per week, and the proportion of weeks worked) and to increase wage rates, earnings, and fringe benefits available on the job. We might also expect TAA trainees to find jobs in occupations that match the type of training that they receive. The ATAA wage-supplement program may also induce older workers to become reemployed faster than they would have in the absence of TAA.

Importantly, we expect that TAA participation will reduce employment and earnings for TAA trainees during the period of training, because it is likely that some participants would have held jobs if TAA-funded training was not an option. However, as TAA trainees leave their training programs, we expect their employment and earnings to rise after a period of adjustment. In light of the variation in the duration of training programs, it is difficult to predict how long it will take for positive employment and earnings gains to emerge.

The short-term employment and earnings effects of TAA on program participants who do not receive training—TRA-only workers—are less clear. On the one hand, TAA-induced increases in the receipt of reemployment services offered in One-Stop Centers and elsewhere could also increase the reemployment rates of TRA-only workers soon after job loss. Furthermore, TRA benefits may

provide these workers more time to find suitable employment. However, the offer of TRA benefits could induce some of these participants to extend their unemployment spells and exhaust their UI benefits without increased job search effort, which could lead to earnings reductions.

**Other Sources of Income.** A set of hypotheses closely related to labor market activities involves the effects of TAA on other sources of income (the receipt of UI benefits, pension income, and public assistance; spouse's income; and total household income). We expect that TAA participation will lead to short-run increases in the receipt of UI payments for TAA trainees while they are in training. The offer of TRA benefits to the TRA-only participants could also increase the duration of their initial UI spells. However, as the full economic returns to TAA-funded training and other program services are experienced, we might expect that participants will receive fewer UI benefits, as well as fewer food stamp and cash assistance benefits.

It is possible that TAA trainees may use their pensions to help support them while they are in training, but we expect these effects to reverse during the post-training period. We anticipate that *older* TAA participants will receive lower pension incomes than their comparisons as they experience higher reemployment rates due to ATAA and other program services, resulting in reduced rates of retirement during the four-year follow-up period.

To help compensate for lost income during the period of training, spouses of TAA trainees may work more hours during the enrollment period, although the receipt of TRA benefits may help mitigate these effects. During the post-training period, however, it is possible that trainees' spouses will work less than their comparison group counterparts as the TAA trainees become more likely to find higher-paying jobs that match their skills. The expected direction of program impacts on spouses' labor market activities is unclear for the TRA-only participants.

**Health and Health Insurance Coverage.** TAA could potentially improve participants' overall health status to the extent that program participation increases health insurance coverage. This could occur in the short term due to the availability of the HCTC program, and in the long term due to positive program effects on the likelihood that participants will find jobs that offer health insurance. Psychological and financial benefits derived from the program could also improve participants' health status. The expected effects of TAA on out-of-pocket medical expenses are unclear. Increases in earnings and health insurance coverage could induce TAA participants to seek medical services that they would not seek in the absence of the program, thereby increasing out-of-pocket medical expenditures. However, the availability of health insurance could reduce expenditures for needed medical services that workers would seek irrespective of the TAA program.

**Living Arrangements and Mobility.** The effects of TAA on household income could translate into effects on participants' living arrangements (as measured by their housing status, household size, and marital status at the time of each interview). It is also possible that TAA could influence participants' mobility decisions due to TAA-funded relocation and job search allowances, increases in the use of One-Stop Career Center services to find appropriate jobs, and increases in the employability of trainees due to expanded job opportunities. Mobility is an important issue for the evaluation, because eligible TAA workers are more likely than other displaced workers in the U.S. to come from small areas with low population growth, and thus, may have limited employment opportunities in their home areas (Dolfin and Berk 2010). Thus, we examined impacts on whether participants lived, worked, or trained in other states or countries, and the distance between their zip codes of residence at the time of job loss and at the time of each interview.

## 2. Construction of Outcome Measures

Our analytic approach for the impact analysis focused on estimating period-specific impacts (that is, differences in mean outcomes between treatment and comparison group members within particular spans of time). We constructed period-specific outcome measures using information on the dates that events occurred.[12] For example, we constructed timelines to determine whether sample members were working or in training in each week between their trigger UI claim dates and their follow-up interview dates. We also constructed period-specific measures about the characteristics of each activity—for example, sample members' earnings, number of hours worked or in training, types of training programs in which sample members enrolled, degrees received, and public assistance benefit levels.

Employment- and training-related outcome measures were defined for the following periods after each individual's trigger UI claim: (1) each quarter, (2) each year, and (3) the entire four-year follow-up period. The quarterly measures were used to examine changes in impact estimates over time. We used the yearly measures to summarize activities during the "in-training" and "post-training" periods. As discussed in Chapter VI, in the first two years after the UI claim, many TAA participants were in training. Thus, the last two years during the follow-up period were largely a post-training period. We also constructed outcome measures that summarized sample members' experiences over the entire follow-up period to aggregate impacts during the in-training and post-training periods. These aggregate impacts are germane to the benefit-cost analysis, which will be presented in a separate report.

Some outcome measures pertain only to the time of the interview. For example, information on each respondent's highest grade completed, overall health status, address, and living arrangements were accurate at the time of the initial or follow-up interview.

Finally, all outcomes that are measured in dollars (for example, earnings, medical expenses, and cash assistance) are valued in 2006 dollars. We selected 2006 as the base year, because this was the period when most TAA participants in our sample were receiving TAA services, and, hence, incurring program costs.

# E. SAMPLES FOR THE IMPACT ANALYSIS

Table II-4 displays state sample sizes for the three analysis samples that are used for this report: (1) the certified worker follow-up survey sample, (2) the certified worker administrative records sample, and (3) the TRA-beneficiary sample. This section discusses the construction of each of these samples.

[12] Chapter VII of the MN report provides a detailed discussion of the construction of outcome measures, including the treatment of missing values and outliers.

## Table II-4. Impact Analysis Sample Sizes for Participants and Comparisons, by State

| State | Certified Worker Survey Sample | | Certified Worker Administrative Records Sample | | TRA-Beneficiary Sample | |
|---|---|---|---|---|---|---|
| | Participants | Comparisons | Participants | Comparisons | Participants | Comparisons |
| AL | 67 | 57 | 274 | 315 | 370 | 404 |
| AR | 61 | 57 | 325 | 443 | 345 | 545 |
| CA | 132 | 129 | 585 | 1059 | 516 | 958 |
| CO | 69 | 49 | 230 | 353 | 266 | 309 |
| FL | 49 | 46 | 144 | 180 | 146 | 199 |
| GA | 90 | 63 | 786 | 1088 | 478 | 712 |
| IL | 78 | 71 | 495 | 788 | 435 | 610 |
| IN | 70 | 54 | 597 | 827 | 419 | 659 |
| KY | 64 | 56 | 377 | 426 | 336 | 546 |
| MD | 61 | 49 | 198 | 294 | 203 | 246 |
| MI | 99 | 92 | 681 | 1176 | 584 | 1011 |
| MN | 67 | 65 | 94 | 151 | 247 | 423 |
| MO | 73 | 71 | 251 | 350 | 292 | 459 |
| NC | 189 | 144 | 1,058 | 1,735 | 999 | 1,590 |
| NH | 53 | 42 | 112 | 139 | 56 | 43 |
| NJ | 57 | 56 | 362 | 574 | 476 | 686 |
| NY | 62 | 66 | 334 | 588 | 366 | 619 |
| OH | 82 | 76 | 510 | 889 | 571 | 1,003 |
| PA | 99 | 88 | 497 | 877 | 610 | 1,072 |
| RI | 63 | 64 | 213 | 391 | 288 | 474 |
| SC | 98 | 71 | 505 | 672 | 498 | 671 |
| TN | 92 | 70 | 808 | 1,301 | 461 | 706 |
| TX | 71 | 65 | 330 | 509 | 373 | 572 |
| VA | 81 | 72 | 467 | 810 | 369 | 577 |
| WA | 53 | 59 | NA | NA | NA | NA |
| WI | 74 | 64 | 243 | 347 | 173 | 172 |
| Total | 2,054 | 1,796 | 10,476 | 16,282 | 9,877 | 15,266 |

Source: UI/TRA claims files and certified work lists provided by the 26 study states.

NA = Not available because Washington did not provide administrative records data.

## 1. The Certified Worker Follow-Up Survey Sample

The primary sample used for the impact analysis includes those in the certified worker sample who completed follow-up interviews—2,054 TAA participants and 1,796 comparisons. This sample excludes 230 (crossover) comparisons who completed follow-up interviews, but who were identified afterwards as having ever received any level of TAA service according to the TAPR or updated UI/TRA claims data.

As discussed, we found important treatment-comparison baseline differences for some survey items that were not used in the initial matching process. To account for these differences, we rematched the treatment and comparison groups in the follow-up survey sample using the full set of matching variables from the UI claims, local area, and initial survey data. We used a "kernel" matching algorithm where each TAA participant was compared to all comparison group members in the follow-up sample, regardless of the initially-matched triads. The algorithm assigned weights to each comparison group member based on the similarity of that worker's baseline characteristics to those of each TAA participant. Thus, a TAA participant could have many comparison group matches, each with a different weight. Chapter VI of the MN report describes the kernel matching algorithm in detail.

This approach generated balanced treatment and matched comparison group samples on all the matching variables (see Table II-5 for selected matching variables and Chapter VI of the MN report for the full set of results). None of the treatment-comparison differences is statistically significant for any of the matching variables. Furthermore, as shown in Table II-6, based on the UI wage records, there are no statistically significant differences between treatments and comparisons in their quarterly employment and earnings measures covering the eight quarters prior to job loss, even though these data were not collected in time to be used for matching. The kernel matching approach also generated an analysis sample that included all comparison group members in the follow-up sample (that is, all comparisons had a positive weight for the analysis).

Finally, the survey analysis examined period-specific impacts for employment, earnings, and training outcomes covering the four years (16 quarters) after the UI claim date. However, the length of the follow-up period differed somewhat across sample members. Accordingly, the analysis sample for impacts in years one to three impacts included the approximately 95 percent of sample members whose data covered this period, whereas the year-four samples included the approximately 65 percent of sample members with available data for this period. Accordingly, we constructed separate sets of follow-up weights for these two samples using the same kernel matching algorithm that was described above, and using separate sets of TAA participant weights that are described in Chapter VII of the MN report. All other analyses used the full follow-up survey sample.

## 2. The Administrative Records Samples

The certified worker and TRA-beneficiary administrative records samples were used for the impact analysis to: (1) examine service receipt for the treatment group (using TAPR, WIASRD, and UI/TRA claims data), and (2) estimate employment and earnings impacts using UI wage records. These analyses were conducted using the originally-matched treatment-comparison group triads.

The certified worker administrative records analysis sample includes 10,476 TAA participants and 16,282 originally-matched comparisons across the 25 study states (all but WA) that provided

administrative records data (see Table II-2). The TAA participants include 7,275 workers who were originally defined as participants based on initial TRA claims data and 3,201 originally-defined nonparticipants who were reclassified as participants after the TAPR and updated TRA claims data were collected. The sample excludes 1,727 comparison group crossovers who received TAA services and 214 TAA participants who no longer had any matches after the crossovers were excluded.

The TRA-beneficiary analysis sample includes 9,877 treatments and 15,266 originally-matched comparisons (see Table II-4). This sample excludes 1,539 comparison group crossovers and 218 treatments who no longer had any matches after these crossovers were removed.

Chapter VII of the MN report discusses the construction of weights for the administrative records samples.

## Table II-5. Selected Baseline Characteristics of TAA Participants and Comparisons in the Follow-Up Survey Sample

| Matching Variable (Percentages or Averages) | TAA Participants | Comparisons |
|---|---|---|
| **Characteristics from the UI Claims Data** | | |
| Benefit Year Start Date | | |
| Before 12/11/05 | 22.0 | 22.6 |
| 12/11/05 to 5/28/06 | 29.4 | 30.0 |
| 5/28/06 to 10/29/06 | 29.5 | 28.6 |
| Later than 10/29/06 | 19.1 | 18.7 |
| Average Total Base Period Earnings | $32,965 | $32,981 |
| **Local Area Characteristics** | | |
| Average Unemployment Rate in Year of Job Loss | 5.4 | 5.5 |
| Average Percentage of Workers in Manufacturing in 2005 | 14.0 | 14.2 |
| **Demographic Characteristics from the Survey Data** | | |
| Male | 48.1 | 47.6 |
| Race and Ethnicity | | |
| White, Non-Hispanic | 65.3 | 64.8 |
| Black, Non-Hispanic | 20.3 | 19.9 |
| Hispanic | 8.2 | 9.1 |
| Other | 6.2 | 6.1 |
| Age at Baseline Interview | | |
| 16 to 40 | 24.1 | 23.7 |
| 41 to 50 | 30.3 | 28.6 |
| 51 to 60 | 30.2 | 31.1 |
| 61 or over | 15.3 | 16.6 |
| (Average age) | 48.6 | 49.4 |
| Highest Education Completed | | |
| Less Than High School | 16.1 | 17.1 |
| High School Diploma or GED | 60.5 | 60.9 |
| Some College | 17.6 | 16.3 |
| Bachelors or More | 5.9 | 5.7 |
| Married | 59.5 | 58.9 |
| Has Children | 45.8 | 43.2 |

## Table II-5 (continued)

| Matching Variable (Percentages or Averages) | TAA Participants | Comparisons |
|---|---|---|
| Owns Home | 72.5 | 72.5 |
| Self-Reported Health Is Fair or Poor | 20.2 | 21.4 |
| Has Health Insurance | 90.9 | 90.4 |
| **Income Sources At Time of Job Loss from the Survey Data** | | |
| Average Number of Jobs In Three Years Before UI Claim | 1.3 | 1.3 |
| Average Total Earnings In Year Prior to UI Claim | $28,023 | $27,992 |
| Average Household Income | $42,722 | $41,866 |
| Received Food Stamps | 3.3 | 2.7 |
| Received Cash Assistance | 10.6 | 10.8 |
| **Characteristics of the Job Leading to the UI Claim from the Survey Data** | | |
| Reason for Job Loss | | |
| Laid Off Due to Plant Moving/Closing | 74.9 | 74.7 |
| Laid Off For Other Reason | 23.3 | 23.3 |
| Not Laid Off | 1.8 | 2.0 |
| Expected to Be Recalled | 10.6 | 10.7 |
| Belonged to Union | 30.7 | 30.6 |
| Received Severance Pay | 59.3 | 60.1 |
| Available Fringe Benefits on Job | | |
| Health Insurance | 95.3 | 95.4 |
| Paid Vacation | 95.5 | 94.4 |
| Paid Holidays | 97.7 | 97.5 |
| Paid Sick Leave | 55.2 | 53.9 |
| Retirement or Pension Benefits | 83.9 | 81.9 |
| Occupation | | |
| Manufacturing | 72.0 | 72.3 |
| Engineering, Business, or Management | 6.3 | 6.1 |
| Administrative Support | 8.6 | 8.7 |
| Other | 13.1 | 12.9 |
| Average Number of Employees in Company | 486 | 450 |
| Average Job Tenure (Years) | 13.4 | 13.4 |
| Average Hours Worked Per Week | 44.6 | 45.0 |
| Average Hourly Earnings | $14.77 | $14.87 |
| Sample Size | 2,054 | 1,796 |

Notes: Comparison group figures are based on the weights from the kernel matching and figures for both research groups are based on weights that account for the sample design and survey nonresponse.

* represents a significant difference from the TAA participant population with $p<0.05$ and ** represents a significant difference with $p<0.01$.

## Table II-6: Differences in Employment and Earnings Prior to Job Loss, Using UI Wage Records

| | TAA Participants | Comparison Group | Difference | Standard Error |
|---|---|---|---|---|
| Employed | | | | |
| Quarter 8 Before Job Loss | 92.7 | 92.5 | 0.2 | 1.0 |
| Quarter 7 Before Job Loss | 93.3 | 95.0 | -1.7* | 0.9 |
| Quarter 6 Before Job Loss | 94.4 | 94.0 | 0.4 | 0.9 |
| Quarter 5 Before Job Loss | 97.7 | 98.3 | -0.6 | 0.5 |
| Quarter 4 Before Job Loss | 98.5 | 99.3 | -0.8 | 0.5 |
| Quarter 3 Before Job Loss | 98.9 | 99.0 | 0.0 | 0.3 |
| Quarter 2 Before Job Loss | 99.1 | 99.3 | -0.1 | 0.3 |
| Quarter 1 Before Job Loss | 95.1 | 93.6 | 1.5 | 0.9 |
| Quarterly Earnings ($2006) | | | | |
| Quarter 8 Before Job Loss | 8,440 | 8,354 | 86 | 190 |
| Quarter 7 Before Job Loss | 8,402 | 8,333 | 69 | 153 |
| Quarter 6 Before Job Loss | 8,551 | 8,521 | 30 | 175 |
| Quarter 5 Before Job Loss | 8,741 | 8,883 | -141 | 240 |
| Quarter 4 Before Job Loss | 8,660 | 8,693 | -33 | 172 |
| Quarter 3 Before Job Loss | 8,934 | 9,227 | -294 | 330 |
| Quarter 2 Before Job Loss | 8,820 | 8,753 | 67 | 185 |
| Quarter 1 Before Job Loss | 8,344 | 8,508 | -163 | 294 |
| Sample Size | 1,940 | 1,685 | | |

Source: UI wage records for the 25 states that provided these data.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Difference is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## F. ANALYTIC METHODS

We estimated the average impacts of the receipt of TAA services by comparing the mean outcomes of treatment workers and their matched comparisons. The outcomes of the comparison group represent the counterfactual for the study—that is, the outcomes that the TAA participants would have experienced in the absence of the TAA program. Thus, to the extent that the matching process yielded treatment and comparison group samples with similar unmeasured characteristics at baseline (the time they were laid off from their pre-UI jobs), comparing the mean outcomes of the two groups provides an unbiased estimator of the effects of TAA services on participants' outcomes. This approach was used to estimate the impacts for the full sample, and also for important subgroups defined by baseline worker characteristics and specific program services received by TAA participants.

We estimated impacts using regression methods, where each study outcome was regressed on a treatment status indicator variable and a fixed set of baseline covariates. Baseline covariates were used in the analysis to improve the precision of the impact estimates, and to adjust for the small pre-existing observable differences between the treatment and comparison groups that remained after matching. All estimates were obtained using the sample weights discussed in Chapters VI and VII of the MN report, and the standard errors of all impact estimates were inflated to account for design effects due to unequal weighting and state-level clustering.

This section discusses these analytic procedures in more detail and our approach for presenting and interpreting the impact findings.

### 1. Regression Methods for Estimating Impacts for the Full Sample

The impact estimates for the full sample of TAA participants were obtained using variants of the following regression model:

$$(1) \quad y_{hsi} = \alpha + TAA_{hsi}\beta + \mathbf{X_{hsi}}\boldsymbol{\delta} + \varepsilon_{hsi},$$

where $y_{hsi}$ is the dependent variable for worker $i$ in state $s$ and region (strata) $h$, $TAA_{hsi}$ is an indicator variable equal to 1 for TAA participants (treatments) and 0 for comparison group members, $\mathbf{X_{hsi}}$ is a row vector of baseline explanatory variables, $\varepsilon_{hsi}$ is a mean zero disturbance term, and $\alpha$, $\beta$, and $\boldsymbol{\delta}$ are model parameters.

In this formulation, the estimate of $\beta$ is the regression-adjusted treatment effect, and the associated *t*-statistic can be used to gauge the statistical significance of the impact estimate. The $X_{hsi}$ covariates include the same variables that were used to match the treatment and comparison group members. These include variables from the UI claims data and local area characteristics (for all samples) and the baseline survey data items (for the follow-up survey samples).

All estimates were obtained using the commonly-used statistical package SUDAAN to accommodate the sample design. The exact SUDAAN routines used for the analysis depended on the nature of the outcome variable. We used ordinary least squares methods (Proc Regress) for continuous outcomes (such as quarterly earnings), logit regression methods (Proc Rlogist) for binary

(0/1) outcomes (such as quarterly employment status), and multinomial logit methods (Proc Multilog) for categorical outcomes (such as health or marital status categories). Chapter VII Section D of the MN report discusses our approach using SUDAAN to calculate standard errors of the impact estimates that account for design effects due to unequal weighting and clustering.

We present analysis findings using a series of figures, charts, and tables. The tables (which form the basis for the figures and charts) display the following pieces of information for each outcome measure:

- *The treatment group mean*, which was calculated using sample weights.
- *The regression-adjusted impact estimate for TAA participants*, which is the estimate of $\beta$ from the regression model in (1).
- *The derived comparison group mean*, which was calculated by subtracting the regression-adjusted impact estimate from the treatment group mean, and represents the mean outcome of the treatment group if TAA participation was not an option.[13]
- *The standard error of the impact estimate*, which accounts for design effects due to unequal weighting and state-level clustering.
- *The p-value from a two-tailed statistical test of the null hypothesis of no program impact*, where we indicate, using asterisks, whether the null hypothesis was rejected at the 1 percent, 5 percent, or 10 percent significance level. Our primary standard for identifying program effects is statistical significance at the 5 percent level.

For binary and categorical outcomes, we present impact findings for marginal probabilities (that is, differences in regression-adjusted *proportions* between treatment and comparison group members). The report does not present impact estimates measured as log-odds ratios (that is, the coefficient estimates on the treatment indicator variables) that are sometimes presented in impact reports, because we believe they are more difficult to interpret in our context.

For categorical variables, we conducted *F*-tests to assess the joint significance of the treatment effects across all levels of the categorical variable. In addition, we present *level-specific* impact findings for each categorical variable that were obtained using logit regression models. However, we only discuss these findings if the corresponding joint *F*-test for the categorical variable is statistically significant.

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[13] An alternative approach would be to present the regression-adjusted means for both the treatment and control groups. We rejected this approach, however, because the comparison group was not formally "sampled" from a broader population, but was matched to the treatment group. Hence, the population of interest is the one from which the treatment group was sampled. Thus, we present the raw treatment group mean and the derived comparison group mean using the estimated impacts from the regression model.

## 2. Subgroup Analysis

Program impact estimates for the full sample may conceal important differences in impacts across subgroups of program participants. If impacts do exist overall, they might be heavily concentrated in or much larger for some subgroups. Conversely, if impacts do not exist overall, they might exist for some subgroups. If a subgroup is small, the impact on it might not be large enough to yield a statistically significant difference in the overall sample.

This report addresses four important questions about impacts for subgroups:

1. Is TAA particularly effective for some groups of participants as defined by their personal characteristics or experiences at the time of job loss?

2. Are local economic conditions faced by TAA participants at baseline associated with program impacts?

3. Do impacts differ for TAA trainees and TRA-only participants?

4. Are TAA impacts associated with co-enrollment in the WIA program? What are impacts of the ATAA program for older workers?

Next, we discuss subgroup definitions and the analytic methods that we used to estimate subgroup impacts.

### a. Subgroups Defined by Demographic and Local Area Characteristics

It is important to identify groups of TAA participants who benefit from program participation, so that policymakers can improve program services and target them appropriately. For the subgroup analysis, we identified groups of TAA participants whose backgrounds, employment histories, training needs, and program experiences typically differ in important ways. In addition, we identified several subgroups defined by local area characteristics at the time of job loss, which could be associated with participants' employment and training opportunities, and thus, their subsequent labor market success.

Using survey and UI/TRA claims data, we estimated program impacts on eight sets of subgroups defined by participant demographic and local area characteristics at program entry (Table II-7 displays subgroup sample sizes for the certified worker survey sample):

1. **Age.** The broad age range TAA serves means that the program must serve younger adults as well as those near retirement. This poses a challenge for the program, because the training needs and employment prospects differ substantially for younger and older participants. For instance, nearly 30 percent of participants 60 or older in our sample did not have a high school credential at program entry, compared to less than 10 percent of those 16 to 29. At the same time, those 60 or older had much longer average job tenure on their prior job than those younger than 30 (21 years, compared to 4 years), and had slightly higher hourly wages ($13.90, compared to $12.66). Thus, an important policy objective is to examine how the impacts of TAA vary by age. Separate impact estimates are presented for those in the following age ranges: (1) 16 to 29 years

old, (2) 30 to 39 years old, (3) 40 to 49 years old, (4) 50 to 59 years old, and (5) 60 years and older.

2. **Gender.** Patterns of labor force participation differ by gender (see, for example, Polachek 2004 and Goldin 2004), suggesting that male and female TAA customers tend to have different employment histories and marketable skills. Relative to female participants, male participants earned more in their prior jobs ($16.80 per hour, compared to $13.00 per hour), worked more hours per week (46 versus 43), and were much more likely to be in unions (40 percent, compared to 21 percent) and to have worked in production occupations (77 percent, compared to 67 percent). Furthermore, male participants were more likely to have had some education beyond high school (29 percent versus 19 percent for females) and, as discussed in Chapter VI, had different TAA training experiences. Consequently, an important policy issue is to assess the extent to which program impacts vary by gender.

3. **Educational Attainment.** The TAA program serves workers with a broad range of educational levels, as was shown in Table II-5. Consistent with the returns to education literature (Card 2001), sample members' earnings in their prior jobs increased with their education level from $11.77 per hour for high school dropouts to $23.75 per hour for those with college degrees (although there were no differences in job tenure by education level). Furthermore, the most educated participants were more likely to have been married (83 percent, compared to 60 percent for those less educated), were less likely to be Hispanic or African American, and were overrepresented in large urban areas. In addition, as discussed in Chapter VI, the training rates and experiences of TAA participants in our sample differed by education level. These findings suggest that the pattern of program impacts could differ by education level.

4. **Race and Ethnicity.** The backgrounds of TAA participants differ markedly by race and ethnicity. About 40 percent of Hispanics in our sample did not have high school credentials at program entry, compared to 18 percent of African Americans, 12 percent of whites, and 19 percent of those in other race and ethnicity groups. Similarly, Hispanics tended to earn less on their prior jobs ($12.52 per hour, compared to $13.19 for African Americans and $15.44 for whites). African Americans were more likely to be female (61 percent, compared to about 50 percent for other groups) and were less likely to be married (47 percent, compared to about 65 percent for other groups). In addition, there were some differences by race and ethnicity in participants' local area characteristics. African Americans were largely concentrated in Region 3, whereas Hispanics were largely concentrated in Regions 1 and 4 and were more likely than others to have lived in large urban areas. Whites were more likely than other groups to come from areas with slightly lower poverty rates (12.8 percent, compared to 15.9 percent for blacks and 13.8 percent for Hispanics), but it is interesting that there were no differences by race and ethnicity in the local area unemployment rates. Four subgroups defined by race and ethnicity were used in the analysis: (1) white, non-Hispanic; (2) African American, non-Hispanic; (3) Hispanic; and (4) other (including American Indian, Alaskan Native, Asian, and Pacific Islander).

## Table II-7: Subgroup Sample Sizes and Population Shares for Participants in the Certified Worker Survey Sample

| Subgroup | Number of Participants | Population Shares of Participants (Percentages) |
|---|---|---|
| **Demographic and Local Area Subgroups** | | |
| Age | | |
| Younger Than 30 | 158 | 8.1 |
| 31 to 40 | 374 | 20.2 |
| 41 to 50 | 640 | 31.0 |
| 51 to 60 | 642 | 29.6 |
| Older Than 60 | 240 | 11.2 |
| (Missing) | 0 | |
| Gender | | |
| Male | 997 | 47.8 |
| Female | 1,057 | 52.2 |
| (Missing) | 0 | |
| Educational Attainment | | |
| No High School Credential | 334 | 16.1 |
| High School Credential | 1,192 | 60.3 |
| Some College | 365 | 17.8 |
| Bachelor's Degree or More | 138 | 5.8 |
| (Missing) | 25 | |
| Race and Ethnicity | | |
| White, Non-Hispanic | 1,365 | 63.9 |
| Black, Non-Hispanic | 334 | 22.5 |
| Hispanic | 213 | 7.8 |
| Other (American Indian, Alaskan Native, Asian, Pacific Islander, and Other) | 142 | 5.9 |
| (Missing) | 0 | |
| Family Composition | | |
| Married with Dependent Children | 588 | 28.8 |
| Unmarried with Dependent Children | 313 | 17.3 |
| Married Without Dependent Children | 674 | 30.9 |
| Unmarried Without Dependent Children | 476 | 23.0 |
| (Missing) | 3 | |
| Self-Reported Health Status | | |
| Fair or Poor | 405 | 20.2 |
| Good or Excellent | 1,639 | 79.8 |
| (Missing) | 10 | |

## Table II-7 (continued)

| Subgroup | Number of Participants | Population Shares of Participants (Percentages) |
|---|---|---|
| Type of Area | | |
| In a Metropolitan Area | 1,287 | 62.6 |
| In a Nonmetropolitan Area | 767 | 37.4 |
| (Missing) | 0 | |
| Local Unemployment Rate | | |
| Less than 4.4 Percent | 452 | 20.3 |
| 4.4 to 5.1 Percent | 506 | 24.8 |
| 5.1 to 6.0 Percent | 511 | 28.7 |
| Greater than 6.0 Percent | 585 | 26.2 |
| (Missing) | 0 | |
| **Program Service Receipt Subgroups** | | |
| TAA Training | | |
| Trainees | 1,235 | 64.8 |
| TRA-Only Participants | 624 | 35.2 |
| (Missing) | 0 | |
| WIA Co-enrollment Status | | |
| Co-enrolled | 1,027 | 41.7 |
| Not Co-enrolled | 1,027 | 58.3 |
| Sample Size | 1,940 | 100% |

Source: Initial and follow-up survey data and UI claims data from the 26 study states.

Note: Population shares were calculated using sample weights.

5. **Family Composition.** TAA participants' marital status and the presence or absence of dependent children in their households could influence their program needs and outcomes. The literature suggests that being married and having children has motivational effects that could lead to positive labor market outcomes, especially for men (Chun and Lee 2001, Korenman and Neumark 2001). This suggests that married TAA participants with children may be a group that particularly benefits from TAA training and other program services. On the other hand, opportunity costs of forgoing employment to enroll in a training program may be especially high for these participants, although the presence of an employed spouse could help mitigate these effects. Four family composition subgroups were used in the analysis: (1) those married with dependent children, (2) those unmarried with dependent children, (3) those married without dependent children, and (4) those unmarried without dependent children. The baseline characteristics across these groups differ in expected ways. For instance, those in our sample who were unmarried with dependent children were more likely than members of other groups to be female, to be African American, to have received food stamps, to be younger, and to have earned less on their trigger job. Conversely, married participants typically earned more than those who were unmarried, and married participants without children were typically 5 to 10 years older on average than other participants.

6. **Self-Reported Health Status.** About 20 percent of participants in the certified worker population were in fair or poor health at program entry (according to interview self reports). Compared to those healthier, these less healthy participants tended to be somewhat older, to have lower educational levels (23 percent were high school dropouts, compared to 14 percent of their healthier counterparts), to have higher rates of receipt of cash assistance and food stamps, and to have earned less in their most recent jobs. Consequently, health status at program entry could be associated with participants' labor market success, and hence, with their program impacts. Furthermore, health limitations are a reason that TAA participants can get a waiver from the training requirement, suggesting that TAA program experiences are likely to differ by health status. Thus, we examined subgroup impacts for: (1) those who reported being in poor or fair health at program entry, and (2) those who reported being in good or excellent health at program entry.

7. **Metropolitan or Nonmetropolitan Area of Residence.** The employment and training opportunities of TAA participants are likely to differ for those who come from metropolitan and nonmetropolitan areas. For example, participants laid off from plants in small "one-plant" towns may need to relocate to find jobs, and may need to travel some distance to find suitable training to improve their skills. These barriers could affect the TAA program services that these participants receive as well as their program outcomes. In support of these hypotheses, our data indicate that relative to metropolitan TAA areas, nonmetropolitan TAA areas tend to have much lower population growth, higher unemployment rates, and a larger share of workers in the manufacturing sector. Thus, as part of the subgroup analysis, we examined the extent to which program impacts differ for participants in metropolitan areas and in nonmetropolitan areas.

8. **Local Unemployment Rate.** The TAA program's challenges may be greater in areas with high unemployment rates than in areas where labor markets are stronger and employment opportunities are more available. It is unclear, however, whether program *impacts* will be larger in areas with stronger labor markets. This is because our treatment and matched comparison group members came from the same local areas, and thus faced the *same* local labor markets. Thus, it is possible, for example, that program services might not make a difference in areas with strong economies if individuals in both the treatment and comparison groups become reemployed quickly, whereas TAA services might make more of a difference when jobs are harder to find, by providing participants with effective reemployment and training services that increase their marketability. Four subgroups of similar size defined by the local unemployment rate were used in the analysis: (1) less than 4.4 percent, (2) 4.4 to 5.1 percent, (3) 5.1 to 6 percent, and (4) greater than 6 percent.

We also estimated program impacts for finer subgroups formed at the intersections of these categories. This analysis was conducted to help disentangle the subgroup findings, because many of the subgroups are correlated with each other. We do not report the details of these finer subgroup analyses, though we do highlight interesting findings in the text.

We view the subgroups defined by age as particularly important (along with the subgroups for trainees and nontrainees that are discussed below). Thus, in this report, we emphasize impact findings for these subgroups more heavily than for the other subgroups. However, the emphasis we place on various subgroups varies somewhat, depending on the outcome measure and our hypotheses about the extent and nature of program impacts.

### b. Subgroups Defined by Participants' Program Experiences

For the subgroup analysis, we also examined impacts for the following three subgroups defined by participants' program experiences:

1. Participants who received training (trainees) and those who received TRA but not training (the TRA-only group).

2. Participants who co-enrolled in WIA (according to the WIASRD data) and those who did not.

3. Participants who received ATAA services (according to the TAPR data) and participants at least 50 years old who did not receive ATAA services.

As discussed further below in Section IId, these program service subgroup findings, while policy relevant, must be viewed as less rigorous than the impact findings for the full sample and for the demographic and local area subgroups. These analyses must account for potential selection biases due to decisions made by both participants and TAA counselors regarding the types of program services that participants receive. Because these types of decisions are typically very difficult to model accurately, there could be unobservable differences between participants in a particular TAA service subgroup and their matched comparisons that could bias the program service subgroup findings.

**Trainees and TRA-only Participants.** Examining separate impacts for trainees and TRA-only participants is critical for the analysis, because training is a primary feature of the TAA program. In a previous report, we found that participants' interest in training greatly exceeded their interest in receiving TRA benefits (65 percent versus 26 percent), particularly among younger workers (Dolfin and Berk, 2010). Thus, the opportunity to re-train is the primary reason that most participants apply to the program. Furthermore, as discussed in Chapter VI, nearly two thirds of participants in our sample participated in training during the four-year follow-up period.

Given program rules exempting TAA participants from training, it is not surprising that the backgrounds of TRA-only participants differ from those of TAA trainees. The TRA-only sample tended to be older, to be less educated, to have been on public assistance, and to have earned less in their prior jobs.

Because of these differences, we expect the pattern of their impact findings to also differ. Specifically, we expect the employment-related impacts to emerge later for the trainees, and to perhaps be larger for the trainees after they completed their training and found jobs that matched their skills. To examine these hypotheses, we conducted a subgroup analysis where we defined trainees as those who received TAA-funded training according to the TAPR or initial interview data, and defined TRA-only participants as those who received TRA but not TAA-funded training.

**WIA Co-enrollment Status.** One of the major aims of the Trade Act of 2002 was to improve the speed and success with which trade-affected workers are able to return to the labor market. To accomplish these goals, the Act utilized several strategies, including a renewed focus on early intervention services, better upfront assessment and reemployment services, increased incentives and financial support for rapid reemployment, improved benefits and supports during training, and better connections to the labor market during training.

To facilitate these goals, the Act mandated closer collaboration between the TAA program and state and local One-Stop systems and partners, including WIA. Under the Act, One-Stop Career Centers were denoted as the main points of participant intake and delivery of benefits and services, and emphasized that trade-affected workers were to be eligible for and should have access to services provided by programs such as WIA and Wagner-Peyser.

Given these mandates, it is of policy interest to examine TAA impacts separately for participants who co-enrolled in WIA and those who did not. Interestingly, we find very few differences between the characteristics of participants in these two groups, except for differences in their regions and states of residence (Region 3 had a significantly higher share of co-enrollees than other regions). This is consistent with findings from the implementation analysis that found differences across state TAA programs in their ties with state and local One-Stop delivery systems. Thus, impacts by co-enrollment status are somewhat confounded by impacts by region and state, and must be interpreted carefully.

**ATAA Participation Status.** The ATAA program, which was funded as a demonstration program under the Trade Act of 2002, allows workers who are 50 years of age and older—workers for whom retraining may not be appropriate because of their nearness to retirement—to receive a wage supplement if they accept reemployment at a wage lower than their wage at separation. Take-up of ATAA has been quite low (about 5 percent of eligible participants) for the reasons discussed in Chapter IV. Thus, to obtain a sufficient sample for estimating subgroup impacts for ATAA

participants, we conducted this analysis using the certified worker administrative records sample only, but not the survey sample.

Finally, it is important to mention that we did not examine subgroup impacts for participants who participated in the HCTC program due to low program take-up rates, and because HCTC information is only available in the survey data, not in the TAPR data. Thus, survey sample sizes of HCTC participants were too small to support credible subgroup impact analyses for the HCTC program.

### c. Estimation of Subgroup Impacts

To estimate impacts for a subgroup defined by a participant's demographic or local area characteristic, we compared the distribution of outcomes of treatment and matched comparison group members in that subgroup. For example, impact estimates for males were obtained by comparing the outcomes of male treatment and comparison group members. To estimate impacts for a program experience subgroup (for example, TAA trainees), we compared the outcomes of treatment group members who had that program experience with their matched comparisons. Chapter VI of the MN report discusses how the comparison group weights were adjusted for the subgroup analyses.

We estimated impacts for each subgroup in turn using a straightforward modification to equation (1), where for simplicity of exposition, we assume a subgroup with two levels:

$$(2) \quad y_{hsi} = \alpha + TAA_{hsi}\beta + \mathbf{X_{hsi}}\boldsymbol{\delta} + (B_{hsi} * TAA_{hsi})\lambda + \varepsilon_{hsi}.$$

Equation (2) differs from Equation (1) due to the inclusion of the interaction term, $B_{hsi} * TAA_{hsi}$, where $B_{hsi}$ is a binary indicator variable for the subgroup of interest (for example, whether the participant is male). (Note that $B_{hsi}$ is assumed to be a component of $\mathbf{X_{hsi}}$.) The regression-adjusted impact for those with $B_{hsi} = 1$ (for example, males) is $(\beta + \lambda)$, and for those with $B_{hsi} = 0$ (for example, females) it is $\beta$. The parameter $\lambda$ represents the *difference* in the impacts across the two subgroup levels. Equation (2) can be generalized to subgroups with more than two levels (such as race/ethnicity) by including additional treatment-by-subgroup indicator variables.

We used a staged approach for identifying subgroup impacts. First, we conducted *F*-tests to gauge whether differences in impacts across subgroup levels were statistically significant. Second, if the *F*-tests were significant, we discuss the *t*-test results for each subgroup level. If the first-stage *F*-tests were not statistically significant, we do not discuss the *t*-test findings (although we present these results). We adopted this approach to help account for the likelihood of finding spurious significant impacts due to the multiple testing problem (see below), and because the impact findings across subgroup levels should be judged in relation to each other, rather than in isolation.

### d. Interpretation of the Subgroup Impacts

The subgroup findings must be interpreted carefully for several reasons. First, sample sizes for the survey sample (the primary analysis sample) were selected to yield precise impact estimates for the full sample, but not for subgroups (and especially not for small subgroups). Thus, the subgroup

analyses have relatively low power for detecting true program impacts that can realistically be found. Second, the propensity score matching was performed using the full sample of participants and not separately for each subgroup. Thus, there could be some imbalance between the treatment and comparison groups within subgroups, although we controlled for these differences in the regression models.

Importantly, the *program service* subgroup findings must be viewed as only suggestive of true program effects. The full sample impact estimates are unbiased under the assumption that the matching variables used in the propensity score matching models captured all observable and unobservable differences between the TAA participants and their matched comparisons that are correlated with key study outcomes. Thus, these matching models needed to account for two types of potential selection biases: (1) firms' decisions to apply for TAA and (2) the decisions of workers in TAA-certified firms to apply for and participate in the program. The program service subgroup analyses, however, needed to account for an *additional* layer of selection bias due to decisions made by both participants and TAA counselors regarding the types of program services that participants received. These types of decisions are very difficult to model accurately using the available baseline matching variables, suggesting that there could be unobservable differences between participants in a particular TAA service subgroup and their matched comparisons that could bias the subgroup findings. In recognition of these sample selection issues, we label the program service subgroup findings as "treatment-comparison differences" rather than "impacts."

Another important issue about the program service subgroup findings is that they pertain only to the population of participants who are typically assigned to those program service streams (because the results were obtained by comparing the outcomes for participants assigned to specific components of treatments with their matched comparisons). The results cannot necessarily be used to measure the effectiveness of a particular program service for the average TAA participant. Nor can the results necessarily be used to assess how a participant in one program component would fare in another one. As discussed, this occurs because there are differences in the characteristics of those assigned to specific components (for example, the training and TRA-only program components).

## 3. Sensitivity Analyses

An important feature of our analytic approach is to assess the sensitivity of key employment-related impact findings to alternative samples, earnings data sources, and matching methods. These sensitivity analyses are particularly important in comparison group designs where statistical modeling decisions and assumptions are typically required to estimate program impacts, and where there is not always a scientific basis for selecting among various methods. We conducted the following sensitivity analyses to estimate key employment and earnings impacts using variants of our benchmark approach:

- *Using UI wage records and the survey sample*. As discussed, survey- and UI-based earnings measures each have advantages and disadvantages. The survey data cover earnings from all formal and informal jobs, but could suffer from misreporting and survey nonresponse. The UI wage records data are available for all sample members and do not suffer from survey misreporting, but could suffer from employer misreporting. Further, UI wage records do not cover all workers (such as the self-employed, military personnel, and Federal workers) and earnings from informal employment or for sample

members who were employed in a different state than the state of the initial UI claim. Because both data sources provide credible earnings measures, we compared impact findings using both sets of measures.

- *Using the TRA-beneficiary and certified worker administrative records samples along with the UI wage records.* The TRA-beneficiary sample consists of workers who received TRA payments after they exhausted their regular UI benefits. This sample had similar UI claims dates as the TAA certified worker sample, but it excludes TAA participants who did not receive TRA. Both the TRA-beneficiary and certified worker administrative records samples are larger than the survey sample, and thus, provide more precise impact estimates. However, the comparison groups for these samples were matched using UI claims and local area data only, but not the initial survey data. Thus, the comparison group matches for these administrative records samples are of lower quality than for the survey sample.

- *Using alternative matching strategies for the survey sample.* As discussed, we used kernel matching methods to construct comparison group weights for the survey sample. To examine the robustness of this matching approach, we also estimated impacts where the comparison group matches were obtained using nearest neighbor matching methods, and where we altered key bandwidth parameters for the kernel matching process (see Chapter VI of the MN report).

In addition, we also conducted the following additional analyses to assess potential selection biases in the benchmark impact estimates:

- *Using UI exhaustees in the survey sample.* The analysis samples for the evaluation included new UI claimants, and "time 0" for matching was the UI claim date associated with the trigger job separation date. A disadvantage of this approach is that some members of the treatment sample started collecting UI benefits before their firms became certified for TAA, and some may have ultimately participated in TAA because they could not quickly find jobs; this sample selection problem could yield impact estimates that are somewhat biased downwards. Thus, to examine the robustness of the earnings impact findings to potential selection biases, we estimated impacts using matched samples of exhaustees in the survey sample. This sample provides an *upper bound* on the impact estimates, because about 80 percent of participants in the survey sample exhausted UI, compared to only about 50 percent of matched comparisons, suggesting that TAA has a large effect on exhaustion rates and that comparison group exhaustees were less "marketable" than the treatment group exhaustees. In addition, we estimated earnings impacts using the survey sample that excluded treatment individuals whose UI claim dates were before their firms' petition certification dates (where their matched comparisons were also excluded).

- *Including additional control variables in the regression models.* More comparisons than treatments were actually recalled to their trigger jobs (12.6 percent, compared to 6.9 percent). Actual recall status could be an outcome of the TAA program if, for instance, TAA-certified firms are less likely to recall their workers receiving generous TAA benefits. The matching models included *expected* recall status, but not the potentially endogenous actual recall status measure. However, as a sensitivity analysis,

we included actual recall status as a covariate in the regression models. In addition, we included detailed industry indicators to further address potential selection bias issues. These variables were not used in the initial matching process due to small cell sizes, but the two samples turned out to be balanced on these indicators. The inclusion of these covariates in the regression models did not alter the impact findings, and thus, are not reported. However, we report impact findings from analyses that excluded treatment and comparison workers who were recalled to their jobs

- *Using TAA nonparticipants and their matched comparisons.* Although TAA could have some effect on TAA-eligible nonparticipants, we expect these impacts to be small. Furthermore, we expect that the direction of the selection biases for the nonparticipants to be *opposite* that for the TAA participants (that is, the nonparticipants may have decided not to participate in the program because they found jobs quickly and, thus, may have been more "employable" than their matched comparisons). Thus, a finding of small or zero impacts for the TAA nonparticipants would provide strong evidence about the credibility of the impact findings for the TAA participants.

## 4. Criteria for Identifying Program Effects

The impact analysis generated impact estimates on a large number of outcome measures and for many subgroups. Thus, many hypothesis tests were conducted to gauge treatment-comparison differences. These myriad analyses could lead to the multiple testing problem where spurious significant impact findings are likely to be found by chance (see Schochet 2009). Suppose separate *t*-tests are conducted for each comparison to test the null hypothesis of no impacts, where the Type I error rate (statistical significance level) is set at $\alpha = 5$ percent for each test. This means that the chance of erroneously finding a statistically significant impact is 5 percent. However, if multiple tests are conducted, the chances of finding false positive results increase substantially. For example, the Type I error rate increases to 40 percent for 10 independent tests, to 64 percent for 20 independent tests, and to 92 percent for 50 independent tests. Thus, an analysis challenge is to interpret impact findings so that they provide information about the effectiveness of TAA services that will be useful to program staff and policymakers, while at the same time minimizing the chances of accepting spurious significant impact findings due to the multiple testing problem.

Our main approach to account for the multiple testing problem is to differentiate between *confirmatory* and *exploratory* impact analyses. Confirmatory analyses pertain to a small number of primary hypotheses related to program effects on key employment, earnings, and training outcomes for the full sample of participants, and provides a sharp focus for analysis and interpretation. These analyses address the key research questions that were specified in the study protocols, and focus on the impacts that the study was designed to detect. For the confirmatory analyses, we did not employ multiple comparisons corrections to adjust the Type I error rate downwards for hypothesis testing using the Bonferonni, Benjamani-Hochberg, or related methods, but examined the pattern of employment and earnings results with a particular focus on the year-four findings (the most recent time period). In the presentation of study findings, we always lead with the confirmatory impact findings.

The impact findings for other study outcomes with weaker a priori hypotheses about expected program impacts (such as health insurance coverage and public assistance receipt) and for population subgroups are deemed as exploratory. These analyses, while informative, do not have

the same level of statistical rigor as confirmatory analyses. They are used in the following ways: (1) to corroborate and provide color to the findings from the confirmatory analyses, (2) to identify new hypotheses about program effects, and (3) to identify potential areas for program improvement. Thus, we believe that these findings are likely to be useful to program stakeholders.

We also used other approaches to arrive at our overall conclusions. For instance, we examined the magnitude of the significant impact estimates to determine whether the differences are large enough to be policy-relevant, and examined the robustness of study findings using results from the sensitivity analyses. In addition, we examined whether the patterns of impacts are similar across related subgroups and outcomes, and whether they conform with implementation study findings.

## 5. Statistical Power

The TAA evaluation was designed to detect a true impact on quarterly earnings of $273 for the survey sample (and $137 for the larger administrative records sample), using a two-tailed test at 80 percent power and a 5 percent significance level. This minimum detectable impact (MDI) was adopted so that the study would provide a sufficient level of precision for detecting earnings impacts that would produce a positive net benefit of the TAA program from both the government's and society's perspective.

The actual MDI for quarterly earnings for the survey sample is about double the anticipated value. This occurred for two main reasons. First, the actual standard deviation of quarterly earnings is about $4,700, compared to the assumed value of $3,000. Second, the design effect due to weighting for the comparison group was nearly 6, which occurred due to the kernel rematching process that was conducted to balance the treatment and comparison groups on initial survey data items (such as expected recall status, union membership, and other pre-UI job characteristics) for which there were unexpected treatment-comparison differences (see Chapter VI of the MN report). This led to some comparison group members being repeatedly assigned large weights when matched to different treatment group members. For similar reasons, the actual MDI for the survey-based quarterly employment rate is about 6.5 percentage points, compared to the anticipated value of 4.5 percentage points. The overall design effect due to state-level clustering and unequal weighting for the estimation of quarterly earnings impacts is about 2.4.

# III. THE DECISION TO PARTICIPATE IN TAA

Once a petition for TAA is certified, states are obliged to notify covered workers of their eligibility for the program. They do so by first requesting lists of covered workers, along with the workers' contact information, from the affected employers. State officials then mail letters informing the workers of their potential eligibility for services and inviting them to attend orientation sessions at which TAA services are explained. Workers might also learn about their potential eligibility for TAA at Rapid Response events even before a petition is certified, or through announcements that State Workforce Agencies disseminate through various media outlets.

Not all workers who are eligible for TAA take up the offer of services. Worker advocates have cited a lack of aggressive outreach as one factor contributing to low take-up rates (Rosen 2006), but workers may also choose not to access TAA because they anticipate being recalled or are confident that they can find suitable reemployment without assistance.

As a way of establishing the context for the impact results that follow later in this report, this chapter presents the study's findings on TAA participation decisions. Because this report focuses on impacts for TAA participants (as opposed to the broader sample of TAA eligibles or those TAA eligibles who do not participate in TAA), we focus carefully on what constitutes TAA participation and discuss important ambiguities in this definition. We then use initial survey data to describe the reasons why eligibles choose to participate in TAA, as a way of understanding the types of assistance that workers seek from the program.

As will be described in this chapter, we define a TAA participant as an eligible worker who receives a *significant* TAA service, including TAA-supported training, TRA, ATAA, HCTC, job search or relocation allowances, or subsistence or travel allowances for those in training. This definition thereby excludes the considerable numbers of workers who receive only waivers, which can be likened to One-Stop Career Center core services.

Using this definition, about 50 percent of those eligible for TAA become TAA participants. This estimate is essentially the same regardless of whether it is calculated from administrative data that states provided or from initial survey data. However, the TAA participation rate differs markedly from state to state—in some states, no more than 30 percent of eligibles participate, while in other states more than 80 percent do. Participation also varies depending on workers' characteristics. For example, among eligibles, females are more likely to participate than males, African-Americans more than Hispanics, and older workers more than those who are younger. In comparison to participants of the WIA Dislocated Worker Program, TAA participants tend to be older, have lower levels of education, and have higher pre-participation quarterly earnings.

About 70 percent of persons who become TAA participants cite their interest in training or schooling as a reason for applying to the program. Other reasons, such as an interest in receiving TRA or HCTC, are much less commonly cited. Because workers' motivations for participating can be expected to relate to the benefits they anticipate upon program completion, these findings provide important clues as to where TAA's greatest impacts might be expected.

## A. PARTICIPATION IN TAA

The research design for this study enables us to define a nationally representative sample of TAA eligibles—that is, those who experienced job separation and were covered under a certified worker list. Starting from this base, we are able to calculate the percentage of eligibles who access TAA services.

### 1. Who Counts as a TAA Participant?

We received TAPR data from the participating states,[14] and intended to classify eligibles as TAA participants if they appeared in these files. A complication, though, is that not all states have applied a consistent definition of what it means to be a TAA participant for purposes of preparing their TAPRs. ETA's guidance has been consistent and clear that a participant record should be opened "for all individuals who receive services or benefits financially assisted by" the TAA program.[15] However, the U.S. Government Accountability Office (GAO) and our own investigations have determined that not all states rigorously follow ETA's guidance (U.S. GAO 2006). A particular problem, discussed more fully in Chapter V of the MN report, is that only about half of the states nationwide include in their TAPRs data on individuals who receive waivers but no other TAA services, since they have not considered those who receive only waivers to be TAA participants.[16]

Exacerbating the problem caused by the TAPR's inconsistent coverage has been the explosion in the use of waivers in the wake of the enactment of the Trade Act of 2002, at least in some states. This dramatic increase is the result of two primary factors. First, states became more apt to issue waivers to protect workers' eligibility for TRA in the face of the Trade Act's 8/16 deadlines for receipt of training. As noted in Chapter I, to be eligible for TRA, a worker must enter training by the latter of 8 weeks after the petition is certified or 16 weeks after the separation date, unless he or she receives a waiver for the training requirement before that deadline. Some states' TAA administrators feel that the 8/16 deadlines are too aggressive, given the time it takes the state to obtain certified worker lists from employers, notify workers of their eligibility, schedule intake appointments, and help workers make prudent training choices (D'Amico et al. 2009). Consequently, they routinely issue waivers to give workers more time for training enrollment.

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[14] The TAPR is the client-level reporting system used in the TAA program. As discussed in the MN report, we requested that states send us TAPR data for anyone who participated in TAA between April 2004 (the earliest date that persons in the sample could have been eligible to participate in TAA given the petitions' impact dates) and June 2010.

[15] This language can be found in the instructions for the TAPR at least since 2005.

[16] In 2006, after this fact came to light, ETA issued explicit instructions for the TAPR that made clear that waiver receipt was to be considered a TAA service, and, hence, individuals who received a waiver should be included in the TAPR submissions. States' practices appear to have changed gradually after this guidance was released, but it is still unclear whether compliance is complete.

Second, ETA issued guidance that states could reasonably issue waivers to demonstrate workers' eligibility for HCTC. To be eligible for HCTC, trade-affected workers must be covered by certified petitions and either be receiving TRA or be deemed as eligible to receive TRA once they have exhausted UI. If a worker is still on UI, the state's determination that he or she would otherwise be TRA eligible must include its determining that the worker is expected to be in training by the 8/16 deadlines or will receive a waiver before then. To simplify the calculus for this decision, ETA suggested that it would generally be appropriate for states to issue marketable-skills waivers to workers who were still on UI and in advance of the 8/16 deadlines, even for those who had not yet decided to enroll in training. Doing so would provide tangible evidence of HCTC eligibility and, meanwhile, would give workers a chance to test the labor market before the need for training was definitively determined.[17] In the face of these considerations, some states began to issue waivers to everyone—or nearly everyone—eligible for TAA who attended Rapid Response events or other TAA orientation sessions, even to those who had not expressed an intention of seeking HCTC or any other TAA service.

Combined with the fact that waivers are inconsistently recorded in the TAPR, this increase in waiver use means that in some states 50 percent or more of all TAA participants included in the TAPRs are those who received only a waiver, while in other states there are none (see the MN report for details). This dramatic variation is clearly a problem for the evaluation. To begin with, using inclusion in the TAPR as evidence that someone is a TAA participant would mean that impact estimates would be weighted towards states that include waiver-only participants in their TAPRs, even though other states might be delivering equivalent services to similar numbers of people. Moreover, counting waiver-only recipients would mean that, at least in states that issued waivers on a widespread basis to protect HCTC eligibility, some persons—and perhaps many persons—would be counted as TAA participants who received effectively nothing beyond One-Stop core services.

Thus, for purposes of estimating program impacts in this report, we define TAA participation more narrowly, to be those who received a *significant* TAA service, including one or more of:

- TAA-funded training;
- TRA;
- ATAA;
- HCTC; or

[17] This guidance was issued as TEGL 11-02 Change 1, in 2003. The research team reported that, as an unintended consequence of ETA's guidance, the use of waivers had skyrocketed, causing states substantial administrative burden (see D'Amico et al. 2009). In response to this problem, ETA rescinded its earlier guidance in TEGL 11-02 Change 3, issued in 2006.

- any of the various allowances that the program makes available to cover special circumstances (that is, a job search allowance, a subsistence or travel allowance for those in training, or a relocation allowance).

Evidence of receipt of these services comes from two main sources: administrative data and survey data. Administrative data is itself of two types. Our primary source is the TAPR data that states provided, because this should cover everyone who was served in TAA during the eligibility period covered by the certified petitions that make up the sampling frame. Because states are asked to record in the TAPR anyone who receives any of the above listed services (with the exception of HCTC; see below), the TAPR should in principle enable us to accurately identify all TAA participants as the term is defined in this report.

We discovered, however, that some individuals who are shown as having received TRA in the UI/TRA claimant files we received are in fact not included in the states' TAPR submissions (see Chapter V of the MN report). Therefore, to supplement the TAPR as an administrative data source, we use evidence of TRA receipt from the UI/TRA claimant files. We add these additional TAA participants to those from the TAPRs.

Thus, using administrative data, persons are classified as TAA participants if they are listed as having received any of the following: (a) training, ATAA, or job search, travel, subsistence or relocation allowances, recorded in the TAPRs, or (b) TRA, recorded in either the TAPR or the UI/TRA claimant files.[18] Note that, based on this operational definition, HCTC receipt does not cause someone to be classified as a TAA participant if the individual did not also receive one of the services listed above. This is because evidence that an individual received the tax credit is not a reportable TAA activity; hence, is not recorded in the TAPR or in any other report that state workforce agencies need to submit.

Survey data provide another source for learning about TAA participation. The initial survey was administered to a representative sample of TAA eligibles (that is, those covered by certified petitions) to learn about the TAA services they received. Questions specifically focused on receipt of TRA, TAA-funded training, ATAA, HCTC, job search allowances, relocation allowances, and travel and subsistence allowances for those in training.

Table III-1 reports the TAA participation rates calculated from each of these data sources and shows that the two rates match quite closely. Using administrative data, we find that 50.2 percent of

[18] In keeping with the sample frame as defined in Chapter II, these individuals would also each need to be covered by a petition certified for TAA between November 2005 and October 2006, appear on a certified worker list that a state provided us, and have received a UI payment.

those who are eligible do participate in TAA, while the rate using survey data is 51.1 percent, a difference of just .9 percentage points.[19]

Because HCTC receipt is not captured through administrative data, a fairer comparison of the degree of correspondence would exclude HCTC from the survey's measurement of TAA participation. Doing so yields an estimated participation rate of 50.2 percent, exactly the figure we get using administrative data.[20]

**Table III-1: TAA Participation Rates, as Measured by Administrative and Survey Data**

| | Measured by Administrative Data | Measured by Survey Data |
|---|---|---|
| Eligibles who Participated in TAA | 50.2 | 51.1 |
| Excluding HCTC | -- | 50.2 |
| Sample Size | 19,389 | 2,744 |

Source: Administrative data and initial survey data.

Note: Sample weights were applied to account for sample design effects, and, for the survey sample, for survey nonresponse.

The very close correspondence in participation rates calculated from these two data sources suggests their equivalence for purposes of identifying TAA participants. However, the administrative data offer much larger sample sizes, and therefore can be used to estimate subgroup differences in participation rates more reliably. Because of this advantage, the administrative data are used to estimate state and demographic group differences in participation rates. These are reported in the subsequent sections.

[19] The rate of participation estimated from the survey data and reported here is slightly higher than the 50.3 percent participation rate reported in the evaluation's companion paper by Dolfin and Berk (2010). Dolfin and Berk excluded those who received only travel and subsistence allowances from their calculations

[20] Even eliminating HCTC from the survey's finding, the comparison is still not exact, because the survey was administered between March 2008 and April 2009, while the administrative data covers TAA participation up through June 2010. Some eligibles who had not participated in TAA by the time of the baseline survey might have participated after that date. Hence, they would be included in the findings from the administrative data, but not from the survey data. Restricting the administrative data to those who participated by December 2008 yields a participation rate of 49.7 percent.

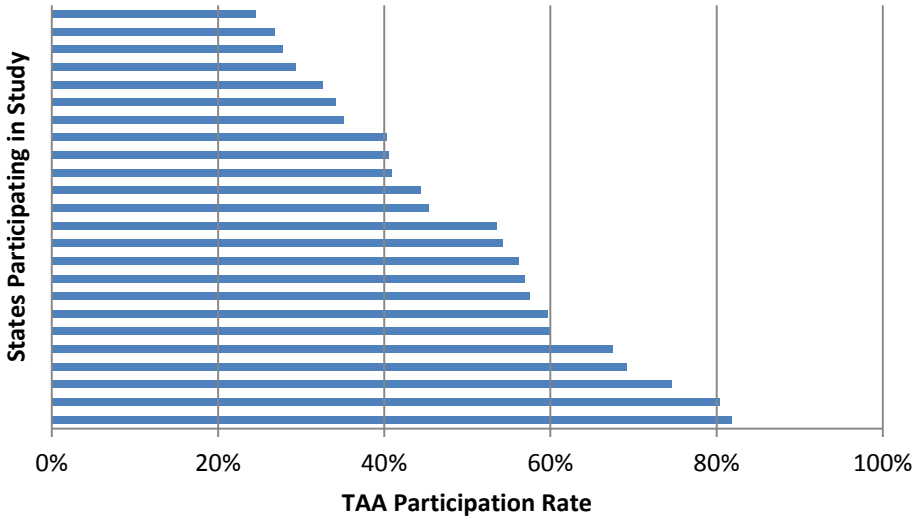
## 2. State Differences in TAA Participation Rates

Once a petition is certified, states obtain lists of affected workers and their contact information from employers. They then send letters inviting these workers to attend TAA orientation sessions or intake appointments. As noted elsewhere, state officials report that employers are generally cooperative in supplying the lists, and the names appear to constitute a good accounting of all eligible workers (D'Amico et al. 2009). Further, states appear to uniformly practice due diligence in notifying workers whose names appear on the lists.

However, some states appear more aggressive in their outreach efforts than others and make it easier for workers to access services (D'Amico et al. 2011; Salzman 2011). For example, in some states, workers who have not responded to initial mailings are sent reminder letters or are telephoned and invited in for services. Similarly, some states are more diligent in their efforts to reach non-English speakers, for example by translating letters into Spanish or other languages, if they have reason to believe that substantial numbers of workers covered by a given petition have limited English-language proficiency.

Whether as a consequence of these aggressive outreach efforts or for other reasons (e.g., the economic climate in some states allows workers to find reemployment without needing TAA services), some states record substantially greater take-up rates among eligibles than others. As Exhibit III-1 shows, in some states only about 30 percent of eligibles participate in TAA, while in others more than 80 percent participate. As a result, the state distribution of TAA participants, whose impacts are the focus of this report, looks considerably different from the state distribution of all eligibles and of TAA-eligible nonparticipants.

**Exhibit III-1: Participation Rate in TAA Among Eligibles, by State**




Source: Administrative data.

Note: Sample weights were applied to account for sample design effects. Each horizontal bar represents one state's TAA participation rate, calculated from among the state's eligibles. States are unnamed to protect their confidentiality.

## 3. Demographic and Regional Differences in TAA Participation

Table III-2 shows that there are also pronounced differences in TAA participation rates by demographic attributes of eligibles. For example, eligibles who are female are much more likely to participate in TAA than are those who are male. Eligible African-Americans participate at a considerably higher rate than do other groups, while Hispanics have the lowest participation among the racial/ethnic groups shown here. TAA participation rates increase steadily with age, especially for workers up to about age 40, a tendency that is displayed graphically in Exhibit III-2. Middle income workers have higher participation rates than lower or higher income workers.

We earlier reported pronounced differences in TAA participation rates across states. Likewise, there is also regional variation, with ETA's Region 3 showing significantly higher participation rates, and Regions 4, 5, and 6 showing lower participation rates. TAA participation rates are highest in smaller metropolitan areas or in urban or rural areas adjacent to any metropolitan area, while they are the lowest in the very largest metropolitan areas or in small urban or rural areas not adjacent to any metropolitan area. Potentially, a lack of access to One-Stop Career Center services can explain the latter finding, while the modest take-up rate in very large metropolitan areas (i.e., metropolitan areas with populations greater than 250,000) can perhaps be explained by the possibility that alternative job opportunities are relatively plentiful in these areas, making the need for TAA services less acute.

The geographical differences in TAA participation rates, coupled with the distribution of demographic attributes across TAA eligibles (shown in the second column of Table III-2), mean that TAA participants have a considerably different demographic profile than eligibles. For example, eligibles are considerably more likely to be male than female (56 percent versus 44 percent), but males have much lower TAA participation rates than females, so males and females in fact make up about equal proportions of TAA participants (shown in the third column of the table).

Importantly, TAA participants tend to be white (66 percent), older workers (41 percent are ages 50 or more and 71 percent are ages 40 or more), and reside in metropolitan or mid-sized urban areas. As will become clear in later chapters of this report, these characteristics are relevant to our interpretation and understanding of the overall impacts of TAA on participants.

## 4. Comparison Between TAA and WIA Participants

In a separate paper prepared as part of the evaluation, we report that TAA eligibles tend to be older than other UI claimants and are less likely to have continued their education beyond high school. They also tend to have had long tenures at their former employers, and to have had higher average wages than other claimants, even while living in areas with lower average earnings (Dolfin and Berk 2010).

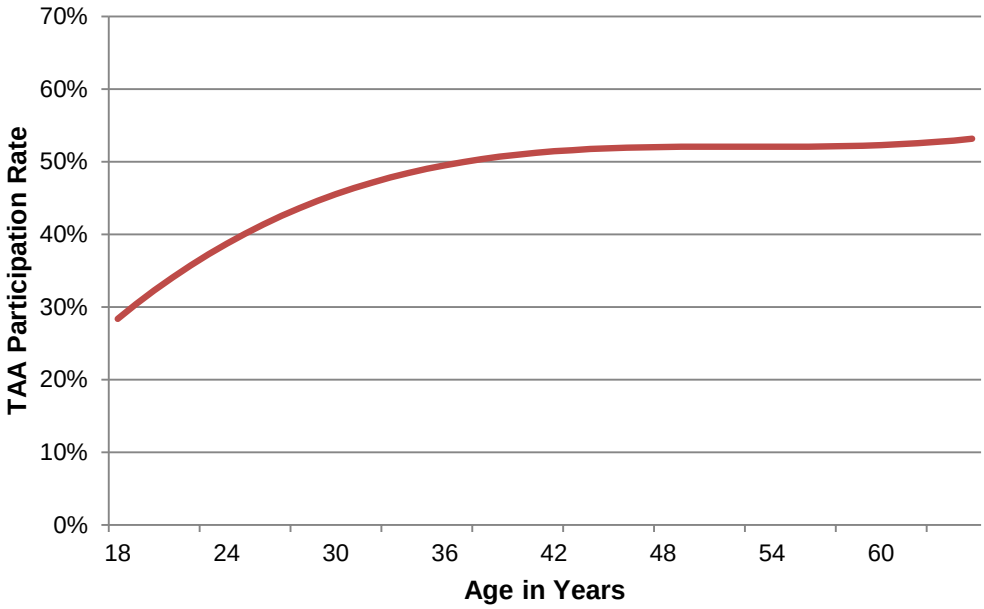
## Table III-2: TAA Participation Rates and Characteristics of TAA Participants and Nonparticipants, by Demographic and Regional Characteristics

| | % of Eligibles Participating In TAA | Characteristics of Eligibles | |
|---|---|---|---|
| | | All Eligibles | Participants |
| All Eligibles | 50.2 | ---- | ---- |
| Gender | | | |
| Female | 56.4 | 44.3 | 49.8 |
| Male | 45.3 | 55.7 | 50.3 |
| Race/Ethnicity | | | |
| White (non-Hispanic) | 50.4 | 66.6 | 66.2 |
| Black (non-Hispanic) | 60.9 | 17.8 | 21.3 |
| Hispanic | 39.5 | 9.8 | 7.6 |
| Other | 42.4 | 5.8 | 4.9 |
| Age (in Years) | | | |
| 29 or younger | 39.7 | 10.7 | 8.5 |
| 30 to 39 | 49.4 | 20.1 | 19.8 |
| 40 to 49 | 50.3 | 30.4 | 30.5 |
| 50 to 59 | 52.7 | 28.9 | 30.4 |
| 60 or more | 54.2 | 9.9 | 10.8 |
| Earnings in the UI Base Period | | | |
| Less than $20,000 | 45.1 | 22.9 | 20.7 |
| $20,000 to $29,999 | 55.4 | 27.6 | 30.5 |
| $30,000 to $39,999 | 54.2 | 19.3 | 20.9 |
| $40,000 to $49,999 | 51.9 | 12.3 | 12.7 |
| $50,000 or more | 42.6 | 18.0 | 15.3 |
| ETA Region | | | |
| 1-Boston Region | 55.8 | 8.6 | 9.6 |
| 2-Philadelphia Region | 46.1 | 14.9 | 13.7 |
| 3-Atlanta Region | 66.3 | 31.8 | 42.1 |
| 4-Dallas Region | 40.3 | 12.0 | 9.6 |
| 5-Chicago Region | 41.4 | 27.0 | 22.3 |
| 6-San Francisco Region | 24.5 | 5.7 | 2.8 |
| Urban/Rural Code | | | |
| Metro area of 250,000 or more | 45.0 | 53.1 | 47.8 |
| Smaller metro area or mid-size urban area adjacent to metro area | 55.9 | 24.9 | 27.8 |
| Small urban or rural area adjacent to metro area | 61.9 | 13.0 | 16.1 |
| Rural or urban area not adjacent to metro area | 45.8 | 9.1 | 8.3 |

Source: Administrative data.

Note: Locational items are coded based on individuals' zip codes, which are drawn from the UI claimant files that states provided. The Urban/Rural Code used here is based on the 2003 Rural-Urban Continuum Codes developed by the Economic Research Service of the U.S. Department of Agriculture.

**Exhibit III-2: TAA Participation Rates, by Age at UI Claim Date**




Source: Administrative data.

Note: This chart was generated by fitting a line to represent the effect of age on TAA participation, after applying sample weights to account for sample design effects.

Because TAA participants and participants of the WIA Dislocated Worker (DW) Program are both dislocated workers served through the One-Stop Career Center system, it is instructive to compare the characteristics of these two groups. As shown in Table III-3, TAA participants tend to be older, are much more likely to lack high school degrees and much less likely to be post-secondary school attendees, and are not as often referred by the Worker Profiling and Reemployment Services Program (WPRS). They also have somewhat higher pre-participation quarterly earnings, mostly because TAA participants are less likely to be zero earners. Finally, relative to WIA dislocated

**Table III-3: Characteristics of TAA Participants and WIA Dislocated Worker Participants**

| | TAA Participants | WIA DW Participants |
|---|---|---|
| Gender | | |
| Female | 49.8 | 52.3 |
| Male | 50.3 | 47.7 |
| Race/Ethnicity | | |
| White (non-Hispanic) | 66.2 | 60.0 |
| Black (non-Hispanic) | 21.3 | 21.7 |
| Hispanic | 7.6 | 13.7 |
| Other | 4.9 | 4.6 |

## Table III-3 (cont'd)

| | TAA Participants | WIA DW Participants |
|---|---|---|
| Age (in Years) | | |
| Younger than 30 | 8.5 | 14.6 |
| 30 to 39 | 19.8 | 23.3 |
| 40 to 49 | 30.5 | 31.3 |
| 50 to 59 | 30.4 | 24.8 |
| 60 or more | 10.8 | 6.1 |
| Highest Grade Completed | | |
| Less than high school diploma or GED | 22.2 | 9.6 |
| High school diploma or GED | 56.3 | 48.4 |
| Some post-secondary | 21.5 | 42.0 |
| Has a disability | 2.5 | 3.4 |
| Eligible veteran | 6.1 | 8.3 |
| Employment Status | | |
| Employed | 3.8 | 4.8 |
| Employed but with notice of termination | 3.7 | 7.4 |
| Not employed | 92.5 | 87.8 |
| Unemployment Compensation Status | | |
| Claimant referred by WPRS | 16.9 | 30.8 |
| Claimant not referred | 38.5 | 41.5 |
| Exhaustee | 21.0 | 10.0 |
| Neither claimant nor exhaustee | 23.6 | 17.7 |
| Limited English Speaker | 4.3 | 3.8 |
| Pre-Participation Quarterly Earnings | | |
| None | 6.4 | 10.3 |
| Greater than $0 to $5,000 | 26.3 | 27.7 |
| $5,001 to $7,500 | 25.1 | 21.6 |
| $7,501 to $10,000 | 20.1 | 16.5 |
| More than $10,000 | 22.1 | 24.0 |
| (Median quarterly earnings) | $6,673 | $6,360 |
| ETA Region | | |
| 1-Boston Region | 9.6 | 26.8 |
| 2-Philadelphia Region | 13.7 | 6.7 |
| 3-Atlanta Region | 42.1 | 18.1 |
| 4-Dallas Region | 9.6 | 8.1 |
| 5-Chicago Region | 22.3 | 22.2 |
| 6-San Francisco Region | 2.8 | 18.1 |
| Number of cases | | 212,391 |

Source: Administrative data. All numbers are percents, except median quarterly earnings, which are in dollars. Pre-participation quarterly earnings are calculated as the average of earnings in the second and third quarters prior to program participation.

WIASRD data are calculated for those who exited WIA sometime between January 1, 2007 and March 31, 2008 and received intensive services. All WIASRD tabulations are additionally restricted to those served in one of the 26 states participating in the TAA impact evaluation, except for the tabulation of ETA region, which is restricted to those served in the continental U.S.

workers, TAA participants are less likely to be drawn from ETA Regions 1 (Boston) and 6 (San Francisco) and much more likely to be from Region 3 (Atlanta).

## B. REASONS FOR APPLYING AND NOT APPLYING

Through the study's initial survey, participants were asked why they applied for TAA services. A summary of results, shown in Table III-4, illustrates that about 71 percent of participants cited an interest in training as a reason for applying. An interest in TRA benefits is the next most frequently cited reason, mentioned by 28 percent of participants. Much less common was an interest in other benefits, such as job search and relocation allowances, HCTC, or ATAA. A companion paper prepared as part of this evaluation reports that, with age, workers' interest in training declines, while their interest in TRA increases (Dolfin and Berk 2010).

### Table III-4: Reasons Cited by Participants for Applying for TAA Services

| | Percent Citing This Reason |
|---|---|
| Interested in training or schooling | 70.7 |
| Interested in TRA benefits | 27.6 |
| Interested in job search/relocation allowances | 8.6 |
| Needed help/seemed like a good idea | 2.0 |
| Interested in a better job | 1.8 |
| Interested in HCTC | 1.8 |
| Interested in ATAA | 1.1 |
| Unemployment Insurance benefits ended | 1.0 |
| Other or don't know | 6.2 |

Source: Initial and follow-up surveys.

Note: Sample weights were applied to account for sample design effects, and, for the survey sample, for survey nonresponse. Respondents could cite more than one reason, so the percentages sum to more than 100 percent.

The same paper also reports reasons that eligible nonparticipants give for not applying. The most commonly cited reason, mentioned by about 36 percent of eligible nonparticipants, is that they had found other jobs. But lack of information about the program is another common reason, with 14 percent reporting that they did not know about the program and 12 percent saying they did not know how to apply for program services.

# IV. PARTICIPANTS' EXPERIENCES IN TAA

To understand the impacts that TAA may have had on employment and other outcomes, this chapter describes the TAA services that participants received. We use administrative data—namely, the TAPR and TRA claimant files that states provided—to show the predominant patterns of service receipt, and to examine the extent to which services vary by participants' characteristics and from state to state. We next discuss the timing and duration of program participation, and then describe the types of TAA-funded training that TAA participants undertake. Next, we use initial and follow-up survey data to look at the extent of HCTC receipt, since this service is not a reportable item on the TAPR. Finally, we look at the extent of co-enrollment of TAA participants in WIA. With the exception of the analysis of HCTC receipt, all the results reported in this chapter were calculated for the certified worker TAA participant administrative data sample after applying sampling weights to account for sample design effects. According to the definition described in the previous chapter and used throughout this report, all workers who were eligible and who received a significant TAA service were counted as TAA participants.

We find that more than 90 percent of TAA participants received TRA and that about half of participants received TAA-funded training. By contrast, ATAA, HCTC, and job search, relocation, subsistence, and travel allowances were much less common. Older workers were much less likely than younger workers to access TAA training, and states differed markedly from each other in the extent to which their participants accessed training.

We noted in Chapter II that some workers established their UI claims even before they learned they were eligible for TAA. In fact, 27 percent of participants filed their claims more than 90 days before the petitions under which they were covered were certified for TAA. However, almost all participants became TAA eligible within plus or minus 90 days of their claim date.

Once eligible, workers who became participants tended to access services quickly, with about half doing so within the first six weeks. Their duration of participation—defined as time elapsed between the date of first service and date of program exit—was quite variable; participants can be divided into roughly three equal groups, with about one-third participating for up to one year, another third participating for between one and two years, and another third participating for more than two years. The average duration of participation was considerably longer for trainees than it was for those accessing other services, with trainees receiving 90 weeks of service.

About one third of TAA participants accessed services within the same quarter that their UI claims were established, and this percentage rises sharply in the several quarters thereafter. Participation rates peak in quarter three and taper gradually thereafter, but even four years after the claim date a small but non-trivial number of individuals (16 percent) were utilizing TAA services.

As will be reported in Chapter VI, the duration of training for TAA participants who underwent training (regardless of whether the training was TAA funded or not) was quite long, averaging well more than one year. Even TAA-funding training alone was lengthy, with the average time elapsed between the start date and end date of training equal to about 54 weeks, according to the TAPR. Because this training duration is so long, many trainees in our sample completed TAA-

funded training and re-entered the labor market after the onset of the Great Recession, which no doubt hampered their ability to quickly translate skill gains into enhanced employment and earnings.

We also found that 43 percent of TAA participants were co-enrolled in WIA, but the rate of co-enrollment is much higher for TAA trainees that it is for those receiving only TRA, and it is much higher in some states than others.

These findings are discussed in greater detail in this chapter.

## A. TAA SERVICES

In this section, administrative data provided by states are used to describe participants' receipt of various TAA services as well as the combinations of services that most frequently occurred.

### 1. Usage of TAA Services

Table IV-1 shows that 93 percent of persons classified as TAA participants received TRA[21] and just under half received a waiver (in combination with another service) or TAA-funded training (note that about 66 percent received training regardless of funding source, a finding that is discussed further in Chapter VI). About 12 percent received allowances of various types, including job search or relocation allowances, or—for those in training—travel or subsistence payments. ATAA was quite uncommon—just two percent of participants accessed this service.

Table IV-2 shows that participants in specific demographic subgroups tended to access different types of TAA services. The most notable difference across groups is that older participants were less likely to access TAA-funded training than their younger counterparts—more than 60 percent of those under age 40 accessed training, while fewer than 19 percent of those age 60 or more did so. Older participants were also less likely to receive an allowance, though they more commonly received waivers. ATAA, which can only be accessed by those age 50 or more at the time of reemployment, was quite uncommon even when we look at only those who were age-eligible. The differences in services by gender and across groups defined by base-period earnings, while significant, are modest in size.[22]

[21] Chapter IX reports that about 85 percent of TAA participants exhausted UI. Since UI exhaustion is a precondition for TRA receipt, the higher rate of TRA receipt than UI exhaustion suggests modest measurement error in one data source or another.

[22] These patterns of association were confirmed through multivariate analysis, which examined the independent association of each of these attributes with receiving services of different types.

## Table IV-1: Utilization of TAA Services Among Participants

| | Percent of Participants |
|---|---|
| Received TRA | 92.9 |
| Received Waiver (with another service)[a] | 49.2 |
| Received TAA-funded Training | 48.3 |
| Received Allowance (any type)[b] | 11.9 |
| Received Job Search Allowance | 5.0 |
| Received Relocation Allowance | 0.9 |
| Received Travel Allowance | 7.4 |
| Received Subsistence Allowance | 0.3 |
| Received ATAA | 2.1 |
| Sample Size | 10,476 |

Source: Administrative data.

Note: Sample weights were applied to account for sample design effects.

[a] According to the definition of participant used in this report, this category excludes those who received a waiver and no other service.

[b] The sum across these categories exceeds the number receiving an allowance of any type because some individuals received allowances of more than one type.

## Table IV-2: Utilization of TAA Services Among Participants, for Various Subgroups

| | Received TRA | Received Waiver | Received TAA Training | Received ATAA | Received Allowance |
|---|---|---|---|---|---|
| Gender | | | | | |
| Female | 94.4** | 49.2 | 51.5** | 1.5** | 12.1 |
| Male | 91.3 | 49.1 | 45.1 | 2.7 | 11.7 |
| Age (in Years) at Time of UI Claim | | | | | |
| 29 or younger | 93.0 | 45.3** | 63.3** | 0.0** | 20.2** |
| 30 to 39 | 93.2 | 46.2 | 61.5 | 0.0 | 15.6 |
| 40 to 49 | 94.0 | 47.0 | 55.4 | 0.3 | 13.9 |
| 50 to 59 | 89.9 | 51.0 | 38.8 | 5.6 | 8.0 |
| 60 or more | 96.9 | 57.9 | 18.8 | 2.6 | 4.3 |
| Earnings in the UI Base Period | | | | | |
| Less than $20K | 95.9** | 51.7** | 43.4** | 1.1** | 9.3** |
| $20K to $29.9K | 93.5 | 49.2 | 50.2 | 1.4 | 11.9 |
| $30K to $39.9K | 90.9 | 50.5 | 54.7 | 1.9 | 15.7 |
| $40K to $49.9K | 92.2 | 49.0 | 51.2 | 3.0 | 12.6 |
| $50K or more | 90.3 | 43.6 | 39.6 | 4.4 | 9.7 |

Source: Administrative data.

Note: Sample weights were applied to account for sample design effects.

* Subgroup differences are significant at the .05 level

** Subgroup differences are significant at the .01 level

## 2. Patterns of Participation

TAA participants received program services in various combinations of service packages. Three broad groups are defined in Table IV-3. One group, made up of about 50 percent of all participants, received TRA without also receiving TAA-funded training. Because eligibility for TRA requires a participant to be in training (or have completed training) or have a waiver from the training requirement, all those in this category received a waiver, and small numbers (about one percent) received a job search or relocation allowance.

The second major group, making up 48 percent of all participants, is comprised of those who received TAA-funded training. Almost all trainees also received TRA, about half received a waiver, and small numbers received allowances (mostly travel or subsistence allowances for those in training).

Finally, about two percent of all participants received neither training nor TRA, but did receive either ATAA or, very rarely, just an allowance.

Because they predominate so clearly, the first two groups—those who received TRA without training, and those who received training—will be the focus of the services subgroups whose impacts are described later in this report.

**Table IV-3: Predominant Combinations of Service Packages Among Participants**

| | Percent of Participants |
|---|---|
| Received TRA without TAA Training | 49.6 |
| With only a waiver | 48.5 |
| With a waiver and an allowance | 1.1 |
| Received TAA-funded Training[a] | 48.3 |
| With TRA | 43.3 |
| With a waiver | 25.4 |
| With an allowance | 10.5 |
| Neither TRA nor Training | 2.2 |
| Sample Size | 10,476 |

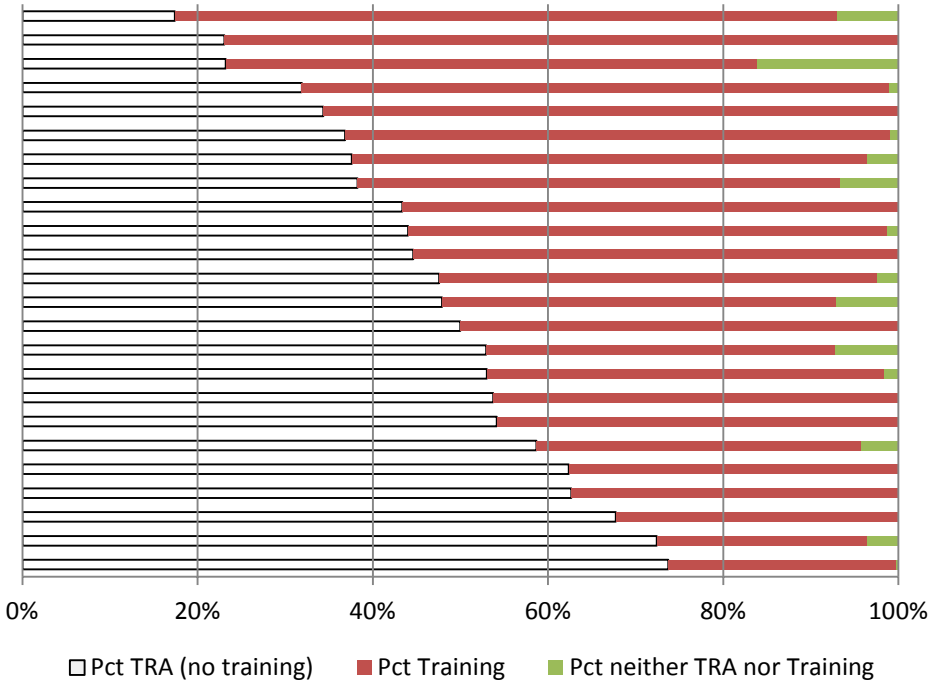
Source: Administrative data.

Note: Sample weights were applied to account for sample design effects.

[a] The sum across the subcategories exceeds the number that received training, because some individuals received more than one of these services in addition to training.

States differed greatly in the extent to which they emphasized different service packages (see Exhibit IV-1). Thus, in some states, TRA without training predominated, with more than two-thirds of participants receiving this service. In other states, training (with or without TRA) was much more prevalent. Finally, a few states showed a relatively high incidence of other services, especially ATAA.

**Exhibit IV-1: State Differences in the Prevalence of Various Service Packages, Among Participants**




Source: Administrative data.

Note: Sample weights were applied to account for sample design effects. Each row represents the data for a single state.

Differences across states are statistically significant at the .01 level using a chi-square test of significance.

## B. SERVICE TIMING AND DURATION

Knowing the onset of participants' eligibility and the duration of their services is important for several reasons. First, the U.S. slipped into a major economic downturn towards the end of the first decade of the 21st century, just after participants entered the study sample; this prevailing macro

economic climate is relevant to our understanding of the alternative job opportunities they faced.[23] Second, examining the onset of services in relation to when participants first become eligible shows how quickly they were enrolled and served. Third, the duration of participation denotes something about the intensity of the services participants may have received and, because those in services were unlikely to be working (except for those on ATAA), demarcates a period when we might expect their employment and earnings to be low. Finally, knowing when participation ended signals when participants might be ready to reenter the labor market, and, hence, when their employment and earnings could be expected to rebound.

## 1. The Onset of the Post-Separation Period

It was important that the evaluation mark the onset of the outcome measurement period for both TAA participants and their comparisons in a comparable way. For a participant, the onset could be defined as commencing with the job separation date, the UI claim date, or the date when the individual became eligible for TAA (which was either the separation date or the petition certification date, whichever came later). All three can be measured with the administrative data at hand.[24] However, for comparison group members, only the UI claim dates were available. Accordingly, to ensure consistency in measurement for both groups, the quarter in which the UI claim date occurred (denoted as $q_0$) demarcates the onset of the outcome measurement period for both participants and comparisons, with outcomes measured in each of the subsequent quarters (quarter $q_1$ up to quarter $q_{16}$ for some individuals).[25]

Exhibit IV-2 shows the frequency distribution of the UI claim date for TAA participants, and, for comparison purposes, the distribution for comparison group members. The distributions for

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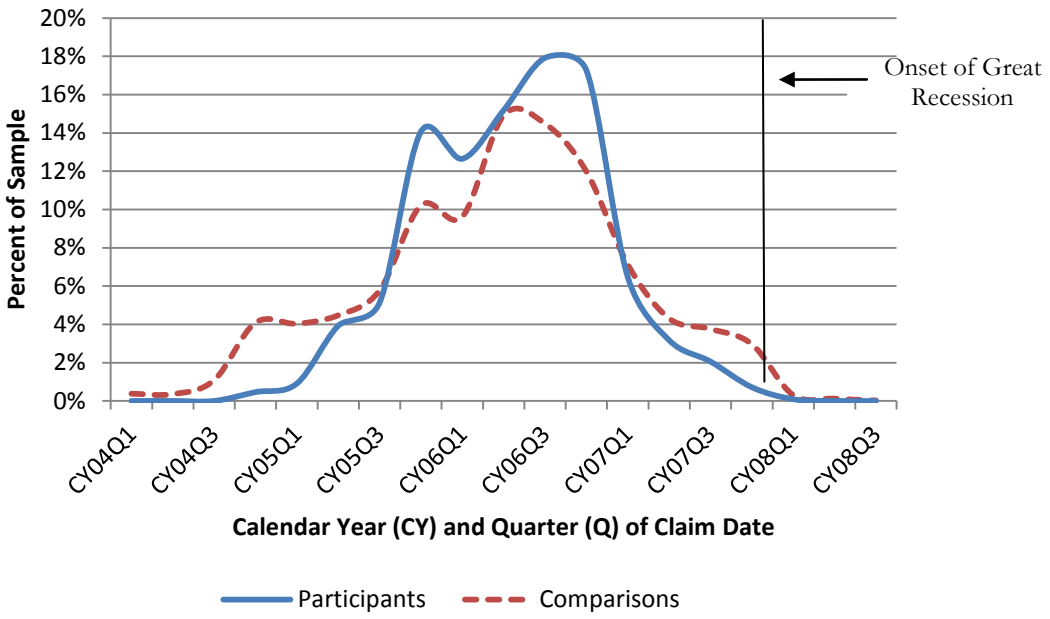
[23] The National Bureau of Economic Research (NBER) dates the onset of the Great Recession to December 2007 and declared it officially over by June 2009 (NBER, U.S. Business Cycle Expansions and Contractions, http://www.nber.org/cycles/cyclesmain.html).

[24] The TAPR includes the job separation date as one of its data items, and the petition certification date is known for all those in the certified worker sample.

[25] There is some ambiguity as to the optimal point at which the baseline period ends and the outcome measurement period begins for those in the treatment group. On the one hand, not all TAA participants knew they were eligible for TAA at the time they established their UI claims, such as when the petition under which the worker was to be covered had not been certified by the time the worker filed his or her claim; this line of reasoning suggests that the UI claim date is too early to demarcate the beginning of the outcome measurement period. On the other hand, there may be some anticipatory behavior such that, even before filing their claims, workers may have known they would become eligible for TAA and this knowledge may have influenced their job seeking behavior while still employed; this reasoning suggests that the UI claim date may be too late to demarcate the outcome period. In this report, we use the UI claim date because it balances these conflicting considerations, has the advantage of corresponding closely to the point of job separation, and can be consistently measured for treatments and controls. Sensitivity checks discussed in Chapter VI suggest that impact results would not differ fundamentally if alternative demarcation points were used.

**Exhibit IV-2: Frequency Distribution of UI Claim Dates for TAA Participants and Comparisons**




Source: Administrative data.

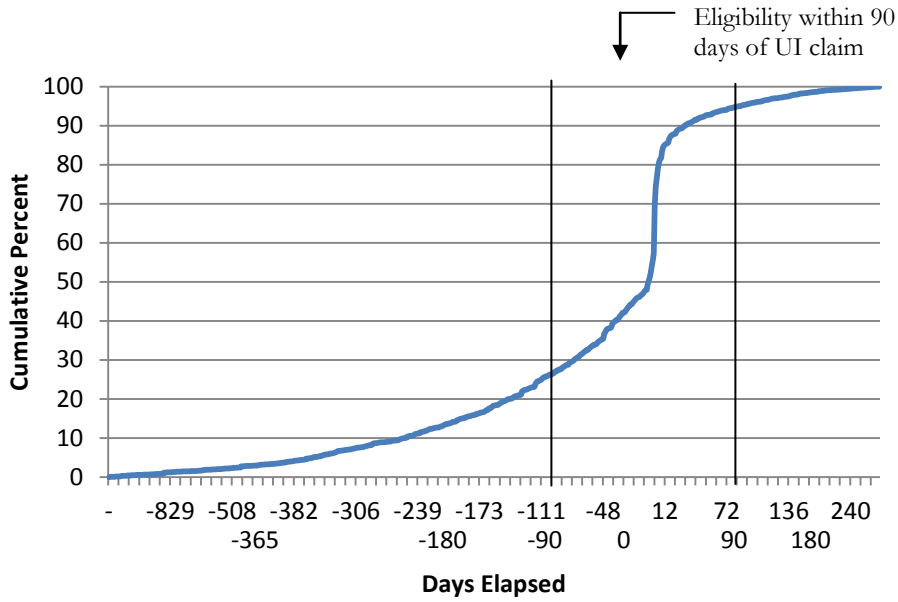
Note: Sample weights were applied to account for sample design effects.

the two groups are near mirror images of each other, evidencing the considerable care that was taken to ensure that comparisons matched quite well to treatment group members on UI claim characteristics (as well as on a host of other factors; see Chapter II).

For TAA participants, an individual's UI claim date need not correspond to the date that he or she first became eligible for TAA. In fact, Exhibit IV-3 shows that about 27 percent established their UI claims more than 90 days before they became eligible for TAA (represented by numbers less than -90 on the horizontal axis in the exhibit). This situation could have occurred when a worker experienced job separation and then filed a UI claim before the applicable petition was certified. It could also have occurred if the worker experienced a separation and filed a claim, was later recalled, and then experienced a subsequent separation under which the worker received TAA services, all within the same UI benefit year. For the 27 percent of participants captured under either of these scenarios, parts of at least two quarters of the outcome measurement period occurred before eligibility for TAA was established. For the vast majority of participants, however, TAA eligibility was established within 90 days (plus or minus) of the UI claim date.[26]

[26] Strictly speaking, these results pertain to TAA participants in the evaluation's analysis sample, and do not necessarily generalize to all participants covered by petitions certified within the study's one-year window. This

**Exhibit IV-3: Cumulative Frequency Distribution of Days Elapsed from Participants' TAA Eligibility Date to UI Claim Date**




Source: Administrative data.

Note: Sample weights were applied to account for sample design effects. Negative numbers denote that the UI claim preceded the TAA eligibility date.

## 2. Time to Onset of Services

One of the goals of the Trade Act of 2002 is to speed workers' entry into services, so as to hasten their return to employment. The provision that Rapid Response be provided whenever a petition is filed (regardless of whether it is certified) and the imposition of the 8/16 deadlines for entry into training are two of the legislation's strategies to promote this goal.

Exhibit IV-4 speaks to the speed with which workers accessed services by showing the cumulative percent of participants receiving services (as indicated in the TAPR) by weeks elapsed from their TAA eligibility dates. Eligibility for TAA begins with the separation date or the petition certification date, whichever is later. As the chart suggests, about half of the participants received their first service (as recorded in the TAPR) within six weeks of their eligibility, and more than 75

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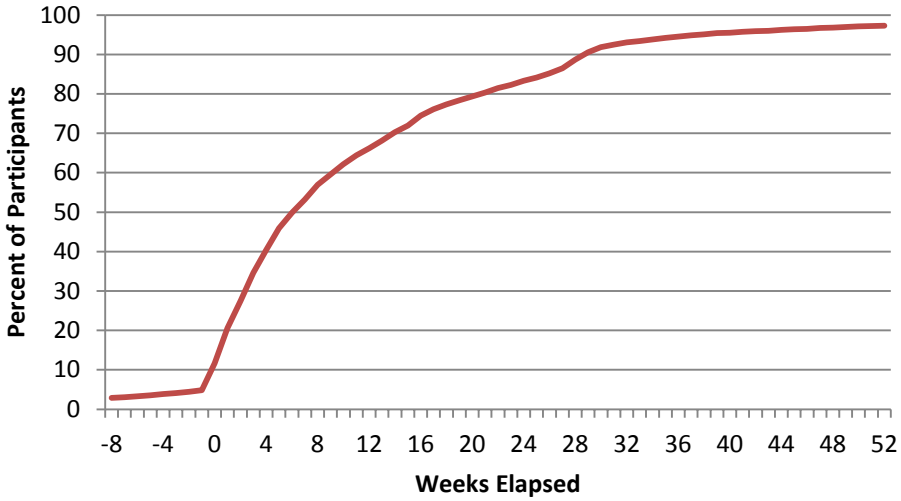
(continued)

limitation in inference occurs because not all separations covered by these petitions had yet occurred at the time the states provided us with their UI claimant files. See Chapter II for details.

percent did so by week sixteen.[27] A small percentage of participants (those represented by negative weeks elapsed) began accessing services even earlier than their TAA eligibility dates, suggesting that some individuals first began accessing services from WIA or some other One-Stop system partner before they became TAA eligible.[28]

**Exhibit IV-4: Cumulative Frequency Distribution of Weeks Elapsed from Participants' TAA Eligibility Date to Participation Date**




Source: Administrative data.

Note: Sample weights were applied to account for sample design effects. Negative numbers denote that participation preceded the TAA eligibility date, and implies that participation began with enrollment in a One-Stop partner program's services.

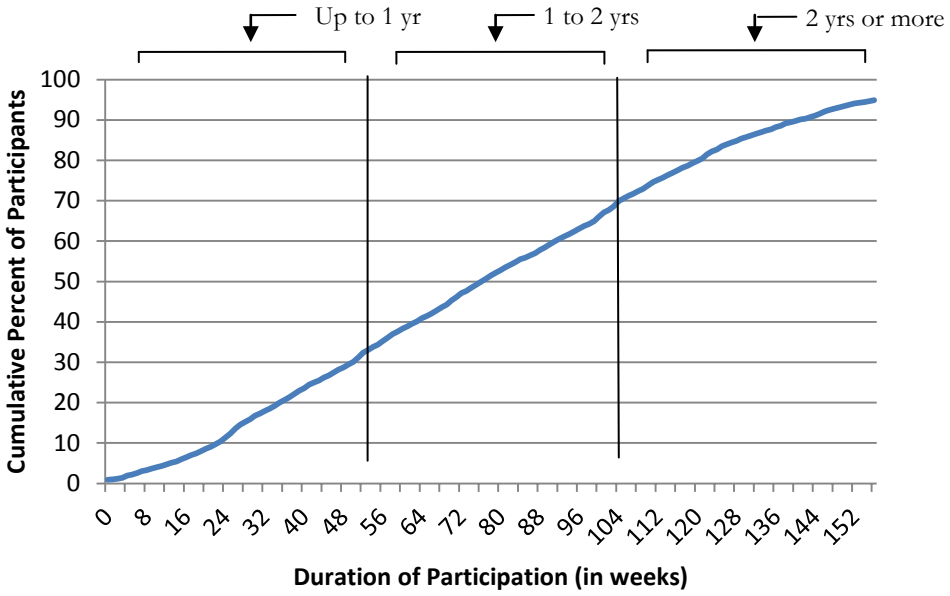
[27] Nearly all those accessing services more than 16 weeks after eligibility received TRA, which means that they would be in technical violation of the 8/16 rule. This fact suggests that (as was discussed in Chapter III) waivers are not always being recorded in the TAPR, or, if they are recorded, are not used to mark the start of program participation.

[28] Although guidance on this has changed over the past decade, according to the most recent TAPR instructions issued by ETA, the date of participation represents the date an individual began receiving his or her first service funded by TAA or a partner program. Due to limitations of states' management information systems, partner program participation is not always recorded; thus, the percentage receiving services from a partner before TAA eligibility is established is likely higher than this chart suggests. Furthermore, date of participation as it is measured in the TAPR does not include workers' access to Rapid Response or other One-Stop core services; taking these activities into account, very likely many or most participants could be considered to have begun services before they officially became TAA eligible.

## 3. Duration of Participation

As explained in Chapter I, TAA can fund participants in training for more than two years, and, for those in training, can provide TRA payments for just as long. Moreover, post-training placement assistance can be provided after training ends, extending the duration of participation still longer. But most individuals participate appreciably less than these upper-bound limits. As Exhibit IV-5 shows, about one third of the study sample participated for less than one year, another third participated for one to two years, and a final third for more than two years.[29]

**Exhibit IV-5: Cumulative Frequency of Duration of TAA Participation (in weeks)**




Source: Administrative data.

Note: Sample weights were applied to account for sample design effects.

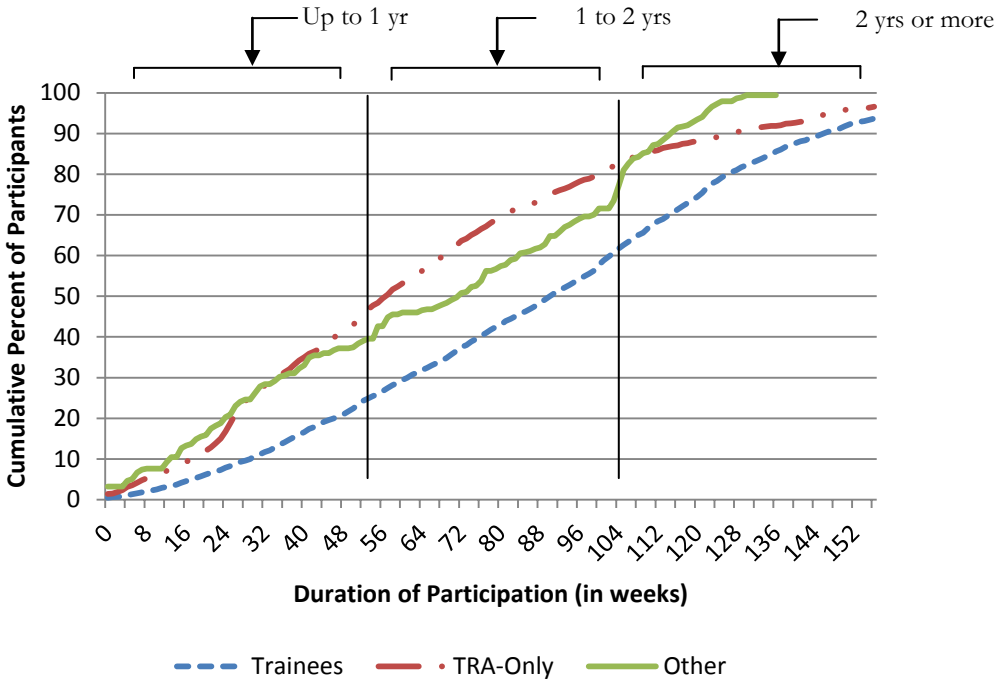
The different service groups that we identified earlier in this chapter had different participation profiles. In particular, trainees tended to have much longer spells of participation. As Exhibit IV-6

[29] The duration of participation is measured as weeks elapsed between the date of program participation (i.e., when participation begins) and the date of program exit, as measured by the TAPR, for those with valid participation and exit dates. Persons with incomplete spells (i.e., who have not yet exited) made up about 14 percent of all participants and were excluded from the tabulation. As a result, there is a slight truncation bias in the duration of participation calculated in this exhibit in comparison to what it would be had it been calculated after all spells of participation were completed.

shows, only about 24 percent of trainees participated for less than one year, while the comparable figures among those who received TRA without training (TRA-only, in the chart) or other services (primarily ATAA recipients) are 46 and 40 percent, respectively.[30] Nearly 40 percent of trainees were still participating in services after two years, while the comparable figure is 16 percent among TRA-only recipients and 23 percent among those who received other services.

Table IV-4 shows these differences in another way, by reporting the average duration of participation for the various service groups. The average duration was 89 weeks for trainees, 64 weeks for those in the TRA-only group, and 66 weeks for those receiving other services.

**Exhibit IV-6: Cumulative Frequency of Duration of TAA Participation (in weeks), by Services Subgroup**




Source: Administrative data.

Note: Sample weights were applied to account for sample design effects.

[30] Individuals can receive basic TRA for 52 weeks, minus the number of weeks paid through UI. These weeks can be paid over a 104-week period, beginning with the first week following the week in which the worker's most recent qualifying separation occurred, and for any subsequent week in which the conditions for receipt of basic TRA apply. Workers in the TRA-only subgroup can participate in TAA beyond the 104-week period to the extent that they are still receiving job search assistance or other One-Stop Career Center services.

**Table IV-4: Mean and Median Weeks of Participation in TAA, by Service Subgroup**

| | Sample Size | Weeks of Participation | |
|---|---|---|---|
| | | Mean | Median |
| All Participants | 6,690 | 79.8 | 77.0 |
| TAA-funded Trainees | 3,865 | 89.2 | 91.0 |
| TRA without TAA Training | 2,639 | 64.4 | 57.0 |
| Others | 186 | 66.4 | 72.0 |

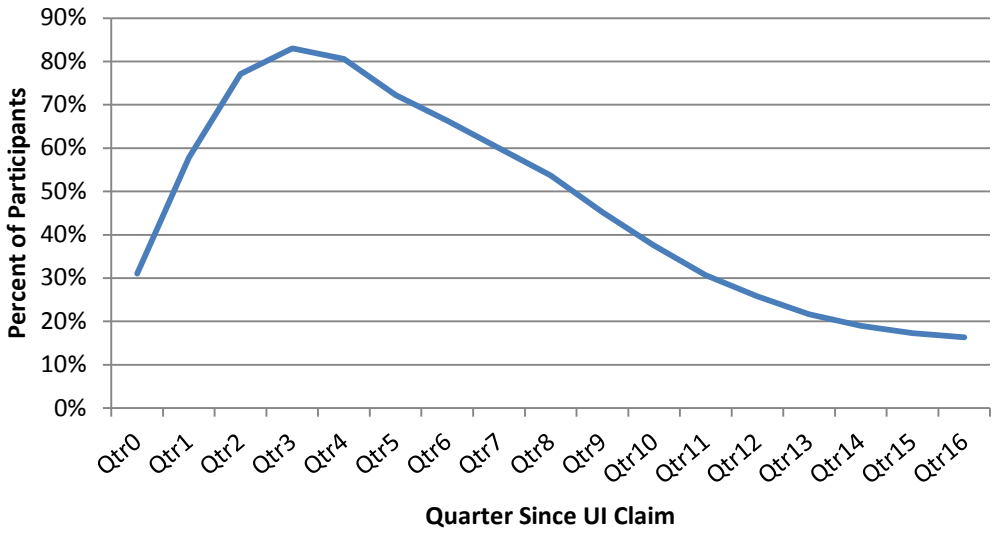
Source: Administrative data.

Note: Sample weights were applied to account for sample design effects. Weeks of participation is calculated from the TAPR as the weeks elapsed from the participation date to the exit date.

## 4. Participation Rates by Quarter Since UI Claim

We have seen from previous displays that not everyone is yet eligible for TAA at the time they file their UI claims (Exhibit IV-3), that once they become eligible it takes some workers a while before they receive their first TAA services (Exhibit IV-4), and that individuals spend varying lengths of time accessing these services once their spell of participation begins (Exhibit IV-5). Exhibit IV-7 summarizes this information by showing the percent of participants enrolled in TAA in each of the outcome measurement quarters, starting with quarter 0 (Qtr0), the quarter in which the UI claim date occurred.

**Exhibit IV-7: Percent of TAA Participants Utilizing TAA Services since UI Claim Date**



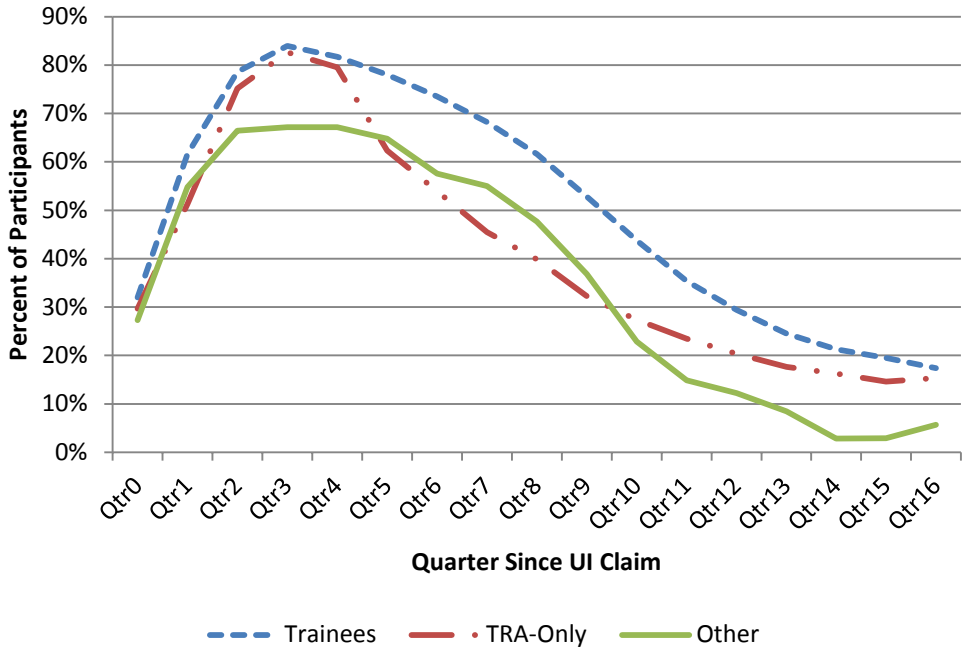

Source: Administrative data.

Note: Sample weights were applied to account for sample design effects.

As the exhibit shows, only about 30 percent of participants were participating in TAA beginning with the quarter in which the claim occurred, but the participation rate rises sharply in the several quarters thereafter, reaching a peak of 83 percent by quarter three. The participation rate drops steadily over the subsequent years, but, even by quarter 16, more than 16 percent were still participating. These 16 percent had not necessarily participated continuously in TAA during the four years since their claim begin dates; more likely that they had varying durations of participation and had first begun participating in TAA in any of the preceding quarters. In any event, this exhibit shows that appreciable percentages of individuals participated in TAA during any single quarter during the four years after their UI claim dates, which has implications for the sorts of employment and earnings that might be expected of them.

We saw from Exhibit IV-6 that trainees had longer spells of participation than others, and this finding explains why, in comparison to other participants, a higher percentage of trainees were enrolled in TAA during each of the quarters after the UI claim date (see Exhibit IV-8). For example, at two years after the UI claim date (Qtr8), 62 percent of trainees were participating in TAA, while the comparable figure among TRA-only participants was 40 percent. After three years (Qtr12), the figures are 29 percent and 20 percent for the two groups, respectively. Among those receiving other services (i.e., neither training nor TRA), the peak participation rate in any single quarter was not as high as for the other two groups, and the participation rate was consistently lower for them beginning in quarter 10.

**Exhibit IV-8: Percent of TAA Participants Utilizing TAA Services since UI Claim Date, By Service Group**




Source: Administrative data. Sample weights were applied to account for sample design effects.

Note: Trainees are those listed in the TAPR as having undertaken training; TRA-only are those who received TRA without training; "Other" denotes those who received some other TAA service but neither training nor TRA (most received ATAA).

## C. TAA-FUNDED TRAINING AND LABOR MARKET ENTRY

The goal of training in the TAA program is to enable trade-affected workers to gain new skills that will enable them to obtain suitable reemployment. As clarified in TEGL 13-05, suitable employment is defined as employment in a job where the skill requirements are substantially equal to or at a higher level than the job from which the worker was displaced, and that pays at least 80 percent of the worker's previous wage. Although the goal is for training to lead to suitable employment, the formal requirements for the approval of training speak only to the need for and availability of training:

1. No suitable employment is available for the worker in the absence of training,
2. The worker would benefit from the training,
3. There is a reasonable expectation of employment following completion of the training,
4. The training the worker requests is reasonably available,
5. The worker is qualified to undertake and complete the training,
6. The training is suitable for the worker and available at a reasonable cost.

Previous reports prepared as part of this evaluation examined how states have operationalized some of these provisions. We reported that, as a way of meeting the reasonable cost criterion, states typically impose cost caps meant to cover the cost of tuition and books and supplies, but that the caps are almost always considerably more generous that what is allowable for WIA-funded training. Most states, but not all, additionally require the worker to select training from the WIA eligible training provider list in order to ensure training quality.

Perhaps the biggest implementation hurdle that states have mentioned relates to complying with the 8/16 rules. These rules cause a worker to lose eligibility for TRA unless he or she is enrolled in approved training no later than 16 weeks after the separation or 8 weeks after the petition certification date, whichever comes later, unless the worker is granted a waiver from this requirement. Case managers reported their concern that the 8/16 deadlines often do not give workers enough time to become informed about TAA eligibility, decide to undertake training, undergo appropriate assessments to help them select training programs, and research alternative providers. Partly to protect workers' eligibility for TRA while keeping them from making overly hasty training decisions, states commonly issue waivers from the training requirement, which, they report, can greatly increase their paperwork burden (D'Amico et al. 2009).

As Chapter VI will describe, occupational skills training by far predominated among TAA trainees (as opposed to remedial training or higher education). Further, almost all of this training took place in a classroom setting, even though, according to regulations then in place (see TEGL 11-02), states were to give priority to on-the-job training (OJT). A major reason why OJT was not more widespread is that TAA staff did not have the time to develop and oversee OJT training slots (D'Amico et al. 2009).

As will also be reported in Chapter VI, the duration of training for TAA participants who underwent training (regardless of whether the training was TAA funded or not) was quite long, averaging well more than one year. Even TAA-funding training alone was lengthy, with the average

time elapsed between the start date and end date of training equal to about 54 weeks, according to the TAPR. It is worth drawing attention to the implications of this fact for the timing of labor market entry: trainees in this study were likely to have completed their TAA-funded training and sought re-entry into a national labor market that had significantly deteriorated.[31] As such, their participation in TAA training may be slow to realize returns. Exhibit IV-9 makes this point by showing the calendar quarter of presumed labor market entrance for TAA trainees, and, for comparison, other TAA service groups. For purposes of this exhibit, we assume that trainees suspended their job searches while they were in training, and resumed them (i.e., re-entered the labor market) upon completing their TAA-funded training. We also assume that TAA participants in the two other service groups were in the labor market—that is, either actively engaged in job searches or re-employed—continuously since their UI claim dates.[32]

The exhibit shows that more than 50 percent of trainees re-entered the labor market after the onset of the recession, whereas less than one percent of TAA participants in the two other training groups did so.

## D. SURVEY ESTIMATES OF HCTC RECIPIENCY

Whether a trade-affected worker took advantage of the Health Coverage Tax Credit (HCTC) is not a reportable item on the TAPR, and hence administrative data we collected from state workforce agencies cannot provide estimates of those who accessed this service. The initial and follow-up surveys administered to TAA participants, however, did ask about HCTC receipt and hence can provide this information.

We saw in Chapter III that less than one percent of TAA participants received only HCTC and no other TAA service (i.e., training, TRA, ATAA, or a job search, relocation, travel, or subsistence allowance). However, the incidence of HCTC recipiency overall is much larger than this one percent figure. As Table IV-5 shows, 13.5 percent of TAA participants reported receiving HCTC. Dolfin and Berk (2010) reported that, among TAA participants, receipt of HCTC increased with age. Moreover, rates of participation were higher for those with higher pre-layoff wages, and the average amount of the tax credit received was $1,150.

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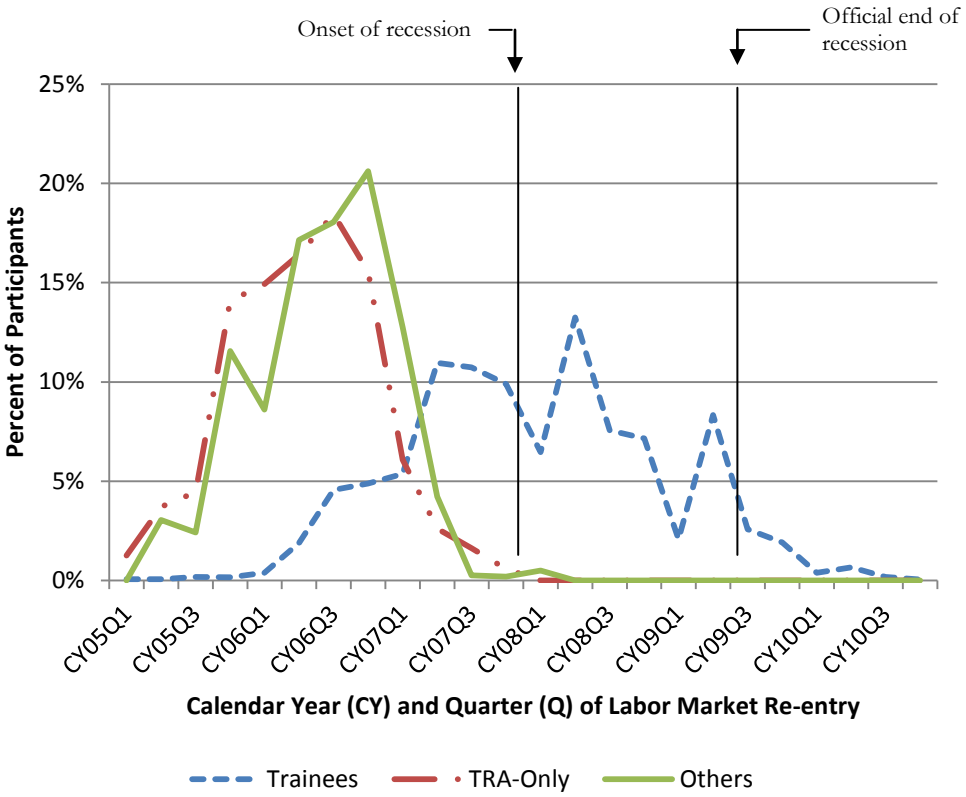
[31] This discussion highlights *national* economic conditions. Many trade-affected workers' communities suffered localized harm that resulted in bleak labor market opportunities even prior to the onset of the recession. As discussed in Chapter I, the selection of the comparison group accounted for these local conditions.

[32] To receive TRA without being in training, a worker must generally comply with the UI work search test, unless he or she has been granted a waiver for a reason that would not require job search. However, 77 percent of TRA-only participants and those in the "other" service category who were granted waivers were listed as having marketable skills as the waiver condition, so the assumption that individuals in these groups were actively engaged in job searches is a reasonable one.

## Exhibit IV-9: Participants' Presumed Labor Market Entry, by Calendar Year and Quarter, for TAA Service Subgroups




Source: Administrative data. Sample weights were applied to account for sample design effects.

Note: For TAA trainees, labor market re-entry is dated as their date of training completion, as recorded in the TAPR. For other service groups, it is dated as the UI claim date. Trainees are those listed in the TAPR as having undertaken training; TRA-only are those who received TRA without training; "Other" denotes those who received some TAA service other than training or TRA (most received ATAA).

## Table IV-5: Percent of TAA Participants Receiving HCTC

| | Percent |
|---|---|
| Received HCTC | 14.5 |

Source: Mathematica Initial and follow-up survey.

Note: Sample weights account for sample design effects and nonresponse.

## E. CO-ENROLLMENT IN WIA

TEGL 5-00, issued in the wake of the enactment of WIA, expresses ETA's vision that TAA should be well integrated into the One-Stop service delivery system. TEGL 21-00, released in the spring of 2001, followed up by encouraging the co-enrollment of TAA participants into the WIA Dislocated Worker Program and admonished states to ensure that, at the least, trade-affected workers be given timely access to early intervention and One-Stop core services. The Trade Act of 2002, with its requirement that One-Stop Career Centers serve as the main points of participant intake, further emphasizes the importance of system coordination. Several papers prepared as part of this evaluation explored TAA's linkages with WIA and noted both the considerable strides that states have made in implementing ETA's guidance and the challenges that remain (Dunham 2009; Mack 2009).

Reliably measuring the extent to which TAA participants are co-enrolled in WIA has been notoriously difficult heretofore, because of limitations of states' data systems. Both the TAPR and the WIASRD include fields denoting which TAA and WIA participants are co-enrolled in the other's program, but state respondents acknowledge that these data items are reported unreliably. In principle, the TAPR and WIASRD files that states send to ETA separately as part of the states' reporting requirements could be matched by the participant identification (ID) numbers to measure the extent of co-enrollment, but these files do not include Social Security Numbers (SSNs) and states commonly use different ID numbers for coding individuals in their TAPR and WIASRD submissions.[33] However, the evaluation team asked states to provide administrative data with SSNs appended, so we can measure the extent of co-enrollment more carefully than has previously been possible. For purposes of this discussion, we define co-enrollment to mean those TAA participants who are listed in the WIASRD as having received a staff-assisted service.[34]

### 1. Co-enrollment Overall and by Subgroup

Table IV-6 draws on these data to show the percent of TAA participants enrolled in WIA, overall and by demographic subgroup. As the table shows, about 38 percent of TAA participants are co-enrolled in WIA overall. Of those co-enrolled, individuals are overwhelmingly enrolled in the WIA formula-funded Dislocated Worker Program, though 16 percent were enrolled under a WIA National Emergency Grant (NEG), and 12 percent in the adult program. Almost two thirds were listed in the WIASRD as having received training services, 31 percent received intensive services (but not training), and 6 percent received staff-assisted core services (but neither intensive nor training services).

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[33] The instructions that ETA has issued for both the TAPR and WIASRD admonish states to use a common ID in each data file for indentifying individuals that are enrolled in both programs. However, exploratory analyses we have conducted make clear that this guidance is not generally followed.

[34] Additionally, we require the date on which WIA participation begins to be on or after the UI trigger claim date, to avoid treating as co-enrollees individuals who may have been enrolled in WIA well before their trade-related separation.

## Table IV-6: Enrollment in WIA by TAA Participants, Overall and by Demographic Subgroup

| | | Gender | | Age at the Time of UI Claim | | | |
|---|---|---|---|---|---|---|---|
| | Overall | Female | Male | LT 30 | 30-39 | 40-49 | 50 + |
| Co-enrolled in WIA | 38.3 | 41.7 | 35.2 | 43.6 | 43.5 | 41.9 | 31.6 |
| Program of Participation[a b] | | | | | | | |
| Dislocated Worker (except NEG) | 80.5 | 82.1 | 78.6 | 84.7 | 82.5 | 80.0 | 79.6 |
| NEG | 16.3 | 13.0 | 19.9 | 20.4 | 16.2 | 17.7 | 14.0 |
| Adult | 11.6 | 12.8 | 10.2 | 9.2 | 12.5 | 11.3 | 12.2 |
| Youth | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| Other or not specified | 6.6 | 5.8 | 7.4 | 3.2 | 4.0 | 6.1 | 8.2 |
| Highest Level of Service[a] | | | | | | | |
| Staff-Assisted Core | 5.8 | 4.1 | 7.6 | 4.5 | 3.1 | 5.0 | 8.8 |
| Intensive Services | 31.4 | 30.9 | 32.0 | 19.7 | 27.0 | 28.3 | 40.9 |
| Training Services | 62.8 | 65.0 | 60.4 | 75.8 | 69.8 | 66.7 | 50.3 |

Source: Administrative data.

Note: Sample weights account for sample design effects.

[a] This tabulation is restricted to those TAA participants who are enrolled in WIA.

[b] The sum across program type can exceed 100 percent because some individuals are enrolled in more than one WIA program.

Regarding demographic subgroup differences, males and those ages 50 or older were less likely to be co-enrolled than females and workers who are younger than age 50. Among participants, older workers were much less likely to be listed in the WIA as receiving training services than those who are younger.

Table IV-7 reports rates of WIA co-enrollment by service subgroup. As the table shows, TAA trainees were much more likely to be co-enrolled in WIA than those who received only TRA or other services (that is, neither training nor TRA). Among those co-enrolled, the service subgroups differ as well in the types of WIA services received. Seventy-eight percent of TAA trainees are listed in the WIASRD as a training participant, while the figure among those receiving only TRA is just 37 percent.[35] The TRA-only participants listed in the WIA as receiving training presumably had their training funded wholly by WIA or by some source other than TAA, since these individuals are not listed in the TAPR as trainees. Bear in mind that the level of WIA service an individual received is

[35] A TAA trainee could be listed in the WIASRD as having received training services regardless of whether WIA funded the training, so long as the WIA participant was receiving the training as part of his or her WIA service plan.

tabulated just for those who are co-enrolled, so the 37 percent of TRA-only participants who received training represents only about nine percent of all TRA-only participants.

**Table IV-7: Enrollment in WIA by TAA Participants, Overall and by Service Subgroup**

| | Overall | Trainees | TRA-Only | Other |
|---|---|---|---|---|
| Enrolled in WIA | 38.3 | 54.7 | 25.0 | 32.8 |
| Program of Participation[a b] | | | | |
| Dislocated Worker (except NEG) | 80.5 | 81.3 | 79.0 | 80.1 |
| NEG | 16.3 | 19.4 | 10.7 | 12.5 |
| Adult | 11.6 | 8.9 | 16.0 | 22.3 |
| Youth | 0.0 | 0.0 | 0.0 | 0.0 |
| Other or not specified | 6.6 | 6.5 | 6.9 | 2.3 |
| Highest Level of Service[a] | | | | |
| Staff-Assisted Core | 5.8 | 3.9 | 9.0 | 9.0 |
| Intensive Services | 31.4 | 18.0 | 53.8 | 79.2 |
| Training Services | 62.8 | 78.1 | 37.2 | 11.8 |

Source: Administrative data.

Note: Sample weights account for sample design effects. Trainees are those listed in the TAPR as having undertaken training; TRA-only are those who received TRA without training; "Other" denotes those who received some other TAA service but neither training nor TRA (most received ATAA).

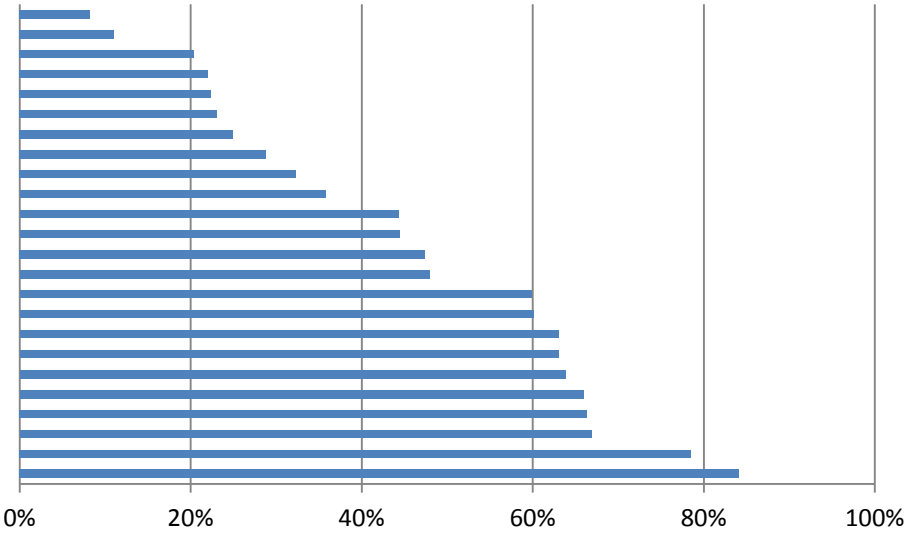
[a] This tabulation is restricted to those TAA participants who are co-enrolled in WIA.

[b] The sum across program type can exceed 100 percent because some individuals are enrolled in more than one WIA program.

## 2. State Differences in Co-enrollment Rates

Through qualitative research, we learned that states' workforce agencies differ greatly in the emphasis they place on co-enrollment and with respect to the policies they develop about it. The results shown in Exhibit IV-10 confirm that these differing state policies translate into co-enrollment rates that also differ greatly—in some states, the rate at which TAA participants are co-enrolled exceeds 80 percent, while in others the rate of co-enrollment is barely 10 percent.

## Exhibit IV-10: Co-enrollment Rate of TAA Participants in WIA, by State




Source: Administrative data.

Note: Sample weights account for sample design effects. Each bar represents one state's co-enrollment rate.

# V. IMPACTS ON REEMPLOYMENT SERVICES

The Trade Act of 2002 and ETA's accompanying guidance (see especially TEGL 11-02) emphasize that trade-affected workers should be provided access to early intervention and reemployment services, as well as to a broad array of One-Stop core and intensive services, even before a petition certification decision has been made. Furthermore, ETA admonishes that TAA, operating in the context of the One-Stop Career Center system, should not attempt to duplicate services that are already available in the One-Stop system. Therefore, the co-enrollment of TAA participants in WIA might be the most efficient means of delivering necessary reemployment and case management services to these workers. In light of these considerations, we hypothesize that the TAA program's partnerships in the One-Stop system increase access to reemployment services for TAA participants.

At the same time, comparison group workers were drawn from the same local areas as the TAA participants and were therefore likely to have been in similar proximity to One-Stop Career Centers as TAA participants. Thus, an important evaluation objective was to measure these "counterfactual" services received by the comparisons, because they represent the services that participants would have received in the absence of the TAA program

This chapter describes the experiences of TAA participants and comparisons in accessing reemployment services, as well as the impact of TAA on the receipt of these services. Specifically, it addresses the following key research questions:

- By way of establishing the counterfactual—the services that TAA participants would have received in the absence of the TAA program—what reemployment services do comparison group members access and with what frequency?
- Does TAA participation increase the receipt of reemployment services?
- Does TAA participation increase access to WIA program services? Does it have a larger effect on increasing access to some types of WIA program services rather than others?
- Do the impacts of the TAA program on reemployment services vary by workers' demographic or local area characteristics or based on their service subgroup?

For the most part, we examined these questions using data from the initial and follow-up surveys. We looked at services focused on helping individuals find employment quickly as well as those with the broader goals of assisting individuals in longer-term career planning. We supplemented this analysis by using administrative data for TAA participants and their comparison group counterparts in order to examine rates of enrollment in WIA and the types of WIA services that enrollees received.

We found that more than three quarters of those in the comparison group accessed reemployment services since their job losses, suggesting widespread accessibility of these services to the unemployed. Nonetheless, TAA still substantially increased service receipt—94 percent of TAA

participants received at least one reemployment service, while 77 percent of comparison group members reported doing so, a statistically significant gain of 17 percentage points.

According to the survey data, TAA increased access to reemployment services of all types, including those designed to help workers find jobs immediately—with resume assistance or job searches, for example—and those focused on longer-term career planning. Overwhelmingly, the One-Stop Career Center system was the primary source that TAA participants and their comparisons used to access these services, but, although TAA participants and comparisons used the services from the same source, TAA participants were much more likely to report that they found the services helpful. TAA also substantially increased the extent to which participants enrolled in WIA with staff-assisted services. Whereas only 4 percent of comparisons received WIA staff-assisted services, 38 percent of TAA participants did so, a statistically significant increase of 34 percentage points. The higher rates of receipt of reemployment services in the survey data than the WIASRD data suggest that many sample members—and especially comparisons—accessed only One-Stop self-services or informational services, and were doing so perhaps as part of their registration for UI.

The impacts of TAA were relatively consistent across worker subgroups, although there were a few significant differences. Most notably, the program's impact was larger for younger workers with respect to the receipt of reemployment services as well as the rate of enrollment in WIA.

In detailing these findings, we begin by describing results from the initial and follow-up surveys on the receipt of reemployment services for each research group. We then turn to the administrative data to estimate the impact of TAA on enrollment in WIA, and, finally, to describe differences in program impacts by worker subgroups.

## A. IMPACTS ON RECEIPT OF REEMPLOYMENT SERVICES

As previously noted, the Trade Act of 2002 requires that trade-affected workers be given access to One-Stop core and intensive services, and further mandates that Rapid Response assistance be provided whenever petitions are filed, regardless of whether they are yet certified. This section compares the reemployment services accessed by TAA participants with those accessed by comparisons and reports the impacts of TAA on service receipt. It also describes where workers typically accessed reemployment services, and how helpful they perceived them to be.

### 1. Reemployment Services Received

As Table V-1 shows, 77 percent of comparisons received any of the reemployment services covered in the survey, while the comparable figure among TAA participants is 94 percent, a 17 percentage point increase.

Each of the nine reemployment services considered here has the goal of enhancing workers' careers. However, they can be divided into two categories: those that provide information or guidance focused on immediate job finding, and those that facilitate longer-term career planning. TAA substantially increased access to every one of the services listed in both categories.

With specific regard to job-focused guidance and informational services, 57 percent of comparisons received assistance in searching for work, the most common reemployment service they reported receiving (Table V-1). Still, the TAA program boosted access to this service by 12 percentage points. Impacts on other job-focused reemployment services ranged from 14 percentage points (for receipt of job referrals) to 26 percentage points (for receipt of testing to determine the jobs for which workers were best suited). The latter impact is large primarily because receipt of this service was relatively uncommon among comparisons.

**Table V-1: Impacts on Reemployment Services (Survey Data)**

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Received Any Reemployment Services | 94.4 | 77.0 | 17.4*** | 1.6 |
| Received Job-Focused Reemployment Services | | | | |
| Assistance searching for work | 68.7 | 56.6 | 12.1*** | 2.6 |
| Labor market information about occupations in demand in local area | 65.8 | 42.9 | 22.9*** | 2.4 |
| Referrals to jobs or employers | 53.9 | 40.1 | 13.7*** | 2.4 |
| Help with resume | 59.1 | 37.4 | 21.7*** | 2.3 |
| Tests to see what jobs qualified/suited for | 55.7 | 29.9 | 25.9*** | 2.4 |
| Received Career-Focused Reemployment Services | | | | |
| Information on education or job training programs | 81.2 | 56.3 | 24.9*** | 2.3 |
| Information on how to change careers | 63.4 | 45.0 | 18.4*** | 2.4 |
| Counseling on whether training is appropriate | 36.3 | 14.5 | 21.8*** | 1.8 |
| Counseling to select a training provider | 32.7 | 9.9 | 22.7*** | 1.7 |
| Average number of meetings with a counselor about training[a] | 4.2 | 3.8 | 0.4 | 0.6 |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]This is a conditional outcome, with the tabulation restricted to survey respondents who reported having received counseling to determine whether training was appropriate or which training provider to choose. Therefore, difference between outcomes for TAA participants and the comparison group is not an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

TAA's impacts on career-focused reemployment services were also sizable (Table V-1). For example, 56 percent of comparisons received information on education or job training programs, but 81 percent of TAA participants did so, a significant impact of 25 percentage points. TAA had nearly as large an impact on receipt of information on how to change careers, counseling on whether training would be appropriate, and guidance on choosing training providers. Counseling on training choices is the reemployment service that is the least common among TAA participants, with only about a third receiving counseling on whether training is appropriate or which provider to select. But it is considerably less common among comparisons, so the impact of TAA is sizable (more than 20 percentage points).

Workers who reported receiving counseling about training choices were also asked how many meetings they had with their counselors. Both TAA participants and their comparisons reported an average of about four meetings, and the small difference between the groups is not statistically significant.

Overall, these results indicate that TAA participation has large and statistically significant effects on the receipt of a broad range of reemployment services—services that might lead to quick employment or more thoughtful career planning.

## 2. Source of Services

Because the Trade Act of 2002 mandates that workers covered by petitions be provided with access to One-Stop core and intensive services, it is not surprising that the reemployment services just described were overwhelmingly accessed by TAA participants through the One-Stop system (Table V-2). Of those TAA participants who accessed reemployment services, 83 percent did so primarily at state unemployment or employment offices or at One-Stop Career Centers. However, among comparisons who accessed reemployment services, the percentage was nearly as large, at 81 percent. There is therefore no significant difference between the two groups in this regard and it seems clear that the One-Stop system serves as the main intake point for reemployment services for both TAA participants and comparisons.

However, TAA participants who received reemployment services were significantly more likely than comparisons who received these services to have relied primarily on schools, training providers, colleges, or universities for this assistance (8 percent versus 1 percent, respectively; Table V-2). As will be discussed in detail in Chapter VI, TAA significantly increased receipt of education and training, so no doubt participants' greater access of reemployment services through schools and other training providers reflects their more frequent training enrollment.

By contrast, TAA participants were less likely than comparisons to have relied on employers, the Internet, or placement agencies for reemployment services. However, even among comparisons, the reliance on these sources was slight, and the differences between TAA participants and comparisons, although statistically significant, were small.

**Table V-2: Differences in Locations Where the Majority of Reemployment Services Were Received (Survey Data)**

| | TAA Participants | Comparison Group | Difference | Standard Error |
|---|---|---|---|---|
| Location where Most Reemployment Services Were Received††† | | | | |
| State unemployment or employment office or One-Stop Career Center[a] | 83.3 | 80.7 | 2.6 | 1.9 |
| School, training provider, college, or university[a] | 7.9 | 1.3 | 6.6*** | 1.0 |
| Employer[a] | 3.8 | 8.8 | -5.0*** | 1.3 |
| Another government agency[a] | 1.7 | 0.8 | 0.8*** | 0.3 |
| Internet[a] | 1.3 | 4.3 | -3.0*** | 0.7 |
| Placement agency[a] | 0.6 | 2.3 | -1.7*** | 0.5 |
| Other[a] | 0.8 | 1.4 | -0.5 | 0.4 |
| Don't know[a] | 0.6 | 0.5 | 0.1 | 0.1 |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]This is a conditional outcome, with the tabulation restricted to those who accessed a reemployment service. Therefore, differences between outcomes for TAA participants and those for the comparison group are not impacts.

*/**/*** Effect of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

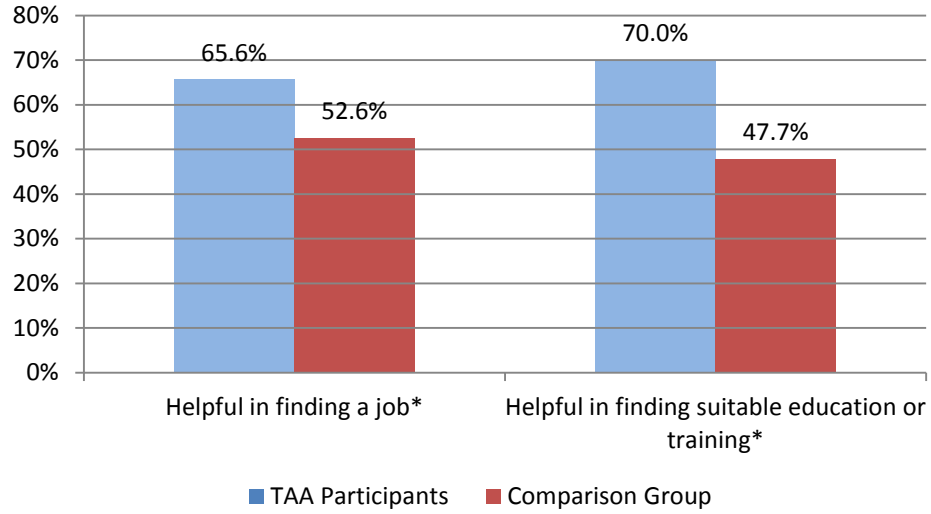
†/††/††† Effect of TAA on distribution of categories is statistically significant at the 0.10/0.05/0.01 level.

## 3. Satisfaction with Reemployment Services

Although TAA participants and comparisons primarily received reemployment services from the same sources (Table V-2), participants typically received a wider array of services than comparisons (Table V-1). This difference may give rise to varied assessments across the two groups of how useful the services were perceived to be.

Exhibit V-1 shows the percentage of TAA participants and comparisons who reported that the services they received were helpful to them in finding jobs or suitable education or training programs. With the analysis restricted to those who used reemployment services and found a job, the exhibit shows a statistically significant effect of TAA on the reported helpfulness of the services. Fifty-three percent of comparisons reported that the reemployment services were very or moderately helpful in their finding a job, while TAA participants reported a 66 percent helpfulness rating, a statistically significant impact of 13 percentage points. With the analysis restricted to those who had enrolled in education or training since their job loss, the estimated effect of TAA on the reported helpfulness of finding a suitable education or training program is 22 percentage points. For this sample, we found that 48 percent of comparisons but 70 percent of TAA participants reported that the reemployment services they received were helpful.

**Exhibit V-1: Differences in Perceptions of Helpfulness of Reemployment Services[a]**




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a] This is a conditional outcome, with the tabulation of helpfulness in finding a job restricted those who found jobs, and helpfulness in finding education or training restricted to those in education or training. Both tabulations are restricted to those who used reemployment services. Therefore, differences between outcomes for TAA participants and the comparison group are not impacts.

*/**/*** Effect of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## B. IMPACTS ON WIA ENROLLMENT

As we saw in Chapter IV, about 40 percent of TAA participants were enrolled in WIA. This finding is reflective of the emphasis that ETA places on linkages and coordination of TAA with its partners in the One-Stop system. However, comparison group members would have also been eligible for WIA services, at least those provided by the WIA Adult Program, and some might have received reemployment services, or even training services, from this source.[36]

[36] Practically speaking, there are no eligibility requirements for WIA Adult Program core services, and individuals can be given access to intensive or training services from this program if they show a need for further services to have their employment goals met. Eligibility for the WIA Dislocated Worker Program is restricted to dislocated workers, defined in section 101(9) of the legislation. Eligibility for the WIA Youth Program is restricted to those ages 14-21 who

Based on WIASRD data for the certified worker administrative records sample, Table V-3 shows TAA's impacts on enrollment in WIA. This tabulation treats a sample member as enrolled in WIA only if he or she appeared in the WIASRD files provided by the states, had a WIA participation date after or no more than 90 days before the UI trigger claim date, and (for reasons discussed in Chapter V of the MN report) was listed as having received a staff-assisted WIA service (as opposed to merely WIA self-services).

**Table V-3: Impacts on Enrollment in WIA Staff-Assisted Services (Administrative Data)**

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Enrolled in WIA | 38.3 | 4.0 | 34.3*** | 1.9 |
| Dislocated Worker Program (except NEG)[a] | 31.3 | 0.8 | 30.5*** | 2.2 |
| NEG[a] | 6.2 | 0.6 | 5.6*** | 0.8 |
| Adult Program[a] | 5.8 | 5.5 | 0.2 | 1.0 |
| Other or not specified[a] | 2.5 | 0.6 | 1.9*** | 0.2 |
| Highest Level of Service | | | | |
| Staff-assisted core services | 2.2 | 1.6 | 0.6 | 0.4 |
| Intensive services | 12.0 | 1.4 | 10.7*** | 1.9 |
| Training services | 24.1 | 1.0 | 23.1*** | 1.3 |
| Sample Size | 9,426 | 15,123 | | |

Source: Administrative data.

Notes: Treatment group weights account for sample design, and comparison group weights are constructed using the original matching triads. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

[a] The sum of the percents enrolled across these categories exceeds the total percent enrolled, because some individuals are enrolled in a program of more than one type.

TAA substantially increased the likelihood of WIA enrollment—only 4 percent of comparison-group members were enrolled in WIA, compared with 38 percent of TAA participants, for an impact of 34 percentage points (TableV-3). As we saw in Chapter IV, most TAA participants

(continued)

are low income and have at least one from a specified list of factors identifying them as at risk, although exceptions can be made.

enrolled in WIA were enrolled in the Dislocated Worker Program. Therefore, it is not surprising that the impacts of TAA were large and statistically significant for enrollment in this WIA program. However, TAA also had a statistically significant—albeit much smaller—impact on enrollment under a National Emergency Grant (NEG) and in other or unspecified WIA programs.[37] There was no impact of TAA on enrollment in the WIA Adult Program, and no TAA participants or comparisons were enrolled in the WIA Youth Program.

We also examined impacts on whether the sample members were enrolled in staff-assisted core services, intensive services, or training services. Table V-3 also shows these results, with each worker categorized according to the highest level of service he or she received. The TAA program's impacts on the receipt of WIA intensive and training services were positive and large. For instance, training was the highest level of service for 24 percent of participants, compared to only 1 percent for comparisons.

Note that an individual could be listed in the WIASRD as having received training even if WIA did not pay for the training, as long as the individual was receiving a WIA-funded service and the training was part of the individual's WIA service plan. Thus, the sizable proportion of TAA participants shown as being enrolled in WIA training likely indicates in many instances that TAA trainees were co-enrolled in WIA, with TAA providing funding for the training and WIA providing case management and other services designed to help the trainees successfully complete their programs and find employment.[38]

It is worth drawing attention to the seeming disconnect between the high proportions of TAA participants and comparisons who received reemployment services from the One-Stop system and the much smaller numbers enrolled in WIA. For example, we saw from Table V-1 that 77 percent of comparison group members received reemployment services and, of that group, 81 percent received their services through the One-Stop system (Table V-2). Yet Table V-3 shows that only 4 percent of comparisons were enrolled in WIA staff-assisted services. By implication, substantial numbers were accessing only One-Stop self-services or informational services, and were doing so perhaps as part of their registration for UI. Although there has been some concern that the remote filing of UI through call centers divorces workers from the One-Stop reemployment services they may need (USGAO 2005), this evidence suggests that the use of these services—at least self-services and information services—is widespread among the workers in our sample.

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[37] The WIASRD extracts from two of the states did not specify the WIA programs. This category also includes the small number identified as being enrolled in state incumbent worker programs.

[38] An alternative explanation is that some individuals began participating in WIA-funded training while waiting for petitions to be certified and TAA eligibility to be established. Once TAA eligibility is established, it is common for payment for any remaining training to then be supported by TAA (Dunham 2009).

## C. SUBGROUP DIFFERENCES

Different subsets of individuals may have different proclivities for seeking out reemployment services. For example, older workers who experience dislocation late in their careers may feel they need substantial assistance in identifying alternative job opportunities and, hence, turn to the One-Stop system or other sources for help; by contrast, younger workers may be less invested in previously established careers and more nimble in finding new employment on their own (Maestas and Li 2006). Similarly, an individual's level of education will be related to his or her marketability and, again, this may translate into a greater or lesser need to seek assistance with the job search. Accordingly, we look at TAA program impacts on the receipt of reemployment services and WIA enrollment according to workers' demographic and local area characteristics.

### 1. Differences in Impacts on Receipt of Reemployment Services

The TAA program's impacts on the receipt of reemployment services are large in magnitude and statistically significant for virtually every subgroup referenced in Table V-4. Thus, across all age groups, racial/ethnic categories, and educational groups, and for males and females, regardless of household composition or health status, TAA significantly increased the receipt of reemployment services.

Nonetheless, there are some subgroup differences. Most notably, the impacts of TAA declined sharply with workers' ages—the impact of the program was about 24 percentage points for those under age 40, but only 9 percentage points for those 60 or over. This pattern occurs because the usage of reemployment services was greater for older members of the comparison sample than it was for their younger counterparts. In contrast, among TAA participants, use of reemployment services generally declined with age.

There were also notable differences in impacts by workers' racial/ethnic composition. In particular, the impacts of TAA were smallest for blacks, but this is almost exclusively the result of the fact that members of the comparison sample who were black used reemployment services at a higher rate than comparisons in other racial/ethnic categories.

Exhibit V-2 shows that the use of reemployment services is the same, at 77 percent, for the comparison groups for the two TAA service subgroups, but that the use of reemployment services is greater for TAA trainees than it is for the TRA-only subgroup. Nonetheless, the impact of TAA is statistically significant for each.

## Table V-4: Impacts on Receipt of Any Reemployment Service, by Subgroups (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 94.4 | 77.0 | 17.4*** | 1.6 |
| Age† | | | | |
| 16 to 29 | 99.3 | 75.6 | 23.7*** | 4.0 |
| 30 to 39 | 95.1 | 70.3 | 24.8*** | 3.7 |
| 40 to 49 | 96.5 | 74.8 | 21.7*** | 2.5 |
| 50 to 59 | 92.2 | 76.3 | 15.9*** | 2.4 |
| 60 or over | 89.0 | 80.1 | 8.9*** | 3.1 |
| Race† | | | | |
| White | 96.1 | 76.2 | 19.9*** | 1.9 |
| Black | 91.1 | 85.2 | 5.8** | 2.9 |
| Hispanic | 91.3 | 67.6 | 23.7*** | 5.4 |
| Other | 91.1 | 64.5 | 26.6*** | 5.6 |
| Gender | | | | |
| Female | 93.3 | 73.8 | 19.5*** | 2.1 |
| Male | 95.5 | 79.4 | 16.1*** | 2.2 |
| Family Composition | | | | |
| Married w/kids | 96.5 | 75.3 | 21.2*** | 2.4 |
| Unmarried w/kids | 93.7 | 81.7 | 12.0*** | 3.0 |
| Married no kids | 92.6 | 76.3 | 16.3*** | 2.6 |
| Unmarried no kids | 94.6 | 76.1 | 18.5*** | 2.7 |
| Education | | | | |
| Less Than High School | 92.5 | 74.9 | 17.6*** | 2.9 |
| High School Diploma | 95.2 | 77.9 | 17.3*** | 1.9 |
| Some College | 93.1 | 79.1 | 14.0*** | 3.2 |
| Bachelor's or More | 96.7 | 73.9 | 22.8*** | 3.8 |
| Health Status at Job Loss | | | | |
| Good Health | 94.7 | 77.3 | 17.4*** | 1.7 |
| Fair or Poor Health | 92.7 | 78.8 | 13.9*** | 2.9 |
| Metropolitan Area | | | | |
| Non-Metropolitan Area | 94.6 | 76.3 | 18.3*** | 2.4 |
| Metropolitan Area | 94.2 | 77.8 | 16.4*** | 1.8 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 95.5 | 75.3 | 20.1*** | 3.0 |
| 4.4 to 5.1 | 93.4 | 72.8 | 20.6*** | 2.9 |
| 5.1 to 6.0 | 95.2 | 74.4 | 20.8*** | 2.6 |
| Greater than 6.0 | 93.5 | 79.8 | 13.7*** | 2.5 |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: The outcome for all rows is receipt of any reemployment service, as measured by the surveys. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

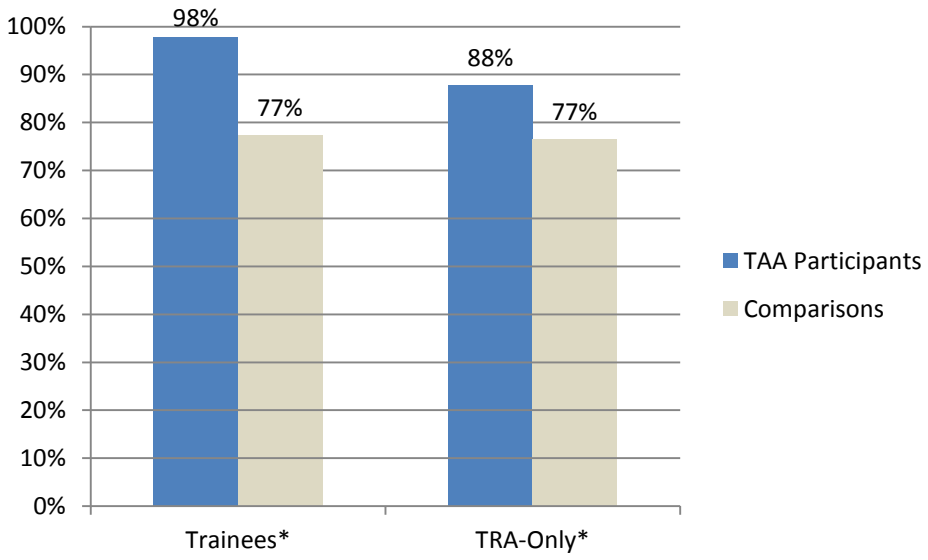
† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## 2. Differences in Impacts on Enrollment in WIA

Administrative data were used to estimate TAA's impacts on enrollment in WIA staff-assisted services across the more limited set of subgroups for whom administrative data were available. As Table V-5 shows, impacts across all of the subgroups are statistically significant and consistently large, in most cases exceeding 30 percentage points.

The large sample sizes afforded by the administrative data sample mean that subgroup differences in these analyses are generally statistically significant. Thus, we could determine that the impact of TAA declined with age, was lower for blacks than for other racial/ethnic groups, and was larger for females than males. These differences are largely due to variability in the enrollment of TAA participants in WIA, rather than because of differences in the enrollment of comparisons; in fact, the enrollment rate for comparisons is remarkably constant, hovering within a narrow range between 1 and 7 percent.

**Exhibit V-2: Impacts on Reemployment Services, by Service Subgroup (Survey Data)**




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

## Table V-5: Impacts on Enrollment in WIA, by Subgroups (Administrative Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 38.3 | 4.0 | 34.3*** | 1.9 |
| Age† | | | | |
| 16 to 29 | 44.5 | 6.8 | 37.7*** | 3.9 |
| 30 to 39 | 43.6 | 4.6 | 38.9*** | 2.2 |
| 40 to 49 | 41.0 | 4.3 | 36.8*** | 1.8 |
| 50 to 59 | 33.9 | 4.1 | 29.8*** | 2.3 |
| 60 or over | 26.8 | 5.6 | 21.3*** | 3.2 |
| Race† | | | | |
| White | 39.4 | 4.2 | 35.3*** | 2.3 |
| Black | 24.3 | 3.0 | 21.3*** | 2.1 |
| Hispanic | 53.8 | 7.0 | 46.7*** | 2.7 |
| Other | 44.1 | 6.7 | 37.4*** | 2.8 |
| Gender† | | | | |
| Female | 41.4 | 4.1 | 37.3*** | 2.6 |
| Male | 35.4 | 4.0 | 31.4*** | 1.6 |
| Metropolitan Area† | | | | |
| Non-Metropolitan Area | 35.7 | 1.3 | 34.4*** | 3.5 |
| Metropolitan Area | 40.1 | 5.8 | 34.2*** | 1.8 |

Source: Administrative data.

Notes: The outcome for all rows is enrollment in WIA, as evidenced by appearance in the WIASRD files with a staff-assisted service, with a date of WIA enrollment on or after the UI trigger claim date. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

# VI. IMPACTS ON EDUCATION AND TRAINING

Education and training play a central role in the TAA program. The program's ultimate goal is to ensure that trade-affected workers develop marketable skills that will enable them to find jobs. The Trade Act of 2002 emphasized that long-term training, the historical focus of the program, may not be the best route to suitable and rapid reemployment for all workers. Nevertheless, training in general persists as the benefit that attracts eligible workers to participate in TAA. Given the program's generous training benefit, we would expect that participation in TAA should increase enrollment in education and training programs and possibly increase educational attainment.

Chapter VI describes the education and training experiences of participants and comparisons and provides estimates of the impact of TAA on key training and education outcomes during the four years after workers' UI claims. The education and training experiences of the comparison group are the counterfactual for this study. Although comparisons were not eligible to receive TAA-funded training, they could still enroll in education and training programs, hypothetically participating in the same training programs as TAA participants. The comparisons' experiences are a benchmark that shows what education and training participants would have engaged in had they not been TAA eligible.

This chapter addresses four primary research questions:

- What amounts and types of education and training would TAA participants have received if they had not been eligible for the TAA program?
- Does the TAA program increase the receipt of education and training?
- Does the impact of the TAA program on receipt of education and training vary by the age or other characteristics of the worker?
- Does TAA participation influence educational attainment, as measured by the receipt of a GED, vocational certificate, or college degree?

We addressed these questions using data on the education and training experiences of sample members from the initial and follow-up surveys. We used information on dates of enrollment in education and training programs, the types of programs attended, time spent in academic classes and vocational training, certificates and degrees received, and the highest grades completed at the follow-up survey. Our analysis distinguished between academic classroom instruction and vocational training. Academic instruction includes three types of training: classes at community colleges, universities, and other settings for the purpose of improving reading, writing, or mathematics skills; obtaining a GED or high school diploma; and learning English as a second language. Vocational training was for specific jobs or occupations and might have been taken in any setting.

We found that the TAA program had a substantial impact on the amount of education and training received by participants. Nearly 66 percent received training compared to 27 percent of comparisons, a statistically significant impact of 39 percentage points. The average TAA participant spent about 8 times as many weeks in education and training as the average comparison group

member (49 weeks, compared to 6 weeks). TAA participants were significantly more likely to have received remedial education or non-occupational higher education, but the largest program impacts were on the receipt of occupational training and education.

Impacts on participation in education and training programs were largest during the first two years of the follow-up period, but persisted in the third and fourth years; in quarter 16, TAA participants were still significantly more likely to be enrolled in training (8 percent versus 3 percent).

Participation in education and training increased the educational attainment of TAA participants. More than 50 percent of participants received educational credentials or degrees, an impact of 30 percentage points. This increase in educational attainment was also evident in participants' reported highest levels of education. By the final interview, TAA participants were significantly more likely to report having received vocational certificates or associate's degrees, and were significantly less likely to report that a high school diploma was their highest level of education. In addition, TAA participation significantly increased the attainment of GEDs for workers without high school diplomas at program entry.

TAA substantially increased the receipt of training for participants of all ages. However, both the take-up of training and the impacts on training were typically larger for the younger participants than for the older ones (especially with respect to occupational training programs). TAA participants also received significantly more training than comparisons for other subgroups, but the pattern of impacts within subgroups varied somewhat. Impacts on training receipt were larger for females, for minority workers, for those without college degrees at program entry, and workers in households with children. Furthermore, impacts on training were smaller in areas with high unemployment rates, because comparisons were more likely to enroll in training in these areas.

The rest of this chapter provides details on these findings. The first section presents impact estimates on participation and time spent in education and training programs, and on the types of programs attended. In the second section, we present impact findings on educational attainment. Finally, we present impacts for key subgroups.

## A. IMPACTS ON PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS

This section compares TAA participants' and matched comparisons' participation in education and training programs during the 16 quarters after their UI claims. TAA participants are eligible to receive funding for one training plan, but the training plan may include a single course or a group of courses that allow an individual to meet a specific occupational goal. For example, an individual training plan could include an education component, like a GED program, and occupational training.

Given the generous TAA training benefit, we would expect large program impacts on the receipt of education and training services. We would expect these impacts to decline over time, although they might not be largest in the first quarters after the UI claim, because TAA participants need to become aware of the TAA program, apply, have their eligibility confirmed, find suitable education or training programs with available slots, and receive approval of their training plans, all before the TAA program will provide funding for training.

## 1. Impacts on Participation

The comparison sample faced the same labor market challenges and potential need for training as the TAA participants. As unemployed manufacturing workers, the majority of comparisons had only high school degrees and had recently lost jobs that paid wages higher than the local average (see Dolfin and Berk 2010). Although the comparisons were not eligible for TAA-funded training, they may still have enrolled in education or training programs. Unemployed workers not eligible for TAA may have turned to WIA for training funds, chosen to self-fund their training, or looked for other sources of training.

We found that, during the 16-quarter follow-up period, 27 percent of comparisons enrolled in training (Table VI-1), most of which occurred immediately after job loss. In the first quarter after the UI claim, 11 percent of comparisons were enrolled in training, and this declined in each subsequent quarter.

Despite this comparison group activity, TAA substantially increased participation rates in education and training programs (Table VI-1). Nearly 66 percent of participants received some education or training during the follow-up period (regardless of whether it was TAA funded or not), compared to 27 percent of comparisons (an impact of 39 percentage points). This large impact on training is not surprising. In a previous report examining reasons for participation and nonparticipation, we found that, by and large, TAA participants in our survey sample applied for the program because they were interested in training (Dolfin and Berk 2010). In fact, interest in training greatly exceeded interest in receiving TRA benefits (65 percent versus 26 percent), particularly among younger workers.

While TAA increased the receipt of training for participants of all ages, the take-up of training was highest for younger workers. More than 85 percent of TAA participants younger than 30 at the time of their UI claims received education or training (Exhibit VI-1). Among comparisons, training rates were also higher for younger workers, but TAA still increased training receipt by 39 percentage points for workers younger than 30, and 57 percentage points for workers aged 30 to 39. Even amongst the oldest workers, 28 percent of TAA participants aged 60 and older received some education or training, compared to 14 percent of comparisons.

As important as understanding who participated in training is knowing when this training took place (Exhibit VI-2 and Table VI-1). Comparison workers were most likely to be enrolled in training in the first quarter following job loss. TAA participants, on the other hand, took longer; training rates peaked in the third and fourth quarters following their UI claims. In the third quarter, about 47 percent of TAA participants were enrolled in training, compared to about 6 percent of comparisons—an impact of 42 percentage points. While training participation was greatest in the first year, one third of TAA participants were still enrolled in training at the end of the second year. By quarter 12, three years after their UI claims, about 16 percent of participants were enrolled, compared to about 3 percent of comparisons, yielding an impact of 12 percentage points. Even 16 quarters after their UI claims, TAA participants were still significantly more likely than comparisons to be enrolled in training (8 percent, compared to 3 percent).

## Table VI-1: TAA Program Impacts on Participation in Training and Education (Survey Data)

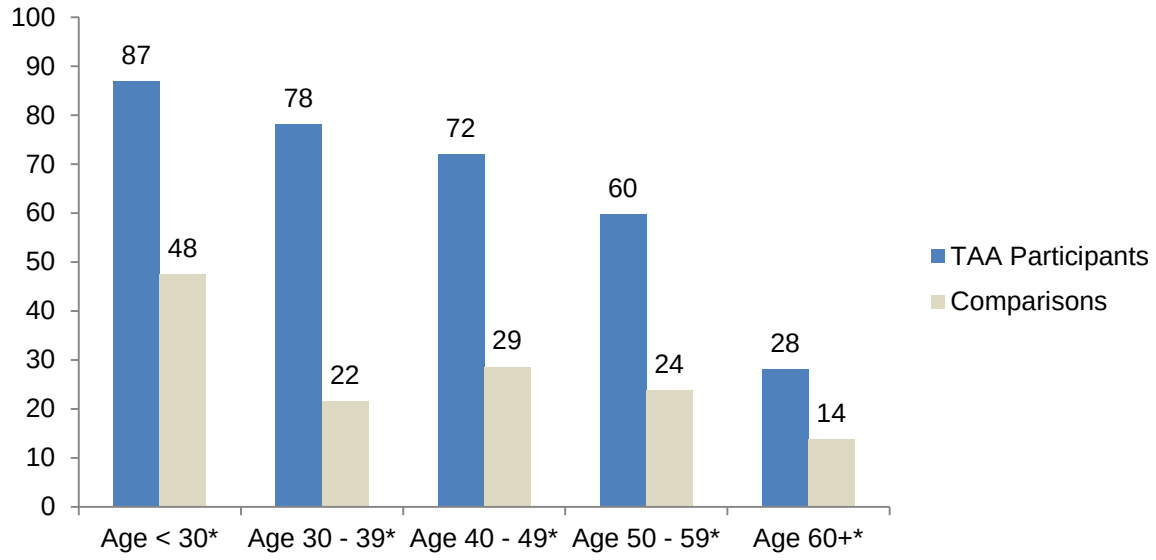
| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Percentage Ever Trained | 65.9 | 26.5 | 39.4*** | 2.5 |
| In Training | | | | |
| Quarter 1 | 25.0 | 11.2 | 13.8*** | 2.0 |
| Quarter 2 | 39.9 | 8.8 | 31.1*** | 1.9 |
| Quarter 3 | 47.4 | 5.5 | 41.9*** | 1.9 |
| Quarter 4 | 47.0 | 6.3 | 40.7*** | 2.2 |
| Quarter 5 | 45.2 | 5.6 | 39.6*** | 2.2 |
| Quarter 6 | 42.6 | 5.3 | 37.2*** | 2.0 |
| Quarter 7 | 38.0 | 4.2 | 33.8*** | 1.9 |
| Quarter 8 | 33.2 | 3.7 | 29.6*** | 1.8 |
| Quarter 9 | 28.2 | 3.8 | 24.3*** | 1.7 |
| Quarter 10 | 24.0 | 3.5 | 20.5*** | 1.4 |
| Quarter 11 | 19.0 | 3.0 | 16.0*** | 1.3 |
| Quarter 12 | 15.6 | 3.2 | 12.4*** | 1.1 |
| Quarter 13 | 13.4 | 3.7 | 9.6*** | 1.2 |
| Quarter 14 | 10.8 | 2.1 | 8.7*** | 1.1 |
| Quarter 15 | 9.4 | 1.6 | 7.8*** | 1.1 |
| Quarter 16 | 8.1 | 3.2 | 4.9*** | 1.0 |
| Sample Size, Quarters 1-12 | 2030 | 1664 | | |
| Sample Size, Quarters 13-16 | 1363 | 1121 | | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Exhibit VI-1: Participation in Education and Training, by Age at UI Claim

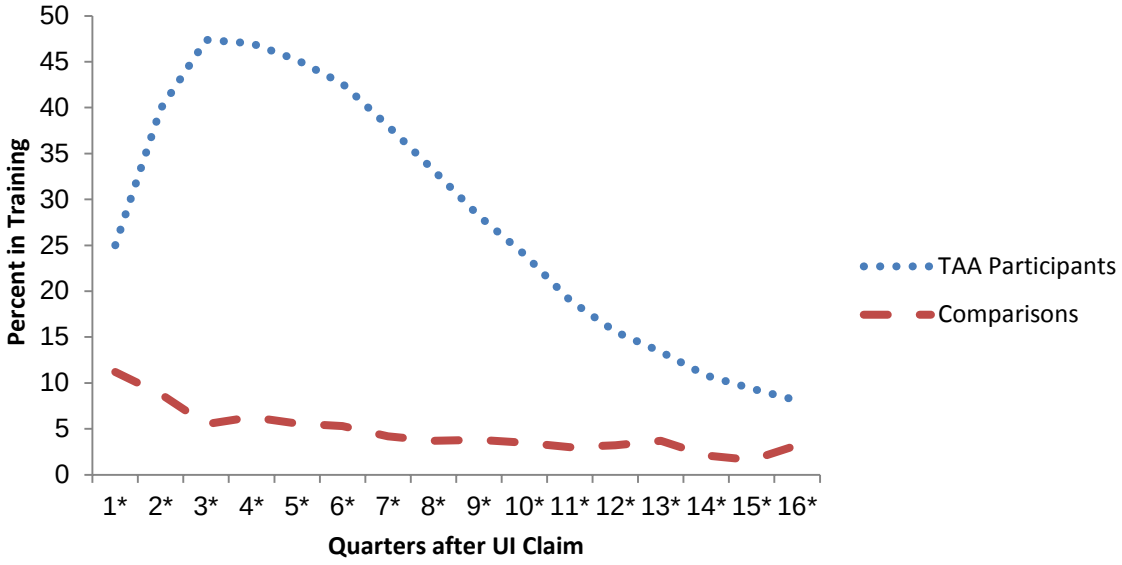



*Source*: Mathematica TAA Initial and Follow-up Surveys.

*Notes*: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

## Exhibit VI-2: Participation in Education and Training, by Quarters after UI Claim




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

The substantial impacts of TAA on enrollment in education and training for at least the first 16 quarters following UI claims are important. If individuals who have enrolled in training programs are less likely to be employed, we would anticipate that training could dampen the employment of TAA participants, even 16 quarters after the UI claim (although longer-term earnings gains might also be anticipated). Thus, these findings suggest that impacts on employment and earnings throughout the period could be affected by treatment-comparison differences in training and education enrollment.

## 2. Impacts on Time Spent in Education and Training Programs

The labor market implications of training will depend on the intensity of the training program. More time spent in training should increase the human capital value of the training, but it also increases the opportunity costs. We measure the time spent in education and training programs by the hours of training in each year following the job loss as well as the total weeks in training during the follow-up period.

In the first year after job loss, the matched comparisons received an average of 38 hours of training, approximately one week of full-time training (Table VI-2). In contrast, the TAA participants received an average of 373 hours of education or training (just over nine full-time weeks), an impact of 335 hours.

**Table VI-2: TAA Program Impacts on Time Spent in Education and Training (Survey Data)**

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Hours in Training | | | | |
| Quarters 1 – 4 | 373.0 | 37.7 | 335.3*** | 17.1 |
| Quarters 5 – 8 | 403.9 | 35.7 | 368.2*** | 20.5 |
| Quarters 9 – 12 | 194.5 | 17.6 | 177.0*** | 13.3 |
| Quarters 12 – 16 | 82.5 | 13.5 | 68.9*** | 9.4 |
| Total Weeks in Training | 48.9 | 6.2 | 42.8*** | 2.0 |
| Sample Size, Quarters 1-12 | 2030 | 1664 | | |
| Sample Size, Quarters 13-16 | 1363 | 1121 | | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

The intensity of training was higher in the second year following job loss. TAA participants spent an average of 404 hours in training, compared to 36 hours for the comparisons. While the average number of training hours declined in years three and four, the impacts on hours in training were still substantial. TAA participants received 177 more hours of training in year three and 69 more hours of training in year four.

During the entire four years following the job loss, the comparison group received an average of six weeks of education or training, while the average across all TAA participants was almost a full year (49 weeks). Thus, TAA participants experienced an additional 43 weeks of education and training (regardless of whether the training was funded by TAA or not). Among only those TAA participants who had ever engaged in some education or training, the average duration of training was 75 weeks, or almost 1.5 years (not shown).

## 3. Impacts on Types of Training Received

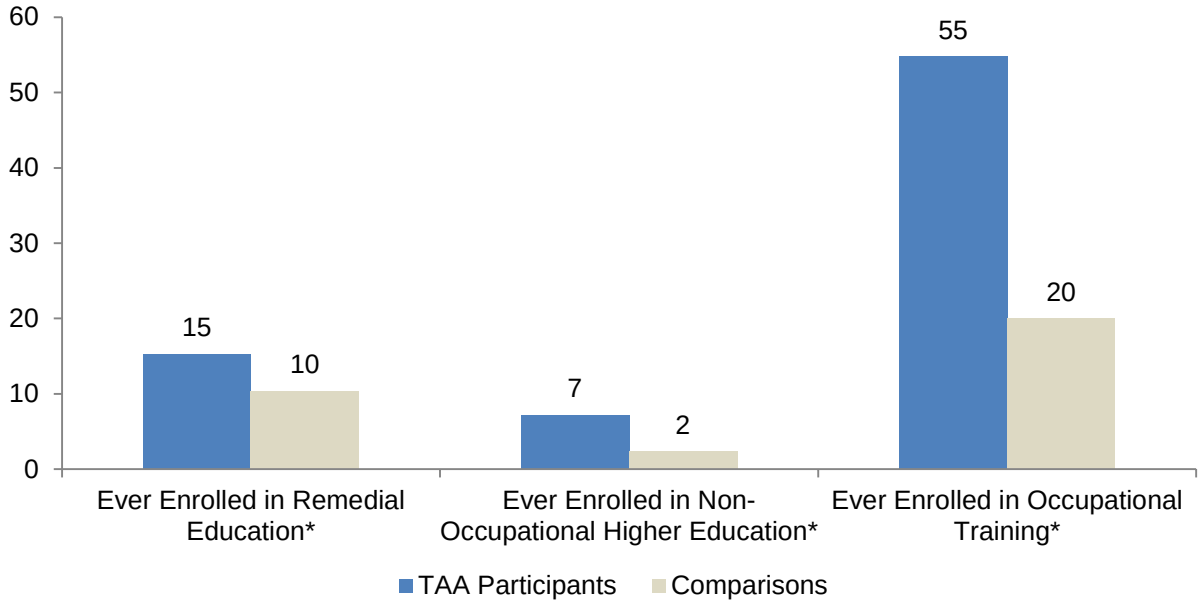
Enrollment in education or training includes a broad array of activities, and the type of training a worker chooses depends on his or her previous education and occupational goals. We classify education and training programs into three broad categories: (a) remedial education, (b) higher education, and (c) occupationally-focused training. The remedial education category includes adult basic education, GED classes, and English as a second language (ESL) courses. The higher education category includes all non-remedial education programs that survey respondents identified as focused on general education, rather than skills or occupations. Occupationally-focused training includes academic programs that were focused on particular occupations.

TAA participants are eligible to receive funding for one training plan, but the training plan may include a single course or a group of courses that allow an individual to meet a specific occupational goal. For example, an individual training plan could include an education component, like a GED program, and occupational training. TAA participants may have also enrolled in additional education or training programs not covered by their training plans. As such, a worker may have reported more than one type of training during the survey follow-up period.

As discussed above, 27 percent of comparisons enrolled in some type of education or training program in the four years following job loss. We find that 10 percent enrolled in remedial education programs, 2 percent enrolled in non-occupational higher education programs, and 20 percent enrolled in occupationally focused programs (Exhibit VI-3). Despite these notable levels of participation among the comparison group, we find that the TAA program did, in fact, significantly increase participation in all categories of training. Fifteen percent of TAA participants received remedial education, an impact of 5 percentage points; seven percent of TAA participants enrolled in non-occupationally focused higher education programs, an impact of 5 percentage points. Most of these TAA participants enrolled in two-year programs at community colleges, although some enrolled in graduate programs.

The most sizable impact of the TAA program was on participation in occupational training. While this was also the most common choice for the comparison group, the TAA program still increased the share of unemployed workers receiving this type of training by 35 percentage points. As shown in Table VI-3, TAA participants were more likely to enroll in programs focused on office and administrative support occupations; installation, maintenance, and repair occupations; and healthcare practitioner and technical occupations. TAA participants were less likely than the

**Exhibit VI-3: TAA Program Impacts on Type of Education and Training**




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

comparisons who enrolled in occupational training programs to enroll in programs focused on transportation and material moving occupations; business and financial operations occupations; and sales and related occupations.

There were some noticeable differences between age groups in the impacts of TAA on the types of training that participants received (Exhibit VI-4 and Table VI-4). TAA increased the receipt of remedial education most significantly for workers aged 50 to 59 (a 10 percentage point impact). In contrast, the TAA program had no impact on the receipt of remedial education for those younger than 30 at the time of job loss. Instead, it had a very sizable impact on the percentage of these youngest workers who enrolled in non-occupational higher education programs (a 21 percentage point impact). Given that the academic higher education programs are likely relatively long programs, it is not surprising that the enrollment in these programs was highest for young workers.

In general, TAA's impact on the receipt of occupational training declined with the age of the worker. It was largest for workers ages 30 to 39, whose enrollment rate exceeded the comparisons by 55 percentage points. The impacts for older workers were still large and significant, but declined for each subsequent age group. The impact for the youngest workers was 34 percentage points; as discussed, many in this cohort enrolled in academic programs instead of occupational training.

## Table VI-3: Differences in Occupational Focus of Training Programs (Survey Data)

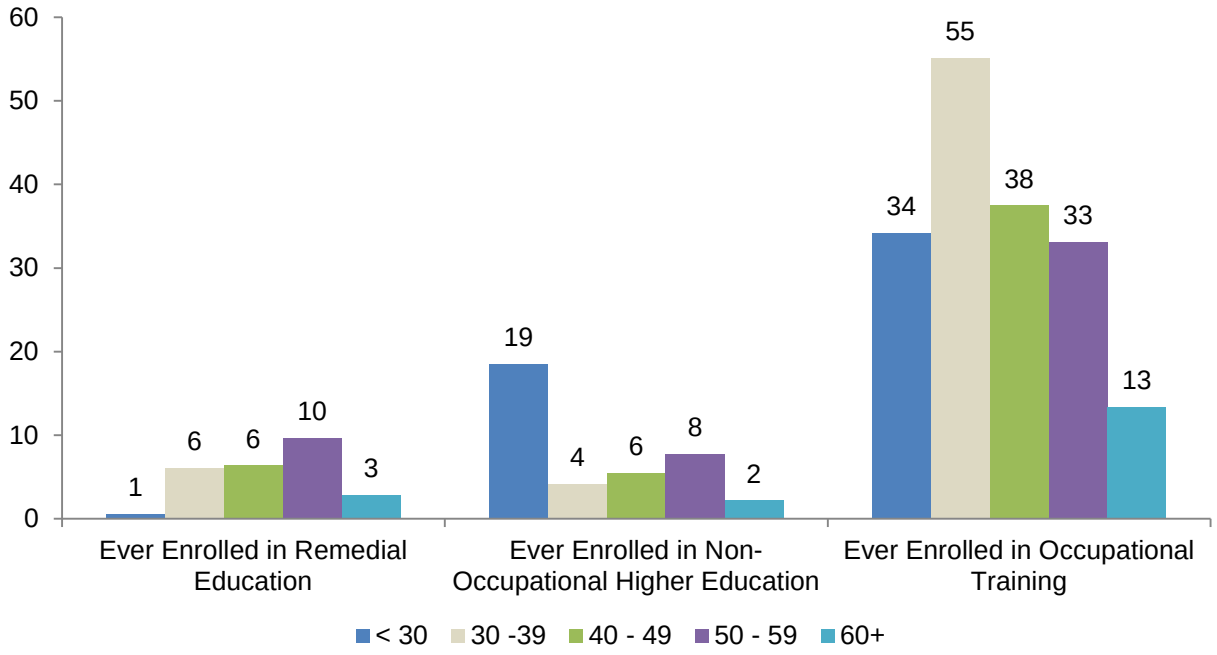
| | TAA Participants | Comparison Group | Difference | Standard Error |
|---|---|---|---|---|
| Occupational Focus of Training Program | | | | |
| Office and Administrative Support | 14.7 | 10.5 | 4.1* | 2.2 |
| Healthcare Support | 12.2 | 10.2 | 2.1 | 1.6 |
| Installation, Maintenance, and Repair | 10.1 | 4.4 | 5.7*** | 2.1 |
| Healthcare Practitioners and Technical | 8.6 | 4.8 | 3.9*** | 1.3 |
| Production | 5.0 | 6.3 | -1.4 | 1.5 |
| Transportation and Material Moving | 4.8 | 8.4 | -3.6** | 1.6 |
| Education, Training, and Library | 2.2 | 1.4 | 0.8 | 0.9 |
| Personal Care and Service | 2.1 | 1.5 | 0.6 | 1.2 |
| Architecture and Engineering | 1.5 | 2.1 | -0.6 | 1.0 |
| Building and Grounds Cleaning and Maintenance | 1.0 | 1.0 | 0.0 | 0.1 |
| Business and Financial Operations | 1.0 | 7.3 | -6.3* | 3.3 |
| Management | 0.8 | 0.6 | 0.2** | 0.1 |
| Protective Services | 0.8 | 0.7 | 0.1* | 0.0 |
| Sales and Related | 0.4 | 5.8 | -5.3*** | 0.9 |
| Food Preparation and Service | 0.4 | 0.7 | -0.3** | 0.1 |
| Construction and Extraction | 0.0 | 0.0 | 0.0 | 0.8 |
| Other | 31.6 | 34.5 | -2.9 | 3.1 |
| Sample Size | 1323 | 361 | | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: The treatment and comparison samples are limited to individuals who enrolled in occupationally focused training programs after their trigger job losses. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

**Exhibit VI-4: Impacts on Type of Education and Training, by Age at UI Claim**




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

## B. IMPACTS ON EDUCATIONAL ATTAINMENT

TAA substantially increased the overall time participants devoted to education and training programs. It is important to determine, however, whether this increase in effort led to training program completion, the receipt of certificates and credentials, and/or higher levels of educational attainment. In an earlier descriptive study of the training experiences of TAA participants, we found that they had high rates of training program completion (Berk 2011). As such, it is not surprising that the impacts of TAA on training program completion closely mirror the impacts on training participation (Table VI-5). Fifty-five percent of TAA participants completed training programs, compared to 24 percent of comparisons (Exhibit VI-5).

While effective education and training programs should increase the human capital of participants—especially when the programs are completed—workers also rely on credentials or degrees to communicate their new skills to employers. The TAA program increased the receipt of degrees and credentials by 30 percentage points (51 percent for participants, compared to 21 percent for comparisons; Table VI-5). The rate of credential receipt was very similar to the training program completion rate, suggesting that almost all training program completers received a degree or credential.

## Table VI-4: Impacts on Types of Education and Training, by Age at UI Claim (Survey Data)

**Ever Enrolled in Remedial Education**

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 15.3 | 10.8 | 4.5*** | 1.6 |
| Age | | | | |
| 16 to 29 | 10.9 | 10.3 | 0.6 | 4.2 |
| 30 to 39 | 14.3 | 8.2 | 6.1** | 2.4 |
| 40 to 49 | 14.8 | 8.4 | 6.4*** | 2.1 |
| 50 to 59 | 18.0 | 8.3 | 9.7*** | 2.3 |
| 60 or over | 14.9 | 12.0 | 2.9 | 3.6 |

**Ever Enrolled in Non-Occupational Higher Education**

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 7.8 | 2.5 | 5.3*** | 0.9 |
| Age† | | | | |
| 16 to 29 | 18.5 | 0.1 | 18.5*** | 4.0 |
| 30 to 39 | 11.3 | 7.1 | 4.2 | 2.8 |
| 40 to 49 | 7.8 | 2.3 | 5.5*** | 1.9 |
| 50 to 59 | 8.9 | 1.1 | 7.8*** | 2.0 |
| 60 or over | 2.3 | 0.0 | 2.2* | 1.2 |

**Ever Enrolled in Occupational Training**

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 54.8 | 20.1 | 34.7*** | 2.1 |
| Age† | | | | |
| 16 to 29 | 73.7 | 39.5 | 34.2*** | 9.4 |
| 30 to 39 | 65.4 | 10.3 | 55.1*** | 4.1 |
| 40 to 49 | 61.8 | 24.3 | 37.5*** | 3.4 |
| 50 to 59 | 47.9 | 14.8 | 33.1*** | 2.9 |
| 60 or over | 21.0 | 7.6 | 13.4*** | 4.6 |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Table VI-5: TAA Program Impacts on Training Completion and Receipt of Credentials (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Completed an Education or Training Program | 54.9 | 24.1 | 30.7*** | 2.7 |
| Received a Certificate or Degree | 50.9 | 20.9 | 30.0*** | 2.7 |
| Obtained a GED[a] | 20.7 | 4.4 | 16.3*** | 3.8 |
| Sample Size | 2054 | 1796 | | |

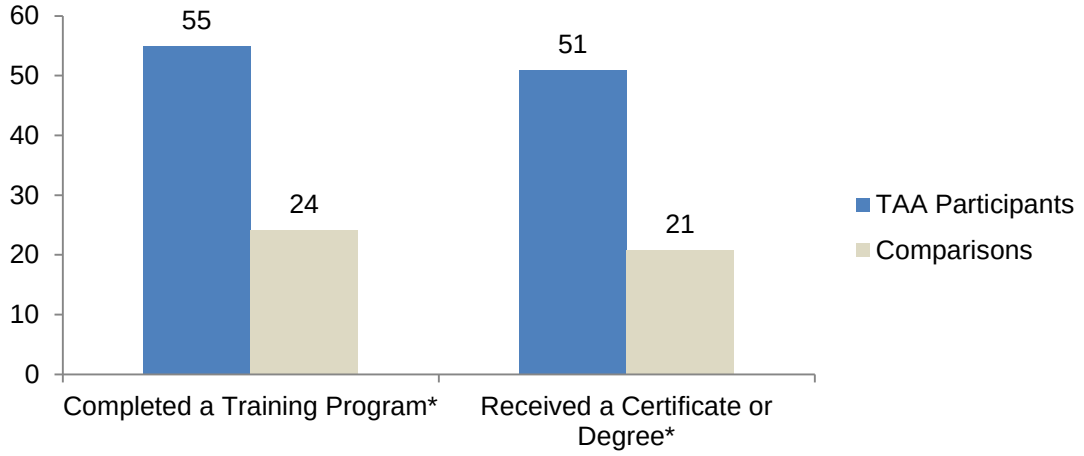
Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a] Sample is limited to workers who reported that they had not completed high school or obtained a GED at the time of the initial survey.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Exhibit VI-5: TAA Program Impacts on Completion of Training and Receipt of Education and Training Credentials




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

At the time of job loss, there was no significant difference between the educational attainment of TAA participants and comparisons. However, by the final interview, this had changed (Table VI-6). At the follow-up interview, only one third of TAA participants reported that a high school degree was their highest level of education, compared to more than half of comparisons, an impact of -18 percentage points. The TAA program increased the percentage of participants that had received vocational certificates by 5 percentage points and the percentage that received associate's degrees by 13 percentage points.

**Table VI-6: TAA Program Impacts on Highest Level of Education Completed (Survey Data)**

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Highest Level of Education at Final Interview | | | | |
| Did Not Complete High School | 8.2 | 10.1 | -1.9* | 1.0 |
| GED | 8.5 | 7.6 | 0.9 | 1.3 |
| High School Graduate | 33.7 | 51.6 | -17.9*** | 2.3 |
| Vocational Certificate | 13.6 | 8.3 | 5.3*** | 1.5 |
| Some College | 6.7 | 6.5 | 0.2 | 1.0 |
| Associates Degree | 21.5 | 8.9 | 12.6*** | 1.8 |
| Bachelors Degree or More | 7.8 | 7.0 | 0.8 | 0.6 |
| Sample Size | 1842 | 1600 | | |

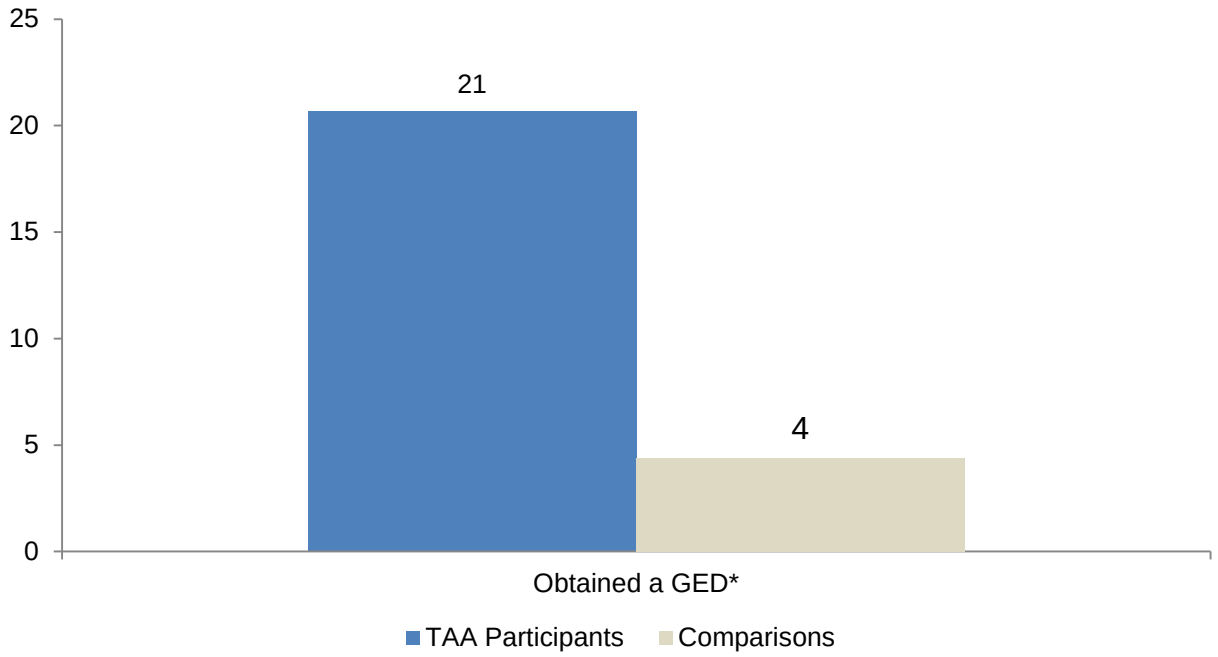
Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

The TAA program provides funding for remedial education including GED and ESL programs. For TAA participants without high school diplomas or English fluency, completing an educational training program may be a prerequisite for enrolling in occupational skills programs. The 2002 amendments facilitated this training by allowing participants to receive TRA benefits for a longer period of time if they enrolled in remedial training. Among comparisons who had not completed high school, 4 percent obtained their GEDs between their trigger job losses and final interviews (Exhibit VI-6). In contrast, almost 21 percent of TAA participants who initially lacked high school diplomas obtained their GEDs, an impact of 16 percentage points.

**Exhibit VI-6: TAA Program Impacts on GED Attainment**




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Sample is limited to workers without a high school diploma at job loss. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

GED = General Educational Development

## C. SUBGROUP DIFFERENCES

In many cases, there may be reason to expect that the impact of the TAA program on education and training would be different for certain groups of individuals. This section examines whether three key training outcomes— participation in training, total weeks in training, and receipt of a degree or credential—differed significantly across subgroups defined by demographic and local area characteristics.

The decision to enroll in training is likely dependent on an individual's background characteristics, skills, labor market opportunities, and beliefs about the potential to gain from training. All else equal, one would expect higher rates of training enrollment for workers with fewer outside labor market opportunities. One would also expect higher rates of training enrollment for workers with more to gain from training, for example those who would have trouble finding employment without additional education or training.

Across all subgroups, TAA participants received significantly more training than comparisons did (Table VI-7). However, the pattern of the training impacts varied somewhat within subgroups. As expected, younger workers (in both the participant and comparison samples) had high training

rates, arguably because they had the longest horizon during which to benefit from the investment in training. Moreover, as discussed earlier, TAA's impact on the receipt of training was larger for younger TAA participants than for older ones. TAA increased receipt of training by 39 percentage points for workers younger than 30, and 57 percentage points for workers aged 30 to 39. However, the impact remained large even for those 60 and older (14 percentage points)—28 percent of participants in this group enrolled in a training program.

The pattern of impacts on receipt of training also varied somewhat by the race and ethnicity of workers. Specifically, the impacts were smaller for whites than Blacks, Hispanics, or workers of other races. Among TAA participants as well as their comparisons, Hispanics had the highest rates of training. Eighty-two percent of Hispanic participants and 35 percent of Hispanic comparisons enrolled in training, compared to 63 percent of white participants and 28 percent of white comparisons.

The TAA program had larger impacts on the training rates of workers from households with children, which might be explained by the fact that workers in households with children are younger than the average TAA worker. The impacts on training did not differ significantly by the workers' gender, education levels, or health status at job loss.

We observed differences in training impacts based on the geographic locations of the workers. We examined subgroup differences based on workers' residence in metropolitan areas and the local unemployment rate at the time of job loss. These local area characteristics may affect the training decisions of both TAA participants and comparisons, because of a different supply of training providers or different levels of demand for workers with various credentials. Workers in areas with low unemployment rates could perceive less need for retraining than workers in areas with higher unemployment rates.

We found that the TAA program had a more significant impact on training for workers living in metropolitan areas than in nonmetropolitan areas. In metropolitan areas, the larger TAA impact was driven both by the higher training rates among TAA participants and the lower training rates among matched comparisons. This result may be somewhat surprising because workers who lose jobs in rural areas may have fewer employment opportunities in their existing occupations or industries, and may therefore be in greater need of retraining than those who lose jobs in metropolitan areas. On the other hand, the supply of training providers and demand for higher-skilled workers may be greater in metropolitan areas.

We also observed variation in the impact of the TAA program on training rates across local areas with different levels of unemployment. Understanding how training rates vary with local economic conditions may provide some insight into potential changes in the overall impact of the TAA program under different macroeconomic conditions. In fact, training rates were smaller in high unemployment areas than in other areas (22 percentage points, compared to 45 percentage points). This occurred because, although the training rate of TAA participants was relatively constant across low and high unemployment areas, the training rate of the comparisons increased with the local unemployment rate. In local areas with unemployment rates less than 4.4 percent, 22 percent of

## Table VI-7: TAA Program Impacts on Receipt of Education or Training, by Subgroups (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 65.9 | 26.5 | 39.4*** | 2.5 |
| Age† | | | | |
| 16 to 29 | 86.9 | 47.5 | 39.4*** | 7.8 |
| 30 to 39 | 78.1 | 21.6 | 56.6*** | 5.0 |
| 40 to 49 | 72.0 | 28.6 | 43.4*** | 3.6 |
| 50 to 59 | 59.7 | 23.8 | 36.0*** | 3.8 |
| 60 or over | 28.1 | 13.8 | 14.4*** | 5.2 |
| Race† | | | | |
| White | 62.9 | 28.2 | 34.7*** | 3.0 |
| Black | 69.0 | 20.7 | 48.3*** | 5.2 |
| Hispanic | 82.2 | 35.4 | 46.7*** | 7.5 |
| Other | 70.4 | 23.5 | 47.0*** | 4.9 |
| Gender | | | | |
| Female | 66.7 | 29.0 | 37.7*** | 3.3 |
| Male | 65.0 | 24.5 | 40.5*** | 2.9 |
| Family Composition† | | | | |
| Married with kids | 75.2 | 25.6 | 49.7*** | 4.0 |
| Unmarried with kids | 75.7 | 29.2 | 46.5*** | 4.9 |
| Married no kids | 56.2 | 30.8 | 25.4*** | 4.4 |
| Unmarried no kids | 59.9 | 23.2 | 36.6*** | 3.6 |
| Education | | | | |
| Less Than High School | 61.4 | 18.3 | 43.1*** | 5.5 |
| High School Diploma | 66.2 | 26.7 | 39.6*** | 2.9 |
| Some College | 69.4 | 38.6 | 30.8*** | 4.5 |
| Bachelors or More | 64.3 | 53.8 | 10.4 | 8.6 |
| Health Status at Job Loss | | | | |
| Good Health | 67.7 | 26.8 | 41.0*** | 2.9 |
| Fair or Poor Health | 58.8 | 23.6 | 35.2*** | 4.3 |
| Metropolitan Area† | | | | |
| Non-Metropolitan Area | 62.8 | 34.3 | 28.6*** | 3.7 |
| Metropolitan Area | 67.8 | 23.3 | 44.5*** | 2.9 |
| Local Unemployment Rate at UI Claim† | | | | |
| Less than 4.4 | 66.0 | 21.7 | 44.3*** | 4.9 |
| 4.4 to 5.1 | 67.1 | 24.9 | 42.2*** | 4.6 |
| 5.1 to 6.0 | 68.9 | 21.7 | 47.2*** | 3.9 |
| Greater than 6.0 | 61.5 | 39.5 | 22.0*** | 4.8 |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: The outcome for all rows is participation in an education or training program within the first four years after the job loss. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

comparisons enrolled in training, whereas in local areas with unemployment rates of 6.6 percent or greater, almost twice as many comparisons enrolled in training (40 percent). The training decisions of comparisons appear to be more responsive to local economic conditions than the training decisions of TAA participants.

The subgroup differences in the impacts on total weeks of training generally mirror the subgroup differences in training receipt, although there are a few exceptions (Table VI-8). Again, the impacts on time in training were greater for younger workers, minority workers, and workers in households with children, and the impacts did not differ by education level.

While we did not find any gender differences in impact on training receipt, there was a significant difference in the impact on weeks in training (45 percentage points for females, compared to 37 percentage points for males). This can be traced to the fact that female TAA participants received 10 weeks more of training than male TAA participants, while female comparisons only received 2 more weeks of training than male comparisons. In contrast, while the TAA program had a larger impact on the receipt of training for workers in metropolitan areas, there were no subgroup differences in the total weeks of training received. The subgroup findings by local unemployment rate followed the findings for receipt of training; the impacts on total weeks of training were smallest for workers living in local areas with high unemployment rates.

Subgroup findings on the receipt of degrees and credentials followed the subgroup findings on the receipt of training (Table VI-9). The most noticeable deviations related to race and ethnicity and to workers' health status at job loss. Specifically, while there were significant racial and ethnic differences with respect to the receipt of training—with smaller impacts for white TAA participants—there were no significant differences in impacts on the attainment of educational credentials. We also found that while there were no significant differences in the impact on training receipt for workers in good health compared to those in poor health, the impact on the receipt of training credentials was smaller for less healthy workers.

## Table VI-8: TAA Program Impacts on Weeks of Education or Training (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 48.9 | 6.2 | 42.8*** | 2.0 |
| Age† | | | | |
| 16 to 29 | 73.9 | 11.8 | 62.1*** | 6.6 |
| 30 to 39 | 62.1 | 4.0 | 58.1*** | 3.5 |
| 40 to 49 | 54.0 | 9.4 | 44.5*** | 3.5 |
| 50 to 59 | 39.7 | 6.2 | 33.5*** | 2.4 |
| 60 or over | 18.2 | 2.8 | 15.4*** | 2.9 |
| Race† | | | | |
| White | 46.3 | 7.4 | 39.0*** | 2.3 |
| Black | 53.8 | 6.2 | 47.6*** | 4.4 |
| Hispanic | 63.7 | 3.7 | 60.0*** | 7.3 |
| Other | 42.0 | 3.5 | 38.4*** | 5.5 |
| Gender† | | | | |
| Female | 53.8 | 8.8 | 45.1*** | 2.7 |
| Male | 43.8 | 6.6 | 37.2*** | 2.3 |
| Family Composition† | | | | |
| Married with kids | 57.5 | 8.6 | 48.8*** | 3.5 |
| Unmarried with kids | 58.6 | 11.9 | 46.7*** | 4.0 |
| Married no kids | 41.3 | 8.4 | 32.9*** | 3.0 |
| Unmarried no kids | 41.3 | 3.9 | 37.3*** | 2.9 |
| Education | | | | |
| Less Than High School | 44.6 | 8.5 | 36.1*** | 3.5 |
| High School Diploma | 49.2 | 7.2 | 42.0*** | 2.4 |
| Some College | 54.3 | 16.4 | 38.0*** | 3.7 |
| Bachelor's or More | 37.7 | 18.7 | 19.0** | 7.5 |
| Health Status at Job Loss | | | | |
| Good Health | 49.3 | 6.1 | 43.2*** | 2.0 |
| Fair or Poor Health | 47.2 | 4.1 | 43.1*** | 4.0 |
| Metropolitan Area | | | | |
| Non-Metropolitan Area | 48.5 | 6.0 | 42.5*** | 2.3 |
| Metropolitan Area | 49.8 | 8.7 | 41.1*** | 2.8 |
| Local Unemployment Rate at UI Claim† | | | | |
| Less than 4.4 | 48.0 | 6.3 | 41.7*** | 4.1 |
| 4.4 to 5.1 | 48.9 | 4.6 | 44.3*** | 3.6 |
| 5.1 to 6.0 | 53.4 | 7.5 | 45.9*** | 3.2 |
| Greater than 6.0 | 44.7 | 10.7 | 34.1*** | 3.1 |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: The outcome for all rows is weeks of education or training during the first four years after the job loss. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table VI-9: TAA Program Impacts on Receipt of Certificate or Degree (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 50.9 | 20.6 | 30.3*** | 2.7 |
| Age† | | | | |
| 16 to 29 | 70.9 | 45.2 | 25.8*** | 9.6 |
| 30 to 39 | 61.8 | 14.1 | 47.7*** | 4.5 |
| 40 to 49 | 57.1 | 25.5 | 31.6*** | 3.7 |
| 50 to 59 | 43.5 | 18.4 | 25.2*** | 3.2 |
| 60 or over | 20.4 | 7.9 | 12.5*** | 4.7 |
| Race | | | | |
| White | 51.0 | 20.4 | 30.6*** | 3.1 |
| Black | 46.8 | 15.8 | 31.0*** | 4.6 |
| Hispanic | 62.1 | 26.8 | 35.3*** | 6.1 |
| Other | 53.5 | 9.6 | 43.9*** | 5.7 |
| Gender | | | | |
| Female | 51.4 | 21.3 | 30.1*** | 3.5 |
| Male | 50.4 | 21.5 | 28.9*** | 2.8 |
| Family Composition† | | | | |
| Married with kids | 61.7 | 22.0 | 39.6*** | 4.0 |
| Unmarried with kids | 55.6 | 18.8 | 36.8*** | 4.9 |
| Married no kids | 42.9 | 25.2 | 17.8*** | 3.3 |
| Unmarried no kids | 44.8 | 14.9 | 29.9*** | 3.0 |
| Education | | | | |
| Less Than High School | 44.6 | 8.5 | 36.1*** | 3.5 |
| High School Diploma | 53.4 | 19.4 | 34.0*** | 3.0 |
| Some College | 55.6 | 31.7 | 23.8*** | 4.6 |
| Bachelors or More | 46.2 | 42.0 | 4.3 | 8.8 |
| Health Status at Job Loss† | | | | |
| Good Health | 53.6 | 20.4 | 33.2*** | 2.9 |
| Fair or Poor Health | 41.2 | 19.3 | 21.9*** | 3.5 |
| Metropolitan Area | | | | |
| Non-Metropolitan Area | 49.4 | 24.5 | 24.9*** | 3.3 |
| Metropolitan Area | 51.9 | 20.4 | 31.5*** | 3.0 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 50.3 | 17.6 | 32.7*** | 5.5 |
| 4.4 to 5.1 | 52.3 | 18.5 | 33.9*** | 4.1 |
| 5.1 to 6.0 | 52.2 | 17.0 | 35.1*** | 3.9 |
| Greater than 6.0 | 48.7 | 30.7 | 18.0*** | 3.9 |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: The outcome for all rows is receipt of a certificate or degree. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

# VII. IMPACTS ON EMPLOYMENT AND EARNINGS

Chapters V and VI showed that TAA participation leads to large impacts on the receipt of reemployment services, time spent in education and training programs, and educational attainment. Thus, TAA could increase participants' long-run productivity, marketability, and employability, as measured by increases in their labor force participation and earnings. The analysis of post-program employment and earnings impacts for the full sample of participants and comparisons comprises the *confirmatory* analysis for the evaluation.

About two thirds of TAA participants in our sample received training, and the average trainee spent about 1.5 years in training. Thus, the full-sample impacts on labor market outcomes will largely be driven by impacts for the trainees. For these participants, we anticipate that TAA will reduce employment and earnings during the period of training, because it is likely that some of these workers would have held jobs if TAA training and TRA benefits had not been an option. However, as TAA trainees leave their training programs, we expect their employment and earnings to rise after a period of adjustment. In light of the variation in the duration of training programs, it is difficult to predict how long it will take for positive employment and earnings gains to emerge. However, based on findings from the preceding chapters, we know that the first two years of follow-up were largely an in-program period for many trainees, and years 3 and 4 were largely a post-program period.

The full-sample impacts will also be influenced by the impacts for TAA participants who do not receive training—TRA-only workers. The expected pattern of impacts for these participants is less clear than for the trainees. On the one hand, TAA-induced increases in the receipt of reemployment services offered in One-Stop Career Centers and elsewhere could increase the reemployment rates of TRA-only workers soon after job loss. Furthermore, the receipt of TRA benefits could allow these workers extra time to search for suitable jobs. However, the offer of TRA benefits could induce some of these customers to extend their unemployment spells and exhaust their UI benefits without increased job search effort, which could lead to short- and, perhaps, longer-term earnings reductions.

This chapter compares the overall employment and earnings experiences of participants and comparisons in the four years after job loss, and addresses the following research questions:

- To what extent does TAA affect the employment and earnings of program participants and their engagement in either training or employment?
- Do employment-related impacts vary by the age of the worker and other key subgroups defined by demographic and local area characteristics at program entry?
- Do employment-related impacts differ for participants who enrolled in TAA-funded training and TRA-only participants? Is WIA co-enrollment associated with earnings impacts? What are the labor market impacts for ATAA recipients?
- Do key impact results differ if one estimates them using administrative rather than survey data or using different samples or matching methods?

These questions were primarily addressed using the certified worker follow-up survey sample and interview data. In the sensitivity analysis, however, we also estimated earnings impacts using UI wage records and the TRA-beneficiary and certified worker administrative records samples.

We found that TAA participation leads to a tradeoff between participation in training and employment. During the period of training, the labor market outcomes for TAA participants were significantly worse than for similar workers who were not eligible for TAA. As participants finished training and returned to work, the employment gap between the participants and the comparisons narrowed, but did not completely close. In the final year of the follow-up period, there was no significant difference in weeks of employment for TAA participants and comparisons. However, while the gap in earnings decreased over time, TAA participants still earned about $3,300 less on average than comparisons in the final follow-up year (a statistically significant difference).

These patterns of findings were particularly strong for younger TAA participants, the group with the most positive training impacts. Among all age groups, these younger workers had the largest negative employment and earnings impacts during the first two years of the follow-up period, but their impacts became statistically insignificant starting in quarter 10. By way of contrast, the earnings impacts for the older age groups remained negative and statistically significant throughout the follow-up period, especially for those 60 and older. The negative earnings impacts for the older workers were partly due to higher retirement rates for older participants than their comparisons (19 percent versus 14 percent for those age 50 and older).

While the results for the service receipt subgroups are only suggestive, we found that by the end of the follow-up period, labor market impacts were more favorable for the trainees than TRA-only participants. The impact on average weeks worked in year 4 was not statistically significant for the trainees, but remained negative and significant for the TRA-only participants. Furthermore, the impact on year 4 earnings was less negative for the trainees, although still statistically significant. Program effects did not differ by WIA co-enrollment status for the full sample of participants or trainees. We did find, however, that the receipt of ATAA was associated with higher rates of employment and no negative program effects on earnings, but there is reason to be cautious in interpreting these findings.

We found significant subgroup differences in labor market impacts based on the workers' ages, race and ethnicity, health status, and local area characteristics. Although, we did not find differences for subgroups defined by gender, family composition, or education level, the main impact story is consistent for all groups—significantly lower rates of employment and earnings throughout the follow-up period, but a closing of the gap over time.

We conducted a series of sensitivity tests to examine the robustness of the main confirmatory impact findings using the full sample of participants. Across all permutations, we found the employment and earnings impact estimates to be robust to alternative data sources, analysis samples, and analytic methods.

The results indicate that the participants were catching up to the comparisons in terms of their labor market earnings, but they had not yet closed the gap by the end of the four-year observation period. Because many TAA participants enroll in training programs for a considerable amount of time, the four-year follow-up period may not be long enough to evaluate the full returns of the TAA program on labor market activity. Furthermore, the latter part of follow-up period was a period of

economic recession, which may have influenced the impact findings. Longer-term follow-up may be necessary to draw firm conclusions about program effects.

The rest of this chapter provides details on these findings. The first section presents impact estimates on employment and earnings for the full survey sample and by the age of the worker. In the second section, we present impact findings for the TAA program service subgroups. In the third section, we test the sensitivity of the employment and earnings impacts. In the fourth section, we assess the effects of the recession on the impact estimates. Finally, we present impacts for other key subgroups defined by participants' demographic and local area characteristics.

## A. IMPACTS ON EMPLOYMENT AND EARNINGS

This section compares the overall employment and earnings experiences of TAA participants and matched comparisons in the follow-up survey sample during the 16 quarters after job loss. We examine impacts by quarter and year after job loss, and over the entire four-year follow-up period. To help place these impact findings into perspective, we begin this section with a discussion of the impact findings on TAA participants' engagement in any productive activity, as measured by *either* being employed or enrolled in an education or training program.

### 1. Overall Impacts on Productive Activity

The estimated quarterly impacts on being either employed or in training—that is, engagement in any productive activity—were small throughout the four-year follow-up period (Exhibit VII-1) and Table VII-1). These impacts were small and negative (but statistically significant) in the first year following job loss, which included the transition period when TAA participants applied for services and selected training providers, but were not statistically significant in subsequent quarters.

The reason for the latter finding is that the positive estimated impacts on training participation that were presented in Chapter V were largely *offset* by the negative estimated impacts on employment (see Exhibit VII-1). Thus, the evidence strongly suggests that, for many TAA participants, training is a substitute for employment. This finding is not surprising given that two thirds of TAA participants in our sample took up the offer of TAA-funded training. The remainder of this section provides more detail on the impact findings on employment and earnings.

### 2. Overall Impacts on Employment and Earnings

The unemployed workers in the comparison group returned to work fairly soon after they were laid off from their trigger jobs. More than one third of comparisons were employed by the end of the first quarter, and 70 percent were employed in the fourth quarter (Table VII-2). On average, comparison workers were employed for 26 weeks during the first follow-up year. After quarter 4, the employment rates for these workers remained fairly steady, climbing to 75 percent before falling back to 69 percent in quarter 16 (perhaps due to the recession that started in December 2007).

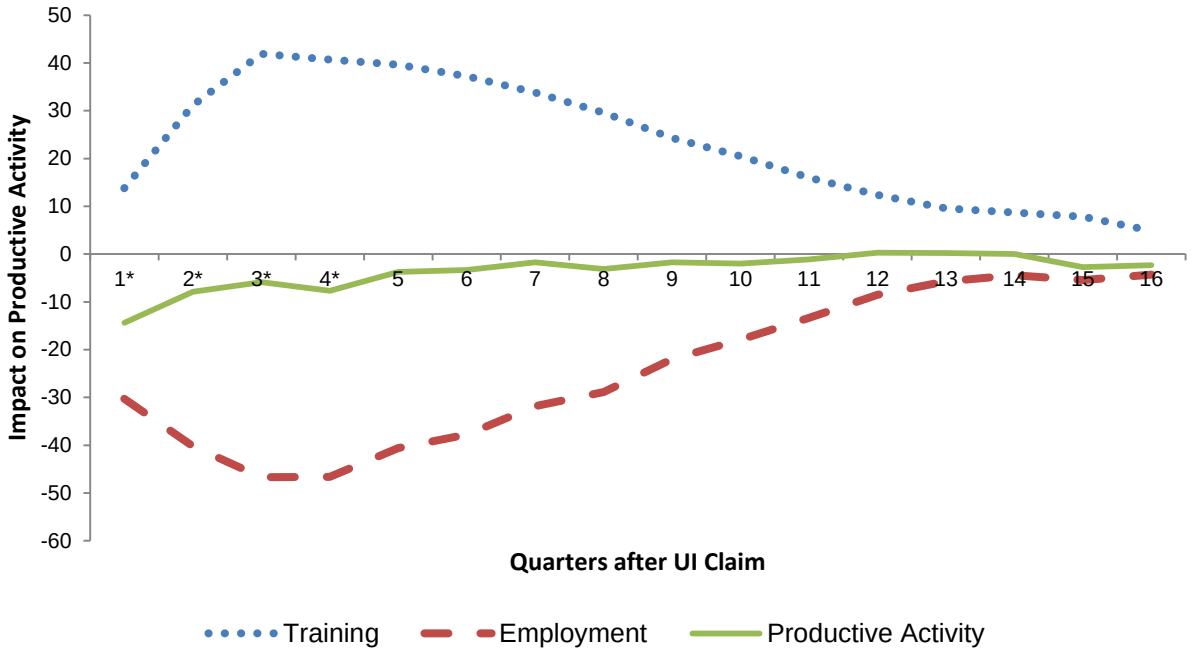
The return to employment was much more gradual for TAA participants (Table VII-2). In the first two years following job loss, the TAA participants were substantially less likely to be employed than the comparisons. Four quarters after job loss, fewer than one fourth of TAA participants were employed, compared to 70 percent of comparisons, a statistically significant impact of -47

percentage points. During the next three years, the employment rate of TAA participants steadily increased. In quarter 12, 60 percent of TAA participants were employed, compared to 68 percent of comparisons, a statistically significant impact of -8 percentage points. By the final quarter, the employment gap had closed to a statistically significant -4 percentage points.

The closing of the treatment-comparison gap in labor market participation was particularly evident in total weeks worked. In the first year following job loss, TAA participants reported an average of 6 weeks of employment, compared to 26 weeks for comparisons (Exhibit VII-2 and Table VII-2). By the final year, the treatment-control difference in weeks worked was not statistically significant (an average of 33 weeks for participants, compared to an average of 35 weeks for comparisons).

By the end of 16 quarters following job loss, 79 percent of TAA participants had held at least one job, compared to 87 percent of comparisons (a statistically significant impact of -8.5 percentage points; Table VII-2). TAA had no impact on the average number of jobs held by TAA participants (1.7 jobs for both participants and comparisons), or on the distribution of the number of jobs that were held among those with at least one job. About one quarter of employed sample members in each research condition held at least 3 jobs during the four-year follow-up period.

**Exhibit VII-1: Impacts on Productive Activity**




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

## Table VII-1: Impacts on Productive Activity (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Productive Activity | | | | |
| Quarter 1 | 30.8 | 45.2 | -14.4*** | 2.4 |
| Quarter 2 | 46.4 | 54.3 | -7.9*** | 2.5 |
| Quarter 3 | 57.7 | 63.6 | -5.9** | 2.4 |
| Quarter 4 | 63.9 | 71.6 | -7.7** | 2.2 |
| Quarter 5 | 69.2 | 72.9 | -3.7 | 2.3 |
| Quarter 6 | 71.4 | 74.7 | -3.3 | 2.1 |
| Quarter 7 | 72.1 | 73.8 | -1.7 | 2.1 |
| Quarter 8 | 71.9 | 75.0 | -3.1 | 1.9 |
| Quarter 9 | 71.2 | 72.9 | -1.7 | 1.9 |
| Quarter 10 | 69.6 | 71.6 | -2.0 | 2.1 |
| Quarter 11 | 68.2 | 69.3 | -1.1 | 2.3 |
| Quarter 12 | 67.8 | 67.5 | 0.3 | 2.3 |
| Quarter 13 | 68.4 | 68.3 | 0.2 | 2.4 |
| Quarter 14 | 68.4 | 68.3 | 0.0 | 2.3 |
| Quarter 15 | 66.9 | 69.5 | -2.7 | 2.4 |
| Quarter 16 | 66.7 | 69.0 | -2.3 | 2.4 |
| Sample Size, Quarters 1-12 | 1,989 | 1,622 | | |
| Sample Size, Quarters 13-16 | 1,340 | 1,089 | | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Table VII-2: Impacts on Employment (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Employed | | | | |
| Quarter 1 | 7.3 | 37.5 | -30.3*** | 2.4 |
| Quarter 2 | 9.5 | 49.7 | -40.2*** | 2.4 |
| Quarter 3 | 15.3 | 62.1 | -46.7*** | 2.4 |
| Quarter 4 | 23.5 | 70.1 | -46.6*** | 2.4 |
| Quarter 5 | 30.8 | 71.4 | -40.6*** | 2.4 |
| Quarter 6 | 36.8 | 74.6 | -37.8*** | 2.5 |
| Quarter 7 | 41.8 | 73.6 | -31.8*** | 2.5 |
| Quarter 8 | 46.2 | 75.1 | -28.9*** | 2.3 |
| Quarter 9 | 51.4 | 73.3 | -22.0*** | 2.3 |
| Quarter 10 | 54.0 | 71.9 | -17.9*** | 2.3 |
| Quarter 11 | 57.1 | 70.4 | -13.3*** | 2.4 |
| Quarter 12 | 59.5 | 68.0 | -8.5*** | 2.5 |
| Quarter 13 | 62.4 | 68.0 | -5.7** | 2.7 |
| Quarter 14 | 64.5 | 68.9 | -4.4* | 2.6 |
| Quarter 15 | 64.4 | 69.8 | -5.4** | 2.5 |
| Quarter 16 | 64.7 | 69.0 | -4.3* | 2.4 |
| Weeks of Employment | | | | |
| Quarters 1 – 4 | 6.0 | 25.5 | -19.4*** | 0.9 |
| Quarters 5 – 8 | 18.9 | 37.8 | -18.9*** | 1.2 |
| Quarters 9 – 12 | 28.1 | 37.1 | -9.0*** | 1.1 |
| Quarters 13 – 16 | 33.0 | 35.0 | -2.0 | 1.3 |
| Percentage Ever Employed | 78.9 | 87.3 | -8.5*** | 1.9 |
| Average Number of Jobs | 1.7 | 1.7 | -0.1 | 0.1 |
| Number of Jobs for Those Ever Employed[a] | | | | |
| 1 | 42.5 | 49.7 | -7.2** | 3.2 |
| 2 | 31.6 | 25.9 | 5.6* | 3.0 |
| 3 | 14.0 | 12.7 | 1.2 | 1.9 |
| 4 or more | 12.0 | 11.6 | 0.4 | 2.0 |
| Sample Size, Quarters 1-12 | 1,989 | 1,622 | | |
| Sample Size, Quarters 13-16 | 1,340 | 1,089 | | |

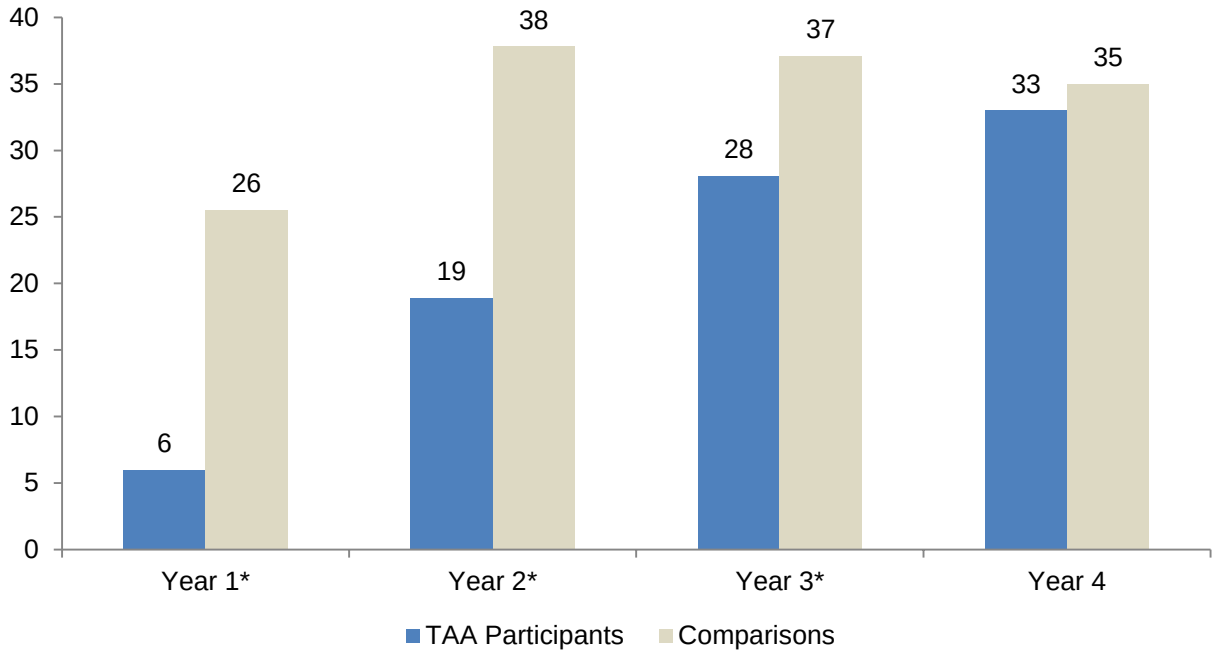
Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a] This value is conditional on having a least one job, so the difference cannot be interpreted as an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Exhibit VII-2: Impacts on Weeks of Employment




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

Not surprisingly, we found a similar pattern of program impacts for earnings as for employment: participants earned significantly less than comparisons throughout the follow-up period, but the gap closed over time, although not completely (Table VII-3). When enrollment in training was at its peak during years 1 and 2, average quarterly earnings were about $3,000 to $4,000 lower for TAA participants than comparisons (Table VII-3). The earnings gap declined as TAA participants reentered the labor market, but the negative impacts remained statistically significant. In the final year of the follow-up period, the average participant earned about $3,300 less than the average comparison, and in the final quarter, TAA participants earned $761 less. Over the entire 16-quarter follow-up period, the average participant earned about $37,100 less than the average comparison.

## Table VII-3: Impacts on Earnings (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Quarterly Earnings (2006$) | | | | |
| Quarter 1 | 348 | 1,964 | -1,616*** | 172 |
| Quarter 2 | 549 | 3,838 | -3,289*** | 254 |
| Quarter 3 | 848 | 4,732 | -3,885*** | 236 |
| Quarter 4 | 1,335 | 5,353 | -4,018*** | 230 |
| Quarter 5 | 1,855 | 5,589 | -3,734*** | 232 |
| Quarter 6 | 2,325 | 5,730 | -3,406*** | 236 |
| Quarter 7 | 2,615 | 5,712 | -3,097*** | 239 |
| Quarter 8 | 2,876 | 5,657 | -2,781*** | 231 |
| Quarter 9* | 3,147 | 5,586 | -2,439*** | 228 |
| Quarter 10 | 3,335 | 5,371 | -2,036*** | 228 |
| Quarter 11 | 3,523 | 5,306 | -1,783*** | 227 |
| Quarter 12 | 3,685 | 5,173 | -1,489*** | 231 |
| Quarter 13 | 3,902 | 4,866 | -964*** | 245 |
| Quarter 14 | 3,980 | 4,821 | -841*** | 235 |
| Quarter 15 | 4,070 | 4,852 | -782*** | 225 |
| Quarter 16 | 4,077 | 4,839 | -761*** | 228 |
| Annual Earnings (2006$) | | | | |
| Quarters 1 – 4 | 3,053 | 15,728 | -12,674*** | 795 |
| Quarters 5 – 8 | 9,574 | 22,561 | -12,987*** | 909 |
| Quarters 9 – 12 | 13,548 | 20,999 | -7,451*** | 871 |
| Quarters 13 – 16 | 15,917 | 19,189 | -3,273*** | 883 |
| Total Earnings, Quarters 1 – 16 | 42,939 | 80,072 | -37,133*** | 3,289 |
| Sample Size, Quarters 1 – 12 | 1,989 | 1,622 | | |
| Sample Size, Quarters 13 – 16 | 1,340 | 1,089 | | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## 3. Impacts by Age

As discussed in Chapter VI, TAA substantially increased the receipt of training for participants of all ages. However, both the take-up of training and the impacts on training were typically larger for younger participants than for older ones (especially for participation in occupational training programs). Consequently, one might expect the pattern of program impacts on labor market outcomes to vary by age. Indeed, we found this to be the case, with significant differences in the size and overall time profile of impacts by age (Table VII-4 and Table VII-5 and Exhibit VII-3). The younger workers, the group with the largest positive training impacts, typically had the largest negative employment and earnings impacts during years 1 and 2, but the impacts became statistically insignificant starting in quarter 10. By way of contrast, the earnings impacts for the older age groups remained negative and significant throughout the follow-up period, especially for the oldest participants.

More specifically, in quarter 5, 85 percent of comparison workers younger than 30 were employed, compared to 22 percent of TAA participants, an impact of -63 percentage points. This was a period when many young participants were enrolled in training and forgoing earnings (the young comparisons earned an average of about $7,700 per quarter during this time). The impact on the quarter 5 employment rate, however, was typically about -50 percentage points for all other age groups, and was -27 percentage points for those 60 and older. The youngest participants quickly closed the gap relative to their comparisons, and by quarter 10, there were no significant impacts on employment rates or average earnings for this group. For those 60 and older, however, the gap in employment and earnings remained sizable throughout the follow-up period (the earnings gap was -$1,100 in quarter 1 and -$1,347 in quarter 16). The earnings impacts for those aged 30 to 39, 40 to 49, and 50 to 59 became less negative over time, but they all remained statistically significant in quarter 16.

## 4. Impacts on Labor Market Status at the Follow-Up Interview

At the time of the follow-up interview, 65 percent of TAA participants and 67 percent of comparisons were employed (Table VII-6). While there was no significant difference in the percentage of TAA participants and comparisons who reported current employment, we found that TAA participants were significantly more likely to be in training or to be retired (impacts of 1.8 and 2.6 percentage points, respectively; Table VII-6). The training finding is consistent with the Chapter VI findings of positive impacts on training in quarter 16. The retirement finding is also consistent with our finding of large negative impacts of the TAA program on the employment and earnings of older workers throughout the follow-up period.

We would expect the impacts on retirement to be concentrated among older workers. We examined impacts on current employment status for workers younger than 50 and for workers 50 and older. For prime-age workers, the TAA program increased the likelihood that a participant's primary activity at the follow-up interview was training (4.1 percent compared to 2.2 percent; Table VII-7). Older participants were also more likely to be in training than their comparisons (1.4 percent, compared to 0.8 percent), but the biggest age differences in impacts was on retirement rates. The TAA program increased the share of participants age 50 and older who were retired by a statistically significant 5 percentage points (18.9 percent, compared to 13.5 percent for comparisons),

## Table VII-4: Impacts on Employment, by Age at UI Claim (Survey Data)

| | Age 29 and Under | | Age 30 - 39 | | Age 40 - 49 | | Age 50 – 59 | | Age 60 and Older | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Comparison Group | Impact | Comparison Group | Impact | Comparison Group | Impact | Comparison Group | Impact | Comparison Group | Impact |
| Employed | | | | | | | | | | |
| Quarter 1† | 47.0 | -38.6*** | 64.0 | -57.9*** | 33.5 | -25.4*** | 25.2 | -18.1*** | 30.9 | -25.3*** |
| Quarter 2† | 60.2 | -54.1*** | 62.5 | -53.9*** | 53.4 | -41.9*** | 44.1 | -34.1*** | 40.6 | -35.3*** |
| Quarter 3† | 63.9 | -54.5*** | 75.9 | -61.8*** | 65.3 | -47.1*** | 54.0 | -36.6*** | 44.1 | -36.6*** |
| Quarter 4† | 82.3 | -62.9*** | 75.3 | -51.5*** | 73.9 | -46.0*** | 64.2 | -39.7*** | 46.8 | -36.2*** |
| Quarter 5† | 84.8 | -62.5*** | 82.0 | -48.7*** | 74.9 | -41.7*** | 66.3 | -32.5*** | 42.5 | -27.2*** |
| Quarter 6 | 87.7 | -58.0*** | 82.5 | -42.2*** | 79.6 | -37.7*** | 64.2 | -26.7*** | 43.9 | -26.8*** |
| Quarter 7† | 84.1 | -44.6*** | 84.2 | -37.6*** | 80.6 | -35.6*** | 68.1 | -24.7*** | 45.7 | -25.1*** |
| Quarter 8† | 78.4 | -30.1*** | 87.0 | -35.0*** | 84.9 | -34.7*** | 68.4 | -21.8*** | 44.4 | -22.7*** |
| Quarter 9† | 76.5 | -25.6*** | 84.9 | -26.7*** | 82.2 | -23.9*** | 65.6 | -14.5*** | 43.5 | -23.2*** |
| Quarter 10† | 56.5 | 0.1 | 77.9 | -15.7*** | 86.3 | -26.9*** | 64.4 | -10.3*** | 41.5 | -19.6*** |
| Quarter 11† | 55.3 | 12.0 | 77.5 | -11.8*** | 84.7 | -22.6*** | 63.9 | -7.6** | 35.3 | -13.6* |
| Quarter 12† | 60.4 | 12.7 | 75.5 | -3.6 | 84.9 | -19.5*** | 57.5 | -2.2 | 36.0 | -14.0** |
| Quarter 13† | 69.5 | 2.5 | 82.0 | -5.5 | 78.8 | -7.5*** | 60.6 | -3.6 | 41.9 | -21.2** |
| Quarter 14† | 65.3 | 13.8 | 78.7 | -0.7 | 79.3 | -6.6** | 64.0 | -3.7 | 37.6 | -17.4** |
| Quarter15† | 69.9 | 8.0 | 81.9 | -1.2 | 74.6 | -1.8 | 64.1 | -5.2 | 32.3 | -13.7* |
| Quarter 16 | 69.7 | 1.7 | 82.7 | -0.9 | 81.6 | -7.7*** | 63.8 | -4.8 | 32.4 | -15.3* |
| Weeks of Employment | | | | | | | | | | |
| Year 1† | 28.1 | -23.3*** | 33.5 | -28.2*** | 26.3 | -18.9*** | 21.7 | -15.4*** | 19.1 | -15.9*** |
| Year 2† | 44.6 | -28.0*** | 43.6 | -22.9*** | 40.9 | -19.9*** | 34.0 | -14.3*** | 22.7 | -13.8*** |
| Year 3 | 32.0 | -1.2 | 43.1 | -10.7 | 44.1 | -12.7** | 33.4 | -5.8 | 20.9 | -10.2*** |
| Year 4 | 34.7 | 4.7 | 43.5 | -3.0 | 40.6 | -3.2 | 32.7 | -2.3 | 20.9 | -10.4** |
| Sample Size, Quarters 1-12 | 144 | 135 | 367 | 272 | 620 | 520 | 624 | 508 | 228 | 170 |
| Sample Size, Quarters 13-16 | 99 | 84 | 224 | 175 | 423 | 361 | 414 | 348 | 147 | 101 |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Differences in impacts of TAA among subgroups are statistically significant at the 0.05 level.

## Table VII-5: Impacts on Earnings, by Age at UI Claim (Survey Data)

| | Age 29 and Under | | Age 30 - 39 | | Age 40 - 49 | | Age 50 – 59 | | Age 60 and Older | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Comparison Group | Impact | Comparison Group | Impact | Comparison Group | Impact | Comparison Group | Impact | Comparison Group | Impact |
| Earnings (2006$) | | | | | | | | | | |
| Quarter 1† | 2,172 | -1,692*** | 3,076 | -2,776*** | 1,754 | -1,340*** | 1,672 | -1,393*** | 1,288 | -1,093*** |
| Quarter 2† | 4,092 | -3,623*** | 5,064 | -4,582*** | 3,715 | -3,095*** | 3,328 | -2,773*** | 1,981 | -1,625*** |
| Quarter 3† | 4,861 | -4,465*** | 5,772 | -5,097*** | 4,833 | -3,830*** | 4,308 | -3,294*** | 2,588 | -2,150*** |
| Quarter 4† | 5,901 | -5,098*** | 6,086 | -4,853*** | 5,400 | -3,882*** | 4,911 | -3,343*** | 2,854 | -2,226*** |
| Quarter 5† | 7,722 | -6,318*** | 6,021 | -3,995*** | 5,719 | -3,783*** | 5,146 | -2,986*** | 2,832 | -2,126*** |
| Quarter 6† | 8,052 | -6,223*** | 6,427 | -3,795*** | 5,841 | -3,292*** | 5,137 | -2,647*** | 2,759 | -1,925*** |
| Quarter 7 | 7,727 | -5,527*** | 6,420 | -3,440*** | 5,948 | -3,049*** | 5,154 | -2,418*** | 2,677 | -1,751*** |
| Quarter 8† | 6,727 | -4,081*** | 6,356 | -2,955*** | 6,022 | -2,892*** | 5,212 | -2,256*** | 2,501 | -1,514*** |
| Quarter 9† | 6,775 | -3,676*** | 6,342 | -2,376*** | 6,049 | -2,499*** | 4,992 | -1,998*** | 2,413 | -1,567*** |
| Quarter 10† | 4,551 | -1,214 | 5,975 | -1,810*** | 6,114 | -2,344*** | 4,686 | -1,493*** | 2,252 | -1,385*** |
| Quarter 11† | 4,017 | -148 | 5,827 | -1,378*** | 6,196 | -2,330*** | 4,721 | -1,324*** | 2,042 | -1,183*** |
| Quarter 12† | 3,719 | 793 | 5,606 | -743* | 6,130 | -2,089*** | 4,554 | -1,211*** | 2,046 | -1,221*** |
| Quarter 13† | 5,122 | -431 | 6,632 | -1,914*** | 5,797 | -1,227*** | 4,238 | -810** | 2,491 | -1,711*** |
| Quarter 14† | 4,998 | -209 | 6,405 | -1,571*** | 5,765 | -1,092*** | 4,179 | -677** | 2,376 | -1,613*** |
| Quarter15† | 5,142 | -18 | 6,502 | -1,557*** | 5,717 | -895*** | 4,079 | -589* | 2,224 | -1,445** |
| Quarter 16 | 5,113 | -264 | 6,466 | -1,369*** | 5,852 | -936*** | 4,045 | -619* | 1,989 | -1,347** |
| Annual Earnings | | | | | | | | | | |
| Year 1† | 20,178 | -17,516*** | 20,091 | -17,429*** | 15,438 | -11,915*** | 14,133 | -10,743*** | 8,613 | -7,011*** |
| Year 2 † | 25,091 | -14,177*** | 25,119 | -14,205*** | 23,411 | -13,025*** | 20,533 | -10,287*** | 10,705 | -7,280*** |
| Year 3 | 23,648 | -6,468*** | 23,634 | -6,453*** | 23,745 | -8,649*** | 18,658 | -5,856*** | 8,778 | -5,401*** |
| Year 4† | 24,398 | -4,945** | 25,710 | -6,257*** | 22,817 | -3,982*** | 16,554 | -2,719** | 9,078 | -6,121*** |
| Sample Size, Quarters 1-12 | 144 | 135 | 367 | 272 | 620 | 520 | 624 | 508 | 228 | 170 |
| Sample Size, Quarters 13-16 | 99 | 84 | 224 | 175 | 423 | 361 | 414 | 348 | 147 | 101 |

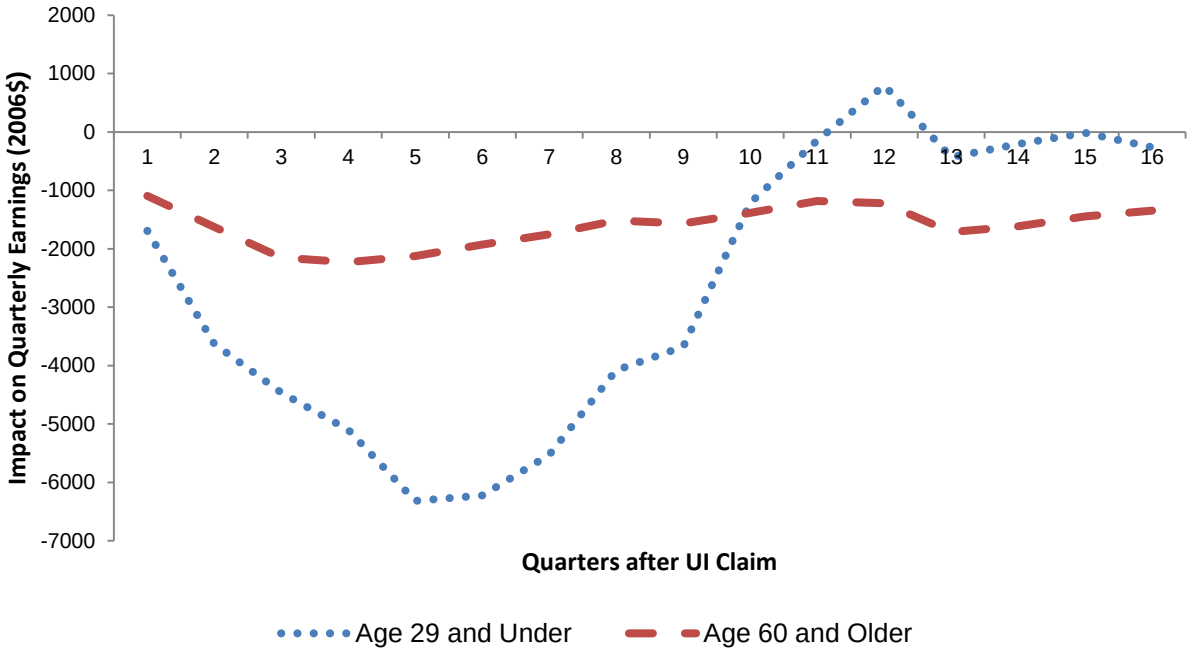
Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Differences in impacts of TAA among subgroups are statistically significant at the 0.05 level.

**Exhibit VII-3: Impacts on Earnings, by Age at UI Claim**




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

whereas there were no impacts on retirement rates for the younger workers. Given the impacts on retirement, we would expect that the TAA program might affect receipt of other sources of income including pension and Social Security benefits. We examine impacts on these other sources of income in Chapter IX.

## B. IMPACTS FOR SERVICE SUBGROUPS

The TAA program does not include a prescribed set of activities for all participants. Some TAA participants enroll in long training programs for new occupations. Others might select shorter training courses in their current occupations. Some participants receive waivers for having marketable skills, so they forgo training and receive TRA payments while looking for appropriate jobs, while others receive TRA payments after getting training waivers for reasons of age or poor health. Some participants are co-enrolled in WIA and receive additional wrap-around services; others are not. Some older workers choose to forgo training and receive ATAA payments to supplement their wages in their new jobs.

## Table VII-6: Impacts on Labor Force Status at the Follow-Up Interview (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Employment Status at Follow-up Interview† | | | | |
| Working | 65.1 | 66.6 | -1.4 | 2.2 |
| In Training | 3.0 | 1.2 | 1.8*** | 0.5 |
| Unemployed | 10.6 | 11.7 | -1.1 | 1.6 |
| Retired | 7.7 | 5.1 | 2.6*** | 0.8 |
| Out of the Labor Force | 13.6 | 15.4 | -1.9 | 1.6 |
| Sample Size | 2,011 | 1,769 | | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA on distribution of categories is statistically significant at the 0.05 level.

## Table VII-7: Impacts on Labor Force Status at the Follow-Up Interview, by Age at UI Claim (Survey Data)

| | Age 49 and Under | | | Age 50 and Older | | |
|---|---|---|---|---|---|---|
| | TAA Participants | Comparison Group | Impact | TAA Participants | Comparison Group | Impact |
| Employment Status at Follow-up Interview† | | | | | | |
| Working | 75.5 | 79.7 | -4.2* | 49.7 | 51.0 | -1.3 |
| In Training | 4.1 | 2.2 | 1.9** | 1.4 | 0.8 | 0.5*** |
| Unemployed | 11.1 | 9.0 | 2.1 | 9.7 | 13.1 | -3.3 |
| Retired | 0.2 | 0.2 | 0.0 | 18.9 | 13.5 | 5.4*** |
| Out of the Labor Force | 9.0 | 8.8 | 0.2 | 20.3 | 21.6 | -1.3 |
| Sample Size | 1,160 | 1,024 | | 851 | 745 | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA on distribution of categories is statistically significant at the 0.05 level.

With such varied program experiences, an important policy issue is to understand the extent to which employment and earnings impacts differ for TAA participants who receive different services. Answering these questions introduces substantial methodological challenges, however. As discussed in Chapter II, because of potential selection biases due to decisions made by both participants and TAA counselors regarding the types of program services that participants receive, the program service subgroup findings must be viewed as only suggestive of true program effects. These decisions are very difficult to model accurately, suggesting that there may be unobservable differences between participants in particular TAA service subgroups and their matched comparisons that could bias the subgroup findings. In recognition of these sample selection issues, we label the program service subgroup findings as treatment-comparison "differences" or "effects" rather than "impacts."

Another important issue related to the program service subgroup findings is that they pertain only to the population of participants who are typically assigned to those program service streams. The results were obtained by comparing the outcomes for treatments assigned to those components to outcomes for their matched comparisons. As such, the results cannot necessarily be used to measure the effectiveness of a particular program service for the average TAA participant, or to assess how a participant in one program component would fare in another one. This occurs because there are differences in the characteristics of those assigned to specific components.

## 1. Trainees and TRA-Only Participants

Because the program experiences and characteristics of trainees and TRA-only participants differ, we expect the pattern of their labor market outcomes to also differ. In particular, we expect that any positive employment-related differences between participants and comparisons to emerge later for trainees, and to perhaps be larger for trainees after they complete their training programs and find jobs that match their skills. To examine these hypotheses, we conducted a subgroup analysis where we defined (1) trainees as those who received TAA-funded training according to the TAPR or initial interview data, and (2) TRA-only participants as those who received TRA but not training.

The data support these expectations: program effects on employment rates were more negative for the trainees than the TRA-only workers during years 1 and 2, but the treatment-comparison employment gap closed more quickly for trainees in years 3 and 4 (Table VII-8 and Exhibit VII-4). By quarter 16, the treatment-comparison difference was not statistically significant for the trainees, but was -9.2 percentage points and statistically significant for the TRA-only group. It is interesting to note that employment rates were higher throughout the follow-up period for the comparisons matched to the trainees than the comparisons matched to the TRA-only workers. This highlights the fact that trainees and TRA-only participants come from different populations, and that our findings for trainees cannot necessarily be used to assess how TRA-only participants would fare if they had instead received TAA-funded training, and vice versa.

The pattern of earnings results for the trainees and TRA-only workers is similar to the pattern of employment results (Table VII-9 and Exhibit VII-5). Treatment-comparison differences in quarterly earnings in years 1 and 2 were more negative for the trainees than for the TRA-only workers (about -$3,500 per quarter, compared to about -$2,500 per quarter), but the subsequent

## Table VII-8: Differences in Employment, by TAA Service Receipt (Survey Data)

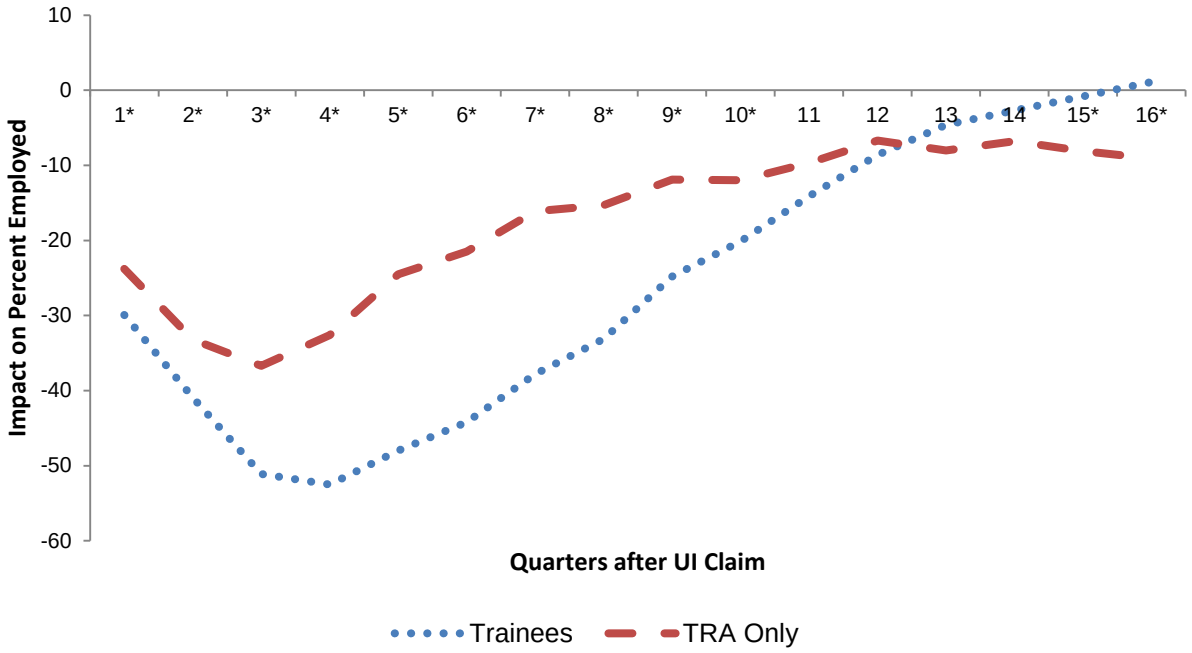
| | All TAA Participants | | Trainees | | TRA Only | |
|---|---|---|---|---|---|---|
| | Comparison Group | Impact | Comparison Group | Difference | Comparison Group | Difference |
| Employed | | | | | | |
| Quarter 1† | 37.5 | -30.3*** | 36.4 | -29.9*** | 33.1 | -23.8*** |
| Quarter 2† | 49.7 | -40.2*** | 48.8 | -40.9*** | 46.0 | -33.2*** |
| Quarter 3† | 62.1 | -46.7*** | 62.6 | -51.1*** | 57.7 | -36.7*** |
| Quarter 4† | 70.1 | -46.6*** | 70.6 | -52.5*** | 64.3 | -32.6*** |
| Quarter 5† | 71.4 | -40.6*** | 71.8 | -48.0*** | 64.5 | -24.5*** |
| Quarter 6† | 74.6 | -37.8*** | 75.4 | -44.2*** | 66.2 | -21.5*** |
| Quarter 7† | 73.6 | -31.8*** | 74.7 | -37.8*** | 65.4 | -16.1*** |
| Quarter 8† | 75.1 | -28.9*** | 76.3 | -33.1*** | 65.9 | -15.3*** |
| Quarter 9† | 73.3 | -22.0*** | 74.9 | -24.8*** | 64.7 | -11.9*** |
| Quarter 10† | 71.9 | -17.9*** | 74.3 | -20.1*** | 65.7 | -12.0*** |
| Quarter 11 | 70.4 | -13.3*** | 73.2 | -14.1*** | 63.5 | -9.7*** |
| Quarter 12 | 68.0 | -8.5*** | 71.1 | -8.6*** | 60.9 | -6.7** |
| Quarter 13 | 68.0 | -5.7** | 71.5 | -4.6 | 60.7 | -8.0*** |
| Quarter 14 | 68.9 | -4.4* | 70.8 | -2.7 | 60.2 | -6.8** |
| Quarter 15† | 69.8 | -5.4** | 72.0 | -0.8 | 59.3 | -8.1** |
| Quarter 16† | 69.0 | -4.3* | 72.5 | 1.1 | 59.9 | -9.2*** |
| Weeks of Employment | | | | | | |
| Quarters 1 – 4† | 25.5 | -19.4*** | 25.6 | -20.6*** | 23.4 | -15.6*** |
| Quarters 5 – 8 † | 37.8 | -18.9*** | 38.5 | -22.3*** | 33.5 | -10.7*** |
| Quarters 9 – 12† | 37.1 | -9.0*** | 38.9 | -10.5*** | 33.4 | -5.8*** |
| Quarters 13 – 16 | 35.0 | -2.0 | 36.8 | -0.7 | 31.2 | -4.0** |
| Sample Size, Quarters 1-12 | | | | 1,212[a] | | 611[a] |
| Sample Size, Quarters 13-16 | | | | 770[a] | | 425[a] |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]This is the sample size for the TAA participant subgroup.

## Exhibit VII-4: Differences in Employment, by TAA Service Receipt




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table VII-9: Differences in Earnings, by TAA Service Receipt (Survey Data)

| | All TAA Participants | | Trainees | | TRA Only | |
|---|---|---|---|---|---|---|
| | Comparison Group | Impact | Comparison Group | Difference | Comparison Group | Difference |
| Quarterly Earnings (2006$) | | | | | | |
| Quarter 1 | 1,964 | -1,616*** | 1,924 | -1,619*** | 1,843 | -1,396*** |
| Quarter 2† | 3,838 | -3,289*** | 3,781 | -3,295*** | 3,539 | -2,851*** |
| Quarter 3† | 4,732 | -3,885*** | 4,758 | -4,142*** | 4,504 | -3,284*** |
| Quarter 4† | 5,353 | -4,018*** | 5,406 | -4,388*** | 4,947 | -3,181*** |
| Quarter 5† | 5,589 | -3,734*** | 5,688 | -4,199*** | 5,090 | -2,756*** |
| Quarter 6† | 5,730 | -3,406*** | 5,845 | -3,872*** | 5,153 | -2,435*** |
| Quarter 7† | 5,712 | -3,097*** | 5,873 | -3,521*** | 5,100 | -2,209*** |
| Quarter 8† | 5,657 | -2,781*** | 5,840 | -3,150*** | 5,078 | -2,015*** |
| Quarter 9† | 5,586 | -2,439*** | 5,827 | -2,667*** | 4,967 | -1,887*** |
| Quarter 10† | 5,371 | -2,036*** | 5,664 | -2,288*** | 4,851 | -1,672*** |
| Quarter 11 | 5,306 | -1,783*** | 5,645 | -1,938*** | 4,809 | -1,651*** |
| Quarter 12 | 5,173 | -1,489*** | 5,557 | -1,556*** | 4,657 | -1,589*** |
| Quarter 13 | 4,866 | -964*** | 5,283 | -1,063*** | 4,412 | -1,376*** |
| Quarter 14 | 4,821 | -841*** | 5,230 | -966*** | 4,365 | -1,348*** |
| Quarter 15 | 4,852 | -782*** | 5,216 | -755*** | 4,288 | -1,319*** |
| Quarter 16† | 4,839 | -761*** | 5,236 | -569** | 4,172 | -1,274*** |
| Annual Earnings (2006$) | | | | | | |
| Quarters 1 – 4† | 15,728 | -12,674*** | 15,654 | -13,242*** | 14,634 | -10,573*** |
| Quarters 5 – 8 † | 22,561 | -12,987*** | 23,079 | -14,658*** | 20,312 | -9,433*** |
| Quarters 9 – 12 | 20,999 | -7,451*** | 21,638 | -7,563*** | 18,575 | -6,240*** |
| Quarters 13 – 16 | 19,189 | -3,273*** | 18,015 | -3,097*** | 15,622 | -5,023*** |
| Sample Size, Quarters 1-12 | | | | 1,212[a] | | 611[a] |
| Sample Size, Quarters 13-16 | | | | 770[a] | | 425[a] |

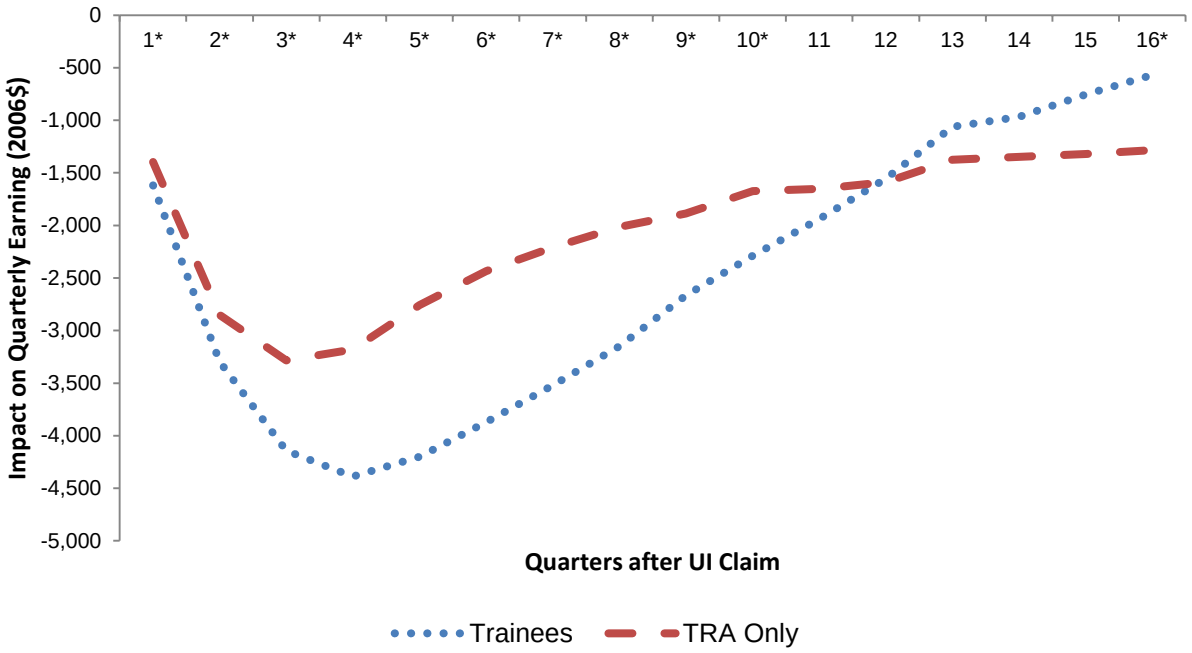
Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]This is the sample size for the TAA participant subgroup.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Exhibit VII-5: Differences in Earnings, by TAA Service Receipt




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

earnings gap closed more quickly for the trainees. By quarter 16, trainees earned $569 less than their comparisons, and the corresponding figure is $1,274 for the TRA-only participants (both are statistically significant). Similarly, in the final follow-up year, trainees earned about $3,100 less than their matched comparisons, and the TRA-only group earned about $5,000 less than their matched comparisons.

Although there are numerous reasons to be cautious about the interpretation of these differences, TAA trainees did appear to be catching up to their comparisons towards the end of the follow-up period. As discussed, the average TAA trainee spends about 1.5 years in training, and the adjustment back into the labor market may take time, especially for workers who switch careers. Thus, longer-term follow-up may be needed to observe the full economic returns to training.

Finally, we examined the extent to which impacts for trainees differed by age. Although these results were somewhat unstable due to small sample sizes, the general pattern of findings suggests that impacts were larger for younger trainees than older ones. For example, the impact on average weeks worked in year 4 was 7 weeks for trainees younger than 40 (a statistically significant impact), 1.7 weeks for trainees between the ages of 40 and 59 (not statistically significant), and -13.1 weeks

for trainees 60 and older (not statistically significant) (not shown). Similarly, the earnings impacts in quarter 16 were positive and statistically significant at the 10 percent level for the youngest trainees, statistically insignificant for those 40 to 59, and negative and significant for trainees 60 and older. Thus, TAA training appears to be more effective for younger customers than older ones.

## 2. WIA Co-enrollment

The 2002 TAA Reform Act mandated closer collaboration between the TAA program and state and local One-Stop systems and partners, including WIA. As discussed in the report on the implementation of the Trade Act of 2002 (D'Amico et al. 2009), the implementation of these mandates was somewhat uneven across states and local areas during the period when our sample of participants was receiving program services. Nonetheless, participants who co-enrolled in WIA may have had access to additional case management and wrap-around services beyond those they received from TAA counselors, including front-end services such as assessment and counseling, job search or training guidance, and placement assistance and follow-up services after training was completed. Thus, it is of policy interest to examine TAA effects on labor market outcomes separately for participants who co-enrolled in WIA and those who did not.

We found no evidence that program effects differed by WIA co-enrollment status (Table VII-10 and Table VII-11). In most quarters, differences between program effects for those who co-enrolled and those who did not were not statistically significant. In some early quarters, program effects were slightly worse for the co-enrollees, but the differences were small, and can likely be explained by the higher rates of training for the co-enrollees; 85 percent of participants who co-enrolled received training, compared to 55 percent of non-co-enrolled participants (not shown).

We also examined program effects on labor market outcomes by WIA co-enrollment status for trainees. This was an important subgroup analysis, because trainees could gain more than other TAA participants from the wrap-around services provided by WIA. Again, however, we did not find significant differences in program effects between trainees who co-enrolled in WIA and trainees who did not (Table VII-12 and Table VII-13).

## 3. ATAA

The ATAA program allows workers who are 50 years of age and older—workers for whom retraining might not be appropriate because of their nearness to retirement—to receive wage subsidies if they accept reemployment at lower wages than what they earned at separation. Take-up of ATAA was quite low during the study period (about 5 percent of eligible participants in our sample), so we estimated subgroup impacts for ATAA using the certified worker *administrative* records sample (which is considerably larger than the follow-up survey sample) and using UI wage records (which cover the 12 quarters after job loss). As discussed below in Section C, the full-sample impact results using the certified worker administrative sample were very similar to the impact results using the follow-up survey sample.

## Table VII-10: Differences in Employment, by WIA Co-enrollment Status (Survey Data)

| | All TAA Participants | | Co-enrolled in WIA | | Not Co-enrolled in WIA | |
|---|---|---|---|---|---|---|
| | Comparison Group | Impact | Comparison Group | Difference | Comparison Group | Difference |
| Employed | | | | | | |
| Quarter 1 | 37.5 | -30.3*** | 37.7 | -29.5*** | 34.0 | -28.8*** |
| Quarter 2 | 49.7 | -40.2*** | 49.5 | -39.3*** | 46.1 | -35.9*** |
| Quarter 3† | 62.1 | -46.7*** | 62.4 | -47.8*** | 60.1 | -42.4*** |
| Quarter 4† | 70.1 | -46.6*** | 70.0 | -46.9*** | 66.8 | -41.2*** |
| Quarter 5† | 71.4 | -40.6*** | 72.4 | -41.7*** | 68.7 | -35.2*** |
| Quarter 6 | 74.6 | -37.8*** | 75.7 | -38.5*** | 71.1 | -33.3*** |
| Quarter 7† | 73.6 | -31.8*** | 72.7 | -31.9*** | 69.8 | -26.0*** |
| Quarter 8† | 75.1 | -28.9*** | 75.1 | -28.8*** | 70.4 | -23.1*** |
| Quarter 9 | 73.3 | -22.0*** | 72.9 | -19.1*** | 68.0 | -16.5*** |
| Quarter 10 | 71.9 | -17.9*** | 72.7 | -16.2*** | 68.2 | -14.1*** |
| Quarter 11 | 70.4 | -13.3*** | 73.4 | -13.6*** | 68.1 | -11.2*** |
| Quarter 12 | 68.0 | -8.5*** | 70.3 | -6.7** | 65.5 | -8.1*** |
| Quarter 13 | 68.0 | -5.7** | 68.4 | -3.9 | 63.9 | -5.9* |
| Quarter 14 | 68.9 | -4.4* | 69.8 | -4.3 | 64.8 | -5.2* |
| Quarter 15 | 69.8 | -5.4** | 71.6 | -4.4 | 67.2 | -7.5*** |
| Quarter 16 | 69.0 | -4.3* | 72.7 | -4.2 | 67.0 | -7.5** |
| Weeks of Employment | | | | | | |
| Quarters 1 – 4† | 25.5 | -19.4*** | 25.6 | -20.0*** | 24.7 | -18.2*** |
| Quarters 5 – 8 † | 37.8 | -18.9*** | 37.9 | -19.8*** | 36.0 | -16.2*** |
| Quarters 9 – 12 | 37.1 | -9.0*** | 38.3 | -9.1*** | 35.3 | -7.4*** |
| Quarters 13 – 16 | 35.0 | -2.0 | 37.4 | -2.0 | 34.7 | -3.9** |
| Sample Size, Quarters 1-12 | | | | 906[a] | | 924[a] |
| Sample Size, Quarters 13-16 | | | | 584[a] | | 594[a] |

Source: Mathematica TAA Initial and Follow-up Surveys and WIASRD administrative data.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]This is the sample size for the TAA participant subgroup.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Differences in impacts of TAA among subgroups are statistically significant at the 0.05 level.

## Table VII-11: Differences in Earnings, by WIA Co-enrollment Status (Survey Data)

| | All TAA Participants | | Co-enrolled in WIA | | Not Co-enrolled in WIA | |
|---|---|---|---|---|---|---|
| | Comparison Group | Impact | Comparison Group | Difference | Comparison Group | Difference |
| Quarterly Earnings (2006$) | | | | | | |
| Quarter 1 | 1,964 | -1,616*** | 1,941 | -1,510*** | 2,024 | -1,751*** |
| Quarter 2 | 3,838 | -3,289*** | 3,700 | -3,146*** | 3,611 | -3,056*** |
| Quarter 3† | 4,732 | -3,885*** | 4,735 | -3,963*** | 4,692 | -3,747*** |
| Quarter 4† | 5,353 | -4,018*** | 5,300 | -4,119*** | 5,236 | -3,707*** |
| Quarter 5† | 5,589 | -3,734*** | 5,514 | -3,787*** | 5,499 | -3,428*** |
| Quarter 6 | 5,730 | -3,406*** | 5,697 | -3,424*** | 5,655 | -3,175*** |
| Quarter 7 | 5,712 | -3,097*** | 5,630 | -3,036*** | 5,590 | -2,842*** |
| Quarter 8 | 5,657 | -2,781*** | 5,655 | -2,776*** | 5,519 | -2,515*** |
| Quarter 9 | 5,586 | -2,439*** | 5,579 | -2,344*** | 5,374 | -2,145*** |
| Quarter 10 | 5,371 | -2,036*** | 5,444 | -1,998*** | 5,174 | -1,754*** |
| Quarter 11 | 5,306 | -1,783*** | 5,471 | -1,745*** | 5,163 | -1,633*** |
| Quarter 12 | 5,173 | -1,489*** | 5,417 | -1,367*** | 5,020 | -1,454*** |
| Quarter 13 | 4,866 | -964*** | 5,088 | -923*** | 4,747 | -1,103*** |
| Quarter 14 | 4,821 | -841*** | 5,056 | -933*** | 4,728 | -1,043*** |
| Quarter 15 | 4,852 | -782*** | 5,032 | -804*** | 4,730 | -980*** |
| Quarter 16 | 4,839 | -761*** | 4,990 | -509* | 4,663 | -958*** |
| Annual Earnings (2006$) | | | | | | |
| Quarters 1 – 4 | 15,728 | -12,674*** | 15,466 | -12,550*** | 15,332 | -12,058*** |
| Quarters 5 – 8 | 22,561 | -12,987*** | 22,383 | -12,987*** | 22,041 | -11,870*** |
| Quarters 9 – 12 | 20,999 | -7,451*** | 20,796 | -6,493*** | 19,873 | -6,360*** |
| Quarters 13 – 16 | 19,189 | -3,273*** | 17,255 | -2,904*** | 16,433 | -3,708*** |
| Sample Size, Quarters 1-12 | | | | 906[a] | | 924[a] |
| Sample Size, Quarters 13-16 | | | | 584[a] | | 594[a] |

Source: Mathematica TAA Initial and Follow-up Surveys and WIASRD administrative data.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]This is the sample size for the TAA participant subgroup.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Differences in impacts of TAA among subgroups are statistically significant at the 0.05 level.

## Table VII-12: Differences in Employment for Trainees, by WIA Co-enrollment Status (Survey Data)

| | All TAA Trainees | | Trainees Co-enrolled in WIA | | Trainees Not Co-enrolled in WIA | |
|---|---|---|---|---|---|---|
| | Comparison Group | Impact | Comparison Group | Difference | Comparison Group | Difference |
| Employed | | | | | | |
| Quarter 1 | 36.4 | -29.9*** | 37.8 | -31.5*** | 36.6 | -29.9*** |
| Quarter 2 | 48.8 | -40.9*** | 49.8 | -43.4*** | 50.0 | -42.9*** |
| Quarter 3† | 62.6 | -51.1*** | 63.3 | -53.2*** | 64.0 | -53.6*** |
| Quarter 4† | 70.6 | -52.5*** | 70.6 | -54.5*** | 71.8 | -55.5*** |
| Quarter 5† | 71.8 | -48.0*** | 73.5 | -51.5*** | 73.4 | -52.4*** |
| Quarter 6 | 75.4 | -44.2*** | 77.1 | -46.0*** | 76.6 | -46.5*** |
| Quarter 7 | 74.7 | -37.8*** | 73.2 | -37.3*** | 74.9 | -39.9*** |
| Quarter 8 | 76.3 | -33.1*** | 75.7 | -33.3*** | 77.2 | -35.8*** |
| Quarter 9 | 74.9 | -24.8*** | 72.8 | -22.1*** | 75.2 | -26.4*** |
| Quarter 10 | 74.3 | -20.1*** | 73.2 | -18.9*** | 75.2 | -22.5*** |
| Quarter 11 | 73.2 | -14.1*** | 73.3 | -14.0*** | 73.2 | -16.0*** |
| Quarter 12 | 71.1 | -8.6*** | 70.4 | -4.5 | 70.9 | -7.9** |
| Quarter 13 | 71.5 | -4.6 | 68.9 | -1.3 | 71.5 | -3.7 |
| Quarter 14 | 70.8 | -2.7 | 70.4 | -2.0 | 70.3 | -2.0 |
| Quarter 15 | 72.0 | -0.8 | 73.8 | -1.2 | 72.0 | -0.3 |
| Quarter 16 | 72.5 | 1.1 | 76.2 | -1.3 | 73.6 | 0.4 |
| Weeks of Employment | | | | | | |
| Quarters 1 – 4 | 25.6 | -20.6*** | 25.6 | -21.3*** | 25.8 | -21.3 |
| Quarters 5 – 8 | 38.5 | -22.3*** | 38.5 | -22.8*** | 38.9 | -23.8*** |
| Quarters 9 – 12 | 38.9 | -10.5*** | 38.4 | -9.6*** | 39.1 | -11.4*** |
| Quarters 13 – 16 | 36.8 | -0.7 | 38.4 | -1.4 | 37.2 | -1.0 |
| Sample Size, Quarters 1-12 | | | | 671[a] | | 778 |
| Sample Size, Quarters 13-16 | | | | 420[a] | | 505[a] |

Source: Mathematica TAA Initial and Follow-up Surveys and WIASRD administrative data.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]This is the sample size for the TAA participant subgroup.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Differences in impacts of TAA among subgroups are statistically significant at the 0.05 level.

## Table VII-13: Differences in Earnings for Trainees, by WIA Co-enrollment Status (Survey Data)

| | All TAA Trainees | | Trainees Co-enrolled in WIA | | Trainees Not Co-enrolled in WIA | |
|---|---|---|---|---|---|---|
| | Comparison Group | Difference | Comparison Group | Difference | Comparison Group | Difference |
| Quarterly Earnings (2006$) | | | | | | |
| Quarter 1 | 1,924 | -1,619*** | 1,889 | -1,513*** | 1,815 | -1,431*** |
| Quarter 2 | 3,781 | -3,295*** | 3,669 | -3,212*** | 3,754 | -3,278*** |
| Quarter 3† | 4,758 | -4,142*** | 4,721 | -4,148*** | 4,717 | -4,161*** |
| Quarter 4† | 5,406 | -4,388*** | 5,307 | -4,460*** | 5,345 | -4,517*** |
| Quarter 5† | 5,688 | -4,199*** | 5,546 | -4,243*** | 5,557 | -4,317*** |
| Quarter 6 | 5,845 | -3,872*** | 5,708 | -3,868*** | 5,672 | -3,940*** |
| Quarter 7 | 5,873 | -3,521*** | 5,625 | -3,375*** | 5,655 | -3,519*** |
| Quarter 8 | 5,840 | -3,150*** | 5,653 | -3,029*** | 5,684 | -3,223*** |
| Quarter 9 | 5,827 | -2,667*** | 5,544 | -2,484*** | 5,660 | -2,796*** |
| Quarter 10 | 5,664 | -2,288*** | 5,398 | -2,104*** | 5,546 | -2,464*** |
| Quarter 11 | 5,645 | -1,938*** | 5,460 | -1,804*** | 5,535 | -2,107*** |
| Quarter 12 | 5,557 | -1,556*** | 5,461 | -1,358*** | 5,489 | -1,671*** |
| Quarter 13 | 5,283 | -1,063*** | 5,151 | -892*** | 5,227 | -1,166*** |
| Quarter 14 | 5,230 | -966*** | 5,154 | -954*** | 5,184 | -1,116*** |
| Quarter 15 | 5,216 | -755*** | 5,226 | -805** | 5,155 | -896*** |
| Quarter 16 | 5,236 | -569** | 5,276 | -482 | 5,202 | -656** |
| Annual Earnings (2006$) | | | | | | |
| Quarters 1 – 4 | 15,654 | -13,242*** | 15,373 | -13,132*** | 15,429 | -13,198*** |
| Quarters 5 – 8 | 23,079 | -14,658*** | 22,407 | -14,448*** | 22,449 | -14,944*** |
| Quarters 9 – 12 | 21,638 | -7,563*** | 20,693 | -6,725*** | 21,156 | -8,091*** |
| Quarters 13 – 16 | 18,015 | -3,097*** | 17,423 | -2,654** | 18,100 | -3,574*** |
| Sample Size, Quarters 1-12 | | | | 671[a] | | 778[a] |
| Sample Size, Quarters 13-16 | | | | 420[a] | | 505[a] |

Source: Mathematica TAA Initial and Follow-up Surveys and WIASRD administrative data.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]This is the sample size for the TAA participant subgroup.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Differences in impacts of TAA among subgroups are statistically significant at the 0.05 level.

We defined two subgroups for the ATAA analysis: (1) ATAA recipients, and (2) age-eligible TAA participants who chose not to receive ATAA. We compared the estimated program effects for these two subgroups. At least in the short-term, we would expect labor market effects to be larger for ATAA recipients than for ATAA-eligible nonparticipants, because ATAA provides incentives for recipients to become reemployed quickly (within 26 weeks), and because ATAA recipients do not receive TAA-funded training services. To the extent that ATAA recipients continue working after they no longer receive their wage supplements (which can last up to two years), we might expect that ATAA participation will also lead to longer-term earnings gains.

As expected, we found that the effects of the TAA program on employment for older workers differed substantially by ATAA receipt. ATAA recipients were much more likely to be employed than their matched comparisons throughout the follow-up period. Almost 80 percent of ATAA recipients were employed in the first quarter following job loss, compared to 55 percent of comparisons, a statistically significance difference of 23 percentage points (Table VII-14). These large positive employment effects continued throughout the 12-quarter follow-up period. In contrast, for ATAA-eligible nonparticipants, the employment effects of the TAA program were negative and statistically significant in each follow-up quarter.

The earnings effects also differed by ATAA receipt (Table VII-15). In most quarters, there was no significant difference in the earnings of ATAA recipients and their matched comparisons. In contrast, ATAA-eligible nonparticipants earned significantly less than their comparisons in every quarter, although the gap did decline somewhat over the follow-up period. Importantly, it is difficult to interpret the earnings impacts for the ATAA recipients, because the quarterly earnings reported in the administrative UI records does not include the ATAA subsidy payment.

There are numerous reasons to be cautious about the interpretations of these differences. TAA participants can only receive ATAA if they find full-time jobs within 26 weeks of job loss, so the ATAA subgroup is conditional on positive employment outcomes. This suggests that sample selection biases might be present, because these workers (who comprise only about 5 percent of those eligible for ATAA) may be particularly motivated to find employment quickly and to have the skills to do so. Thus, it is very difficult to find a matched comparison group for the small number of ATAA recipients in the sample that could overcome these selection issues. This turned about to be a major concern, because the employment and earnings outcomes were very similar for the comparisons matched to the ATAA participants and the comparisons matched to the ATAA-eligible nonparticipants. Thus, we have serious doubts about the quality of the comparison group matches for the ATAA subgroup analysis.

## C. SENSITIVITY OF EMPLOYMENT AND EARNINGS IMPACTS

Using survey data, we were able to closely match TAA participants to comparisons on a very rich set of survey characteristics that are usually unavailable in quasi-experimental evaluations of training programs, which typically rely on administrative data only. Even with our rich survey data, however, there is always the remaining concern that unobservable differences exist between the participants and comparisons that could bias the estimated impacts. Such unobserved factors could include treatment-comparison differences in their skills, marketability, support systems, and motivation to become reemployed, as well as differences in available employment opportunities in their local labor markets.

## Table VII-14: Differences in Employment, by ATAA Receipt (Administrative Data)

| | ATAA Recipients | | | Age Eligible, ATAA Nonparticipants | | |
|---|---|---|---|---|---|---|
| | TAA Participants | Comparison Group | Difference | TAA Participants | Comparison Group | Difference |
| Employed | | | | | | |
| Quarter 1† | 78.8 | 55.4 | 23.4*** | 43.4 | 65.6 | -22.1*** |
| Quarter 2† | 86.1 | 62.6 | 23.6*** | 29.0 | 69.6 | -40.5*** |
| Quarter 3† | 86.0 | 69.7 | 16.3*** | 26.5 | 72.9 | -46.5*** |
| Quarter 4† | 88.8 | 66.7 | 22.1*** | 30.2 | 72.9 | -42.6*** |
| Quarter 5† | 90.1 | 68.5 | 21.5*** | 33.9 | 71.6 | -37.8*** |
| Quarter 6† | 91.0 | 66.2 | 24.8*** | 37.0 | 70.5 | -33.5*** |
| Quarter 7† | 92.7 | 68.1 | 24.6*** | 38.6 | 69.8 | -31.1*** |
| Quarter 8† | 90.0 | 66.5 | 23.5*** | 41.3 | 68.5 | -27.3*** |
| Quarter 9† | 90.2 | 60.8 | 29.4*** | 42.1 | 66.8 | -24.7*** |
| Quarter 10† | 89.2 | 59.5 | 29.6*** | 43.6 | 65.8 | -22.2*** |
| Quarter 11† | 86.3 | 58.5 | 27.9*** | 43.7 | 65.9 | -22.2*** |
| Quarter 12† | 82.4 | 55.3 | 27.2*** | 44.7 | 63.7 | -19.0*** |
| Sample Size | 193 | 412 | | 3,254 | 6,230 | |

Source: Administrative records.

Notes: Treatment group weights account for sample design, and comparison group weights are constructed using the original matching triads. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Differences in impacts of TAA among subgroups are statistically significant at the 0.05 level.

## Table VII-15: Differences in Earnings, by ATAA Receipt (Administrative Data)

| | ATAA Recipients | | | Age Eligible, ATAA Nonparticipants | | |
|---|---|---|---|---|---|---|
| | TAA Participants | Comparison Group | Difference | TAA Participants | Comparison Group | Difference |
| Quarterly Earnings (2006$) | | | | | | |
| Quarter 1† | 6,432 | 4,144 | 2,288** | 2,622 | 4,489 | -1,867*** |
| Quarter 2† | 5,516 | 4,933 | 583 | 1,402 | 4,968 | -3,566*** |
| Quarter 3† | 5,558 | 5,373 | 185 | 1,330 | 5,306 | -3,975*** |
| Quarter 4† | 5,939 | 4,943 | 996 | 1,543 | 5,207 | -3,663*** |
| Quarter 5† | 5,832 | 5,411 | 421 | 1,696 | 5,238 | -3,542*** |
| Quarter 6† | 6,172 | 5,364 | 808 | 1,867 | 5,297 | -3,430*** |
| Quarter 7† | 6,226 | 5,448 | 778 | 2,021 | 5,116 | -3,095*** |
| Quarter 8† | 6,006 | 5,577 | 430 | 2,094 | 4,829 | -2,735*** |
| Quarter 9† | 5,851 | 5,193 | 657 | 2,171 | 4,742 | -2,571*** |
| Quarter 10† | 5,685 | 4,608 | 1,077*** | 2,201 | 4,716 | -2,515*** |
| Quarter 11† | 5,438 | 4,678 | 760** | 2,256 | 4,633 | -2,377*** |
| Quarter 12† | 5,197 | 4,719 | 478 | 2,319 | 4,427 | -2,108*** |
| Sample Size | 193 | 412 | | 3,254 | 6,230 | |

Source: Administrative records.

Notes: Treatment group weights account for sample design, and comparison group weights are constructed using the original matching triads. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Differences in impacts of TAA among subgroups are statistically significant at the 0.05 level.

While it is impossible to prove definitively that our impact estimates are free of selection biases, we conducted a series of sensitivity tests to examine the robustness of the main impact findings described above. For these tests, we estimated impacts using UI wage records rather than survey data as the source of earnings information, and also drew from a variety of survey and administrative records samples. We estimated impacts for eligible nonparticipants in the certified worker survey sample as a specification test and considered alternative model specifications and weighting schemes. These analyses were all conducted using the full analysis samples.

Across all permutations, the same impact story emerged. During the period of training, the labor market outcomes of participants were significantly worse than similar workers who were not eligible for TAA. However, as the participants finished training and returned to work, the gap between the participants and the comparisons closed, but not completely. In the majority of

specifications, there were no significant impacts on employment rates four years after job loss, but the earnings of TAA participants still lagged behind. Thus, our main impact findings appear to be robust to alternative data sources, samples, and analytic methods.

## 1. Using UI Wage Records

For our primary earnings impacts, we measured employment and earnings outcomes using survey data. As discussed in Chapter II, we also collected administrative UI wage records for the follow-up survey sample, the TRA-beneficiary sample, and the certified worker administrative sample. Each data source has its advantages and disadvantages (see Chapter II). The survey data cover earnings from all formal and informal jobs, but could suffer from misreporting and survey nonresponse. The UI wage records data are available for all sample members and do not suffer from survey misreporting, but also do not reflect all types of earnings (for example, self-employment earnings), and do not cover earnings for sample members who were employed in different states than those of their initial UI claims. Furthermore, because of reporting lags in state UI wage records data systems, the UI wage records cover 12 quarters of the follow-up period rather than the full 16 quarters.

Previous studies have documented some earnings differences using survey and UI wage records for populations similar to the TAA population (Kornfeld and Bloom 1999; Schochet, Burghardt, and McConnell 2008). Consequently, it is important to document, using the follow-up survey sample, whether we find consistent impacts using the two data sources. This is especially important because the UI wage records provide the only source of earnings data for the administrative records samples.

The pattern of employment and earnings impacts using the UI wage records mirrors the pattern of impacts using the survey data (Table VII-16 and Table VII-17). For example, in quarter 12 (the most recent period covered by the UI wage records), the estimated impact on the employment rate was -10 percentage points according to the UI wage records and -9 percentage points according to the survey data (both are statistically significant). Similarly, the quarter 12 earnings impact was -$1,352 using the UI data and -$1,489 using the survey data.

It is important to note that earnings *levels* were higher in the UI wage records than in the survey data in the quarters immediately following job loss, but the UI-survey earnings level differences were smaller afterwards. One explanation for this discrepancy in the early quarters is recall bias. At the initial interview, an average of 29 months had elapsed since the UI claim date. Survey respondents may not have remembered all of their early jobs, particularly those jobs of short duration. A second explanation for the difference in earnings levels is that the definition of quarters varied. In the employment timelines constructed from the survey data, the first quarter started the week after the UI job loss. In the administrative file, we used calendar quarters. The first quarter after job loss was the first calendar quarter after the quarter of job loss. Given this distinction, it is not surprising that the largest difference between the survey- and UI-based wages occurred in the first quarter.

## Table VII-16: Impacts on Employment (UI Wage Records)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Employed | | | | |
| Quarter 1 | 46.2 | 57.2 | -11.0*** | 2.4 |
| Quarter 2 | 32.6 | 63.1 | -30.5*** | 2.4 |
| Quarter 3 | 30.3 | 69.8 | -39.5*** | 2.6 |
| Quarter 4 | 33.9 | 69.0 | -35.0*** | 2.8 |
| Quarter 5 | 35.5 | 69.0 | -33.5*** | 2.6 |
| Quarter 6 | 39.7 | 67.0 | -27.3*** | 2.8 |
| Quarter 7 | 44.5 | 70.4 | -25.9*** | 2.6 |
| Quarter 8 | 49.0 | 70.4 | -21.4*** | 2.8 |
| Quarter 9 | 52.5 | 67.3 | -14.8*** | 2.8 |
| Quarter 10 | 54.9 | 65.0 | -10.1*** | 2.6 |
| Quarter 11 | 57.0 | 67.9 | -10.9*** | 2.6 |
| Quarter 12 | 59.5 | 69.5 | -10.0*** | 2.7 |
| Sample Size | 1,976 | 1,715 | | |

Source: Mathematica Initial and Follow-up Surveys and UI Wage Records.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Table VII-17: Impacts on Earnings (UI Wage Records)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Quarterly Earnings (2006$) | | | | |
| Quarter 1 | 2,724 | 3,543 | -819*** | 253 |
| Quarter 2 | 1,687 | 3,632 | -1,945*** | 223 |
| Quarter 3 | 1,423 | 4,883 | -3,461*** | 489 |
| Quarter 4 | 1,828 | 4,677 | -2,849*** | 254 |
| Quarter 5 | 1,838 | 4,793 | -2,955*** | 257 |
| Quarter 6 | 2,015 | 4,653 | -2,638*** | 250 |
| Quarter 7 | 2,280 | 4,889 | -2,610*** | 251 |
| Quarter 8 | 2,619 | 4,742 | -2,123*** | 257 |
| Quarter 9 | 2,731 | 4,440 | -1,709*** | 248 |
| Quarter 10 | 2,950 | 4,500 | -1,550*** | 229 |
| Quarter 11 | 3,008 | 4,755 | -1,747*** | 296 |
| Quarter 12 | 3,249 | 4,601 | -1,352*** | 242 |
| Sample Size | 1,976 | 1,715 | | |

Source: Mathematica Initial and Follow-up Surveys and UI Wage Records.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## 2. Alternative Follow-Up Survey Samples

As described in Chapter II, both our treatments and matched comparisons consisted of *new* UI recipients. A disadvantage of this approach is that some treatments started collecting UI benefits before their firms became certified for TAA. About 28 percent of TAA participants in the certified worker sample were separated from jobs more than 90 days before their firms' petitions were certified. Furthermore, some may have participated in TAA because they could not quickly find jobs. Thus, these TAA participants may have been more likely than their matched comparisons to have unobserved characteristics that were associated with poor labor market outcomes, which could yield impact estimates that are somewhat biased downwards. As discussed next, we conducted several sensitivity analyses to examine these potential sample selection bias issues.

### a. Limiting the Sample to Those in Firms Who Were Certified for TAA Prior to Job Loss

As a sensitivity analysis, we estimated the impacts of the TAA program on employment and earnings while limiting the sample to TAA participants whose firms were certified prior to their job losses and their matched comparisons (Table VII-18 and Table VII-19). The impact findings using this restricted sample were similar to the benchmark impacts, but were slightly less negative during the first three years (Exhibit VII-6). By year 4, TAA participants in both samples reported about $3,000 less in annual earnings than their matched comparisons.

These results are consistent with the notion that TAA participants who were laid off before their firms became certified may have had unobservable characteristics that were more strongly associated with poor labor market outcomes than other participants. However, these effects appear to be small, because the overall pattern of findings remains unchanged using the restricted sample.

### b. Limiting the Sample to UI Exhaustees

An alternative approach for selecting the initial sample would have been to select treatments and comparisons who had exhausted their UI benefits. We did not adopt this alternative because it is likely that many TAA eligibles exhaust UI because they choose to take advantage of the offer of training, TRA, and other TAA services, and not because they could not find jobs. The data support this theory; using our matched samples, more than 80 percent of participants exhausted UI, compared to only 45 percent of matched comparisons. Thus, choosing the comparison group from among exhaustees would likely have yielded "less-employable" comparisons than treatments, which would likely have created a bias towards more favorable findings for TAA.

Nonetheless, as an important sensitivity analysis, we estimated survey-based impacts on employment and earnings by limiting the sample to TAA participants and comparisons who exhausted their UI benefits. These impact estimates can be viewed as an *upper bound* on the true treatment effects.

As expected, we found that the employment and earnings impacts were less negative using the exhaustee sample than the benchmark sample (Table VII-20 and Table VII-21). The earnings impacts were negative and statistically significant for both samples during years 1 and 2, but were less negative for the exhaustee sample: the earnings impact in year 2 was -$12,987 using the benchmark sample, compared to -$5,924 using the exhaustee sample. However, unlike the

## Table VII-18: Impacts on Employment for Participants from Firms Certified Before Job Loss (Survey Data)

| | Participants with Firms Certified Before Job Loss | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Employed | | | | |
| Quarter 1 | 6.8 | 33.8 | -27.0*** | 2.7 |
| Quarter 2 | 10.4 | 46.7 | -36.3*** | 2.7 |
| Quarter 3 | 16.6 | 60.0 | -43.4*** | 2.9 |
| Quarter 4 | 25.2 | 69.0 | -43.7*** | 2.9 |
| Quarter 5 | 32.6 | 69.5 | -36.9*** | 3.0 |
| Quarter 6 | 38.3 | 72.4 | -34.2*** | 3.0 |
| Quarter 7 | 43.3 | 69.6 | -26.3*** | 3.2 |
| Quarter 8 | 48.7 | 71.9 | -23.3*** | 3.0 |
| Quarter 9 | 53.4 | 70.2 | -16.8*** | 3.1 |
| Quarter 10 | 56.0 | 70.1 | -14.1*** | 2.9 |
| Quarter 11 | 58.9 | 69.1 | -10.2*** | 2.8 |
| Quarter 12 | 60.7 | 67.1 | -6.4** | 3.0 |
| Quarter 13 | 63.4 | 67.1 | -3.7 | 3.0 |
| Quarter 14 | 63.3 | 67.0 | -3.7 | 3.0 |
| Quarter 15 | 64.6 | 69.8 | -5.2 | 3.2 |
| Quarter 16 | 66.3 | 69.3 | -3.0 | 3.4 |
| Weeks of Employment | | | | |
| Quarters 1 – 4 | 6.4 | 24.2 | -17.7*** | 1.0 |
| Quarters 5 – 8 | 19.9 | 36.5 | -16.6*** | 1.5 |
| Quarters 9 – 12 | 29.1 | 36.5 | -7.4*** | 1.4 |
| Quarters 13 – 16 | 34.3 | 34.9 | -0.6 | 1.7 |
| Sample Size, Quarters 1-12 | 1,077 | 1,745 | | |
| Sample Size, Quarters 13-16 | 463 | 1,040 | | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Table VII-19: Impacts on Earnings for Participants from Firms Certified Before Job Loss (Survey Data)

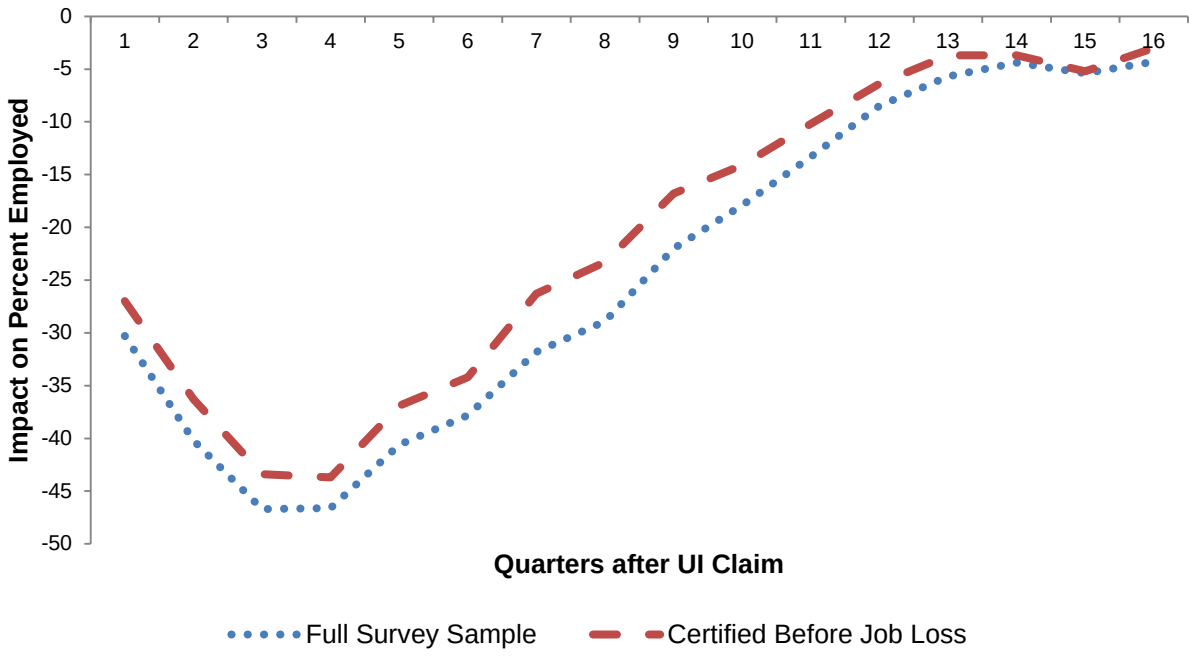
| | Participants with Firms Certified Before Job Loss | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Quarterly Earnings (2006$) | | | | |
| Quarter 1 | 293 | 1,832 | -1,540*** | 260 |
| Quarter 2 | 549 | 3,662 | -3,112*** | 334 |
| Quarter 3 | 898 | 4,640 | -3,742*** | 316 |
| Quarter 4 | 1,485 | 5,242 | -3,758*** | 304 |
| Quarter 5 | 2,011 | 5,538 | -3,527*** | 313 |
| Quarter 6 | 2,483 | 5,620 | -3,137*** | 314 |
| Quarter 7 | 2,765 | 5,544 | -2,779*** | 314 |
| Quarter 8 | 3,042 | 5,510 | -2,467*** | 305 |
| Quarter 9 | 3,322 | 5,470 | -2,148*** | 303 |
| Quarter 10 | 3,521 | 5,346 | -1,825*** | 295 |
| Quarter 11 | 3,701 | 5,353 | -1,653*** | 292 |
| Quarter 12 | 3,892 | 5,321 | -1,429*** | 300 |
| Quarter 13 | 4,013 | 4,997 | -985*** | 314 |
| Quarter 14 | 4,039 | 4,981 | -942*** | 306 |
| Quarter 15 | 4,056 | 4,989 | -933*** | 342 |
| Quarter 16 | 4,171 | 4,771 | -600** | 303 |
| Annual Earnings (2006$) | | | | |
| Quarters 1 – 4 | 3,202 | 15,218 | -12,016*** | 1,114 |
| Quarters 5 – 8 | 10,178 | 22,088 | -11,910*** | 1,210 |
| Quarters 9 – 12 | 14,236 | 20,371 | -6,135*** | 1,134 |
| Quarters 13 – 16 | 12,540 | 15,677 | -3,137*** | 1,052 |
| Sample Size, Quarters 1-12 | 1,077 | 1,745 | | |
| Sample Size, Quarters 13-16 | 463 | 1,040 | | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Exhibit VII-6: Impacts on Employment, by Timing of TAA Certification




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

## Table VII-20: Impacts on Employment for Alternative Samples (Survey Data)

| | Full Survey Sample | | UI Exhaustees | | Workers Not Recalled | |
|---|---|---|---|---|---|---|
| | Comparison Group | Impact | Comparison Group | Impact | Comparison Group | Impact |
| Employed | | | | | | |
| Quarter 1 | 37.5 | -30.3*** | 14.7 | -9.5*** | 31.8 | -25.6*** |
| Quarter 2 | 49.7 | -40.2*** | 24.6 | -18.3*** | 45.3 | -36.5*** |
| Quarter 3 | 62.1 | -46.7*** | 41.1 | -28.4*** | 58.0 | -42.9*** |
| Quarter 4 | 70.1 | -46.6*** | 51.4 | -29.6*** | 67.1 | -43.4*** |
| Quarter 5 | 71.4 | -40.6*** | 54.9 | -24.7*** | 68.6 | -37.6*** |
| Quarter 6 | 74.6 | -37.8*** | 58.9 | -21.8*** | 71.0 | -33.3*** |
| Quarter 7 | 73.6 | -31.8*** | 58.0 | -15.5*** | 70.5 | -27.9*** |
| Quarter 8 | 75.1 | -28.9*** | 61.4 | -15.1*** | 71.8 | -24.8*** |
| Quarter 9 | 73.3 | -22.0*** | 61.1 | -9.7*** | 69.6 | -17.8*** |
| Quarter 10 | 71.9 | -17.9*** | 63.6 | -10.0*** | 69.9 | -15.3*** |
| Quarter 11 | 70.4 | -13.3*** | 59.9 | -2.9 | 68.7 | -11.2*** |
| Quarter 12 | 68.0 | -8.5*** | 58.6 | 1.1 | 65.6 | -5.8** |
| Quarter 13 | 68.0 | -5.7** | 59.1 | 2.4 | 66.0 | -4.3 |
| Quarter 14 | 68.9 | -4.4* | 59.6 | 3.1 | 66.6 | -3.1 |
| Quarter 15 | 69.8 | -5.4** | 59.2 | 4.2 | 68.2 | -3.7 |
| Quarter 16 | 69.0 | -4.3* | 58.5 | 5.5* | 68.0 | -2.9 |
| Weeks of Employment | | | | | | |
| Quarters 1 – 4 | 25.5 | -19.4*** | 16.2 | -11.3*** | 23.4 | -17.7*** |
| Quarters 5 – 8 | 37.8 | -18.9*** | 30.3 | -11.4*** | 36.3 | -17.0*** |
| Quarters 9 – 12 | 37.1 | -9.0*** | 32.3 | -4.2*** | 36.0 | -7.6*** |
| Quarters 13 – 16 | 35.0 | -2.0 | 29.4 | 3.3** | 34.6 | -1.5 |

Source: Mathematica TAA Initial and Follow-up Surveys and UI Claims data.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Table VII-21: Impacts on Earnings for Alternative Samples (Survey Data)

| | Full Survey Sample | | UI Exhaustees | | Workers Not Recalled | |
|---|---|---|---|---|---|---|
| | Comparison Group | Impact | Comparison Group | Impact | Comparison Group | Impact |
| Quarterly Earnings (2006$) | | | | | | |
| Quarter 1 | 1,964 | -1,616*** | 841 | -604*** | 1,651 | -1,393*** |
| Quarter 2 | 3,838 | -3,289*** | 1,804 | -1469*** | 3,472 | -3,006*** |
| Quarter 3 | 4,732 | -3,885*** | 2,726 | -2123*** | 4,419 | -3,622*** |
| Quarter 4 | 5,353 | -4,018*** | 3,437 | -2275*** | 5,049 | -3,723*** |
| Quarter 5 | 5,589 | -3,734*** | 3,767 | -2011*** | 5,349 | -3,490 *** |
| Quarter 6 | 5,730 | -3,406*** | 3,924 | -1617*** | 5,458 | -3,118*** |
| Quarter 7 | 5,712 | -3,097*** | 3,934 | -1285*** | 5,426 | -2,772*** |
| Quarter 8 | 5,657 | -2,781*** | 3,981 | -1078*** | 5,397 | -2,473*** |
| Quarter 9 | 5,586 | -2,439*** | 4,010 | -861*** | 5,339 | -2,147*** |
| Quarter 10 | 5,371 | -2,036*** | 4,089 | -773*** | 5,182 | -1,808*** |
| Quarter 11 | 5,306 | -1,783*** | 4,010 | -495* | 5,163 | -1,599*** |
| Quarter 12 | 5,173 | -1,489*** | 3,887 | -203 | 4,990 | -1,270*** |
| Quarter 13 | 4,866 | -964*** | 3,834 | -22 | 4,717 | -879*** |
| Quarter 14 | 4,821 | -841*** | 3,841 | -32 | 4,730 | -860*** |
| Quarter 15 | 4,852 | -782*** | 3,762 | 97 | 4,775 | -836*** |
| Quarter 16 | 4,839 | -761*** | 3,819 | 77 | 4,682 | -686*** |
| Annual Earnings (2006$) | | | | | | |
| Quarters 1 – 4 | 15,728 | -12,674*** | 8,730 | -6,412*** | 14,370 | -11,547*** |
| Quarters 5 – 8 | 22,561 | -12,987*** | 15,444 | -5,924*** | 21,470 | -11,796*** |
| Quarters 9 – 12 | 20,999 | -7,451*** | 15,104 | -1,601* | 19,740 | -6,072*** |
| Quarters 13 – 16 | 19,189 | -3,273*** | 13,409 | -146 | 16,481 | -3,004*** |

Source: Mathematica TAA Initial and Follow-up Surveys and UI Claims data.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

benchmark sample, the earnings impacts in years 3 and 4 were not statistically significant at the 5 percent level using the exhaustee sample. These results suggest that TAA participation did not lead to positive post-program earnings gains during the four-year follow-up period, even using upper bound estimates.

### c. Excluding Workers Who Were Recalled to Their Jobs

In the matching models, we matched on workers' *expectations* of recall, but not on *actual* recall status, because actual recall status could be an outcome of the TAA program if, for instance, TAA-certified firms are less likely to recall their workers who have access to generous TAA benefits. We did find, however, a significant difference in the share of workers who reported in the survey that they were actually recalled (13 percent for comparisons, compared to 7 percent for participants). Thus, to further assess selection biases, as a sensitivity analysis, we limited the sample to treatments and comparisons who were not recalled to their jobs.

The earnings impact findings are nearly identical using this restricted sample and our benchmark sample (Table VII-20 and Table VII-21). For example, TAA participants in both samples reported about $3,000 less in annual earnings in year 4 than their matched comparisons.

## 3. Using the TRA-Beneficiary Sample

To assess the robustness of the employment-related impact findings, we selected a supplementary nationally representative sample from the universe of TRA beneficiaries. The TRA-beneficiary sample consists of workers who received TRA payments after they exhausted their regular UI benefits. This sample had similar UI claims dates as the TAA certified worker sample, but excludes the small percentage of TAA participants who did not receive TRA. This sample is much larger than the survey sample, and thus, yields more precise earnings impact estimates based on the UI wage records. The comparison group for this sample was matched using UI claims and local area data only, but not the initial survey data. Thus, the comparison group matches for this sample are of lower quality than for the survey sample.

The results from the TRA-beneficiary sample tell a very similar impact story as above (Table VII-22 and Table VII-23). The employment impact in quarter 12 was -4.8 percentage points for the TRA-beneficiary sample and -8.5 percentage points for the follow-up survey sample. The corresponding figures for the earnings impact in quarter 12 are -$1,056 using the TRA-beneficiary sample (and UI wage records) and -$1,489 using the follow-up survey sample (and survey data).

## 4. Using the Certified Worker Administrative Records Sample

In addition to the survey sample, we also selected a larger certified worker sample for administrative data collection. Since both samples were randomly drawn from the same population universe, the estimated impacts based on the two samples should be similar. An important difference between the two samples, however, is that the comparison group for the administrative records sample was matched using UI claims and local area data, but not initial survey data.

## Table VII-22: Impacts on Employment for the TRA-Beneficiary Sample (UI Wage Records)

| | TRA Beneficiaries | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Employed | | | | |
| Quarter 1 | 36.0 | 65.5 | -29.5*** | 2.7 |
| Quarter 2 | 22.5 | 69.3 | -46.8*** | 2.3 |
| Quarter 3 | 24.6 | 71.7 | -47.2*** | 1.7 |
| Quarter 4 | 30.8 | 71.8 | -41.1*** | 1.5 |
| Quarter 5 | 36.2 | 71.3 | -35.1*** | 1.4 |
| Quarter 6 | 40.7 | 70.6 | -29.9*** | 1.3 |
| Quarter 7 | 45.9 | 69.7 | -23.8*** | 1.3 |
| Quarter 8 | 51.3 | 69.2 | -17.8*** | 1.3 |
| Quarter 9 | 55.4 | 68.7 | -13.3*** | 1.2 |
| Quarter 10 | 57.5 | 67.3 | -9.8*** | 1.0 |
| Quarter 11 | 59.2 | 66.2 | -7.0*** | 0.9 |
| Quarter 12 | 60.2 | 65.0 | -4.8*** | 0.9 |
| Sample Size | 9,698 | 14,976 | | |

Source: Administrative UI Claims Files and UI Wage Records.

Notes: Treatment group weights account for sample design, and comparison group weights are constructed using the original matching triads. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Table VII-23: Impacts on Earnings for the TRA-Beneficiary Sample (UI Wage Records)

| | TRA Beneficiaries | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Quarterly Earnings (2006$) | | | | |
| Quarter 1 | 1,787 | 4,233 | -2,446*** | 167 |
| Quarter 2 | 876 | 4,812 | -3,936*** | 123 |
| Quarter 3 | 1,036 | 5,036 | -4,000*** | 117 |
| Quarter 4 | 1,417 | 5,051 | -3,634*** | 107 |
| Quarter 5 | 1,791 | 5,058 | -3,267*** | 101 |
| Quarter 6 | 2,151 | 5,116 | -2,965*** | 92 |
| Quarter 7 | 2,535 | 4,973 | -2,438*** | 104 |
| Quarter 8 | 2,819 | 4,951 | -2,132*** | 114 |
| Quarter 9 | 3,087 | 4,840 | -1,753*** | 103 |
| Quarter 10 | 3,283 | 4,858 | -1,575*** | 103 |
| Quarter 11 | 3,451 | 4,694 | -1,244*** | 86 |
| Quarter 12 | 3,522 | 4,578 | -1,056*** | 84 |
| Sample Size | 9,698 | 14,976 | | |

Source: Administrative UI Claims Files and UI Wage Records.

Notes: Treatment group weights account for sample design, and comparison group weights are constructed using the original matching triads. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

We found consistent employment and earnings impact results using the administrative records and survey samples (Table VII-24 and Table VII-25). In the last quarter covered by the UI wage records (quarter 12), the employment rate impact was -6.0 percentage points using the administrative records sample, compared to -4.3 percentage points using the benchmark survey sample. Likewise, the quarter 12 earnings impacts were similar for the two samples (-$1,054 for the administrative records sample and -$1,489 for the survey sample).

## 5. Impacts on TAA Nonparticipants

An important component of our sensitivity analysis was the estimation of employment-related impacts using TAA *nonparticipants* and their matched comparisons. If the TAA *participants* applied for TAA services because they were less marketable than their matched comparisons along unobservable dimensions, one would expect TAA *nonparticipants*—TAA eligible workers who chose not to receive services—to be more marketable than their matched comparisons. Stated differently, the direction of selection biases should be *opposite* for the nonparticipant impacts and the participant impacts. While TAA may have some positive effects on nonparticipants' labor market activities through the receipt of Rapid Response and other early intervention reemployment services, we expect these impacts to be small. Thus, a finding of small or zero impacts for the TAA nonparticipants would provide evidence about the credibility of the impact findings for the TAA participants.

To estimate impacts for the nonparticipants, we used nonparticipants and their matched comparisons who had completed *initial* interviews (to conserve project resources, follow-up interviews were not conducted with nonparticipants). The kernel matching procedure was used to re-match these participants and comparisons using the full array of matching variables, including those from the initial survey. Thus, although the follow-up period for this analysis was only 8 quarters, this was the period when many comparisons returned to work, and is therefore a relevant period for assessing potential sample selection biases.

Importantly, we found that TAA had no significant impact on nonparticipants' employment or earnings in any of the eight follow-up quarters (Table VII-26). This suggests that sample selection biases are small for this sample. The results from this key sensitivity analysis add considerable credibility to our main impact findings for the TAA participants.

## 6. Alternative Model Specifications

As described in Chapter VI of the MN report, we conducted numerous tests to examine the sensitivity of key outcomes to different weighting schemes for the matched comparisons in the survey sample. In particular, we implemented kernel matching using the matching variables from UI claims and local area data, but *not* from survey data. We also tested weighting schemes that used nearest neighbor matching with replacement, kernel matching with a smaller bandwidth, and kernel matching with a uniform kernel rather than the benchmark Epanechnikov kernel function. We also estimated impact models where functions of the estimated propensity scores were included as model covariates to account for possible differences in model specifications and estimated impacts across propensity score classes (that is, across sample members with different propensity score values).

## Table VII-24: Impacts on Employment for the Certified Worker Administrative Sample (UI Wage Records)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Employed | | | | |
| Quarter 1 | 50.8 | 67.2 | -16.4*** | 2.0 |
| Quarter 2 | 38.2 | 72.0 | -33.8*** | 1.8 |
| Quarter 3 | 37.5 | 74.2 | -36.7*** | 1.6 |
| Quarter 4 | 40.0 | 74.3 | -34.4*** | 1.6 |
| Quarter 5 | 42.1 | 73.4 | -31.3*** | 1.5 |
| Quarter 6 | 45.0 | 72.6 | -27.6*** | 1.5 |
| Quarter 7 | 48.6 | 72.1 | -23.5*** | 1.4 |
| Quarter 8 | 51.8 | 71.0 | -19.2*** | 1.4 |
| Quarter 9 | 54.8 | 69.1 | -14.3*** | 1.4 |
| Quarter 10 | 56.7 | 68.3 | -11.6*** | 1.3 |
| Quarter 11 | 58.1 | 67.3 | -9.2*** | 1.4 |
| Quarter 12 | 59.6 | 65.6 | -6.0*** | 1.3 |
| Sample Size | 10,315 | 15,910 | | |

Source: Administrative UI Claims Files and UI Wage Records.

Notes: Treatment group weights account for sample design, and comparison group weights are constructed using the original matching triads. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Table VII-25: Impacts on Earnings for the Certified Worker Administrative Sample (UI Wage Records)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Quarterly Earnings (2006$) | | | | |
| Quarter 1 | 2,993 | 4,461 | -1,469*** | 229 |
| Quarter 2 | 1,925 | 4,971 | -3,046*** | 150 |
| Quarter 3 | 1,946 | 5,257 | -3,310*** | 112 |
| Quarter 4 | 2,208 | 5,160 | -2,952*** | 100 |
| Quarter 5 | 2,390 | 5,234 | -2,844*** | 91 |
| Quarter 6 | 2,507 | 5,276 | -2,769*** | 74 |
| Quarter 7 | 2,774 | 5,135 | -2,361*** | 89 |
| Quarter 8 | 2,997 | 4,888 | -1,891*** | 76 |
| Quarter 9 | 3,119 | 4,834 | -1,714*** | 83 |
| Quarter 10 | 3,237 | 4,750 | -1,513*** | 100 |
| Quarter 11 | 3,353 | 4,678 | -1,325*** | 109 |
| Quarter 12 | 3,513 | 4,567 | -1,054*** | 117 |
| Sample Size | 10,315 | 15,910 | | |

Source: Administrative UI Claims Files and UI Wage Records.

Notes: Treatment group weights account for sample design, and comparison group weights are constructed using the original matching triads. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Table VII-26: Impacts on Employment and Earnings for the TAA Nonparticipants (Survey Data)

| | TAA Nonparticipants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Employed | | | | |
| Quarter 1 | 35.2 | 33.7 | 1.5 | 2.9 |
| Quarter 2 | 52.0 | 49.5 | 2.5 | 3.2 |
| Quarter 3 | 60.8 | 58.0 | 2.9 | 3.3 |
| Quarter 4 | 65.3 | 62.9 | 2.4 | 3.2 |
| Quarter 5 | 70.3 | 69.5 | 0.9 | 3.2 |
| Quarter 6 | 72.1 | 73.3 | -1.2 | 3.1 |
| Quarter 7 | 74.2 | 75.5 | -1.3 | 3.0 |
| Quarter 8 | 73.5 | 73.4 | 0.0 | 3.0 |
| Quarterly Earnings (2006$) | | | | |
| Quarter 1 | 1,995 | 2,081 | -85 | 217 |
| Quarter 2 | 3,727 | 3,659 | 68 | 283 |
| Quarter 3 | 4,620 | 4,390 | 229 | 292 |
| Quarter 4 | 5,121 | 4,919 | 202 | 311 |
| Quarter 5 | 5,461 | 5,229 | 232 | 316 |
| Quarter 6 | 5,467 | 5,567 | -100 | 318 |
| Quarter 7 | 5,484 | 5,566 | -82 | 320 |
| Quarter 8 | 5,536 | 5,100 | 437 | 311 |
| Sample Size | 670 | 1286 | | |

Source: Mathematica Initial Survey.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

We found that the impacts on employment and earnings were not very sensitive to changes in weighting schemes (Table VII-27). In the fourth year following job loss, our base specification found that TAA participants worked two fewer weeks than comparisons, a statistically insignificant impact. The alternative model specifications found impact estimates ranging from 1.4 fewer weeks to 3.5 fewer weeks. Similarly, the annual earnings impacts in year 4 ranged from -$3,273 to -$3,880 and all were statistically significant.

Importantly, the impact findings were similar using the comparison sample from the benchmark model and the comparison sample constructed using the matching variables from the UI claims and local area data only (but not the survey data). This analysis is important because it suggests that when we use the administrative record samples—and cannot match on the expanded set of survey characteristics—we should not expect sizable bias in the impact estimates.

The impact findings were also not sensitive to alternative model specifications where linear and quadratic estimated propensity score terms were included as covariates in the impact estimation models. The year 4 earnings impact was -$3,683 for models that included the propensity score terms, compared to -$3,273 for models that excluded these variables (not shown). Similarly, the

estimated impact on weeks worked in year 4 was -2.1 weeks in models with the propensity score terms, compared to -2.0 weeks for models without them.

### Table VII-27: Impact Estimates on Employment and Earnings Using Alternate Weights

| | Base Weights | Match on UI Only | Nearest Neighbor | Low Bandwidth | Uniform Kernel |
|---|---|---|---|---|---|
| Weeks of Employment | | | | | |
| Quarters 1 – 4 | -19.4*** | -22.0*** | -19.0*** | -18.8*** | -18.7*** |
| Quarters 5 – 8 | -18.9*** | -18.1*** | -17.5*** | -18.1*** | -18.2*** |
| Quarters 9 – 12 | -9.0*** | -8.2*** | -9.8*** | -9.2*** | -9.1*** |
| Quarters 13 – 16 | -2.0 | -1.4 | -3.5*** | -2.6** | -2.0 |
| Annual Earnings (2006$) | | | | | |
| Quarters 1 – 4 | -12,674*** | -14,510*** | -11,749*** | -12,301*** | -12,176*** |
| Quarters 5 – 8 | -12,987*** | -13,184*** | -11,708*** | -12,676*** | -12,581*** |
| Quarters 9 – 12 | -7,451*** | -7,425*** | -5,925*** | -7,138*** | -6,919*** |
| Quarters 13 – 16 | -3,273*** | -3,659*** | -3,068*** | -3,880*** | -3,542*** |

Source: Mathematica TAA Initial and Follow-Up Survey.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a matching algorithm shown in each column. The first column uses the kernel matching weights used in the main analysis. The second column uses weights constructed by matching only on baseline characteristics available in the UI administrative data. The third column uses a nearest neighbor matching algorithm. The fourth column uses the kernel matching algorithm with a bandwidth of 0.02 instead of 0.07. The fifth column uses the kernel matching algorithm with a uniform kernel rather than the Epanechnikov kernel. Impacts are regression adjusted. Standard errors (not shown) account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

We also tested the sensitivity of the impact estimates to the exclusion of sample members from particular states. The primary treatment group sample for the impact analysis was obtained from the certified worker lists that states are required, by law, to obtain from TAA-certified firms in order to identify workers who could be eligible for services under the petition. The evidence suggests these lists were reasonably complete (see Chapter I of the MN report), but there were certain states where data quality was less certain. In particular, we found two states where the number of names on the certified workers lists exceeded the estimate of affected workers. We also had six states where there was a low match rate (less than 45 percent) between the certified workers lists and the UI claimant file. Furthermore, we had seven states where more than 35 percent of certified workers had layoff dates that fell outside the date range of the available state administrative data, and thus who had missing data.

To test the sensitivity of our benchmark estimates to these data problems, we estimated impacts by excluding from the sample, in turn, each of the problem states. We found that excluding these states had no effect on the impact findings (Table VII-28).

**Table VII-28: Impact Estimates on Employment and Earnings Using Alternate Samples of States**

| | All States | Exclude States With Too Many Certified Workers | Exclude States with Low Match Rate | Exclude States with High Share of Dates out of Range |
|---|---|---|---|---|
| Weeks of Employment | | | | |
| Quarters 1 – 4 | -19.4*** | -18.8*** | -18.8*** | -19.6*** |
| Quarters 5 – 8 | -18.9*** | -17.7*** | -17.6*** | -19.4*** |
| Quarters 9 – 12 | -9.0*** | -8.7*** | -8.4*** | -9.8*** |
| Quarters 13 – 16 | -2.0 | -2.1 | -2.2 | -2.0 |
| Annual Earnings (2006$) | | | | |
| Quarters 1 – 4 | -12,674*** | -12,000*** | -11,952*** | -13,091*** |
| Quarters 5 – 8 | -12,987*** | -12,141*** | -12,218*** | -13,609*** |
| Quarters 9 – 12 | -7,451*** | -6,413*** | -6,395*** | -7,420*** |
| Quarters 13 – 16 | -3,273*** | -3,553*** | -3,684*** | -4,459*** |
| Number of States | 26 | 24 | 20 | 19 |
| Sample Size | 3,780 | 3,463 | 3,007 | 2,770 |

Source: Mathematica TAA Initial and Follow-Up Survey.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a matching algorithm shown in each column. The first column uses the full sample of 26 states used in the main analysis. The second column excludes two states where the number of names on the certified worker lists exceeded the estimate of affected workers. The third column excludes six states that had low match rates (less than 45 percent) between the certified worker lists and the UI claimant file. The fourth column excludes seven states where more than 35 percent of the certified workers had layoff dates that fell outside the date range of the state administrative data.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## D. EFFECTS OF THE RECESSION ON THE IMPACT ESTIMATES

One important piece of context for the employment and earnings impacts is the broader macroeconomic environment. During the 4-year follow-up period, the U.S. experienced a deep recession that officially started in December 2007 (according to the National Bureau of Economic Research). It is impossible to know whether the employment and earnings impacts would have differed in a stronger economy. Employment rates and earnings were probably lower during the recession than they would have been in a strong economy, but they were likely to have been lower for *both* TAA participants and comparisons. But, as we discussed in Chapter IV, one important consequence of the participants' training experience is that trainees typically reentered the labor market at a later time than comparisons. As discussed in Chapter IV, we found that more than 50 percent of trainees reentered the labor market after the onset of the recession (Exhibit IV-9).

As a subgroup analysis, we estimated labor market impacts for two groups of workers: (1) workers who filed for benefits in 2004 or 2005, and (2) workers who filed for benefits in 2006 or

later. Workers with earlier UI claims should have been less affected by the recession during their initial period of reemployment than workers with later claims.

We found the impacts of the TAA program on employment and earnings in years 3 and 4 were significantly different across the two subgroups (Tables VII-29 and VII-30). For TAA participants who filed for UI in 2006 or later, there was no impact on employment or earnings in the final follow-up year. In contrast, TAA participants who filed prior to 2006 had significantly worse employment and earnings outcomes than comparisons throughout the follow-up period. In the final year of the follow-up period, the average participant from the 2004-2005 subgroup earned about $7,250 less than the average comparison, and in the final quarter, the average participant earned $1,767 less.

Interestingly, the employment rates and earnings of the TAA participants were almost equivalent in the two subgroups. The difference in impacts across the subgroups was driven entirely by the employment outcomes of comparisons. Comparisons who filed their UI claim in 2004 or 2005 were able to return to the labor market before the economic conditions deteriorated, whereas comparison workers who lost their jobs in later years and TAA participants who engaged in training faced more challenging economic conditions. We cannot say, however, whether the impacts of the TAA program and the economic returns to training would have been different if the economic conditions had been better.

## E. EFFECTS OF DECLINING INDUSTRIES

Although TAA participants and their matched comparisons were all employed in manufacturing jobs prior to their UI claims, they were employed in a diverse set of industries. Sample members who were employed in industries in severe decline may have encountered greater difficulties becoming reemployed or finding employment at a similar compensation level than workers in industries with less steep declines or that were growing industries. Furthermore, these industry effects could have differed for the treatment and comparison groups, thereby affecting the impact findings.

For the rematching, we did not initially use information on the industry of the worker's pre-UI job. This was because the use of the three-digit North American Industry Classification System (NAICS) codes resulted in many industry categories that contained only a small number of sample members, and the use of the two-digit NAICS codes produced only a small number of categories that had no predictive power in the matching models. However, late in the project, ETA obtained state-level data from the Quarterly Census of Employment and Wages (QCEW) on the percent change in private industry employment between 2004 and 2009 (by three-digit NAICS code). We used these data to construct an *industry growth rate* measure between 2004 (the period just before most

sample members lost their jobs) and 2009 (which is a reasonable follow-up period for assessing which industries were growing or declining).[39]

Almost all TAA participants and comparisons were employed in industries that experienced substantial declines in employment between 2004 and 2009 (Table VII-31). For TAA participants, the average decline in state-level industry employment between 2004 and 2009 was 31 percent. The benchmark comparison sample was also employed in industries with significant declines in employment, but the average decline was 24.7 percent. Because of this difference, we constructed revised kernel weights that included industry growth variables in the matching model (see Chapter VI of the MN report). With the revised kernel weights, there was no significant difference between TAA participants and their matched comparisons in the industry growth rate measure (Table VII-31).

We found very similar employment and earnings impact results using the original and revised kernel weights (Table VII-32 and Table VII-33). In the fourth year following job loss, using the original weights, we found that TAA participants worked two fewer weeks than comparisons, a statistically insignificant impact. With the revised kernel weights, TAA participants worked three fewer weeks than comparisons. Similarly, the estimated earnings impact in year 4 using the original weights was -$3,273, compared to -$4,045 using the revised kernel weights and both were statistically significant.

The impact of the TAA program may depend on the employment opportunities available in a participant's pre-UI industry. Accordingly, we estimated subgroup impacts on earnings in year 4 for three subgroups defined by the industry growth measure: (1) highest decline industries (-100 to -43.5 percent); (2) industries in significant decline (-43.5 to -17.7 percent); and (3) smaller decline industries (-17.7 to 19.6 percent). As context for these subgroup definitions, national private industry manufacturing employment declined by 17.1 percent between 2004 and 2009. The first two subgroups experienced employment declines greater than the national average while the final subgroup fared better than the national average.

[39] For eight percent of the sample, we could not use the QCEW data to calculate changes in employment between 2004 and 2009 because the QCEW state-industry cell was masked for confidentiality reasons or the survey respondent reported a non-manufacturing industry. For these cases, we imputed changes in employment using a state-level regression imputation procedure where the regression models included age, gender, race, education, and pre-UI wages.

## Table VII-29: Impacts on Employment, by Year of UI Claim (Survey Data)

| | UI Claim Year in 2004 or 2005 | | | UI Claim Year in 2006 or Later | | |
|---|---|---|---|---|---|---|
| | TAA Participants | Comparison Group | Impact | TAA Participants | Comparison Group | Impact |
| Employed | | | | | | |
| Quarter 1 | 10.3 | 37.7 | -27.4*** | 6.1 | 38.7 | -32.6*** |
| Quarter 2 | 11.5 | 53.6 | -42.1*** | 8.7 | 50.0 | -41.3*** |
| Quarter 3 | 15.8 | 66.0 | -50.2*** | 15.1 | 60.8 | -45.7*** |
| Quarter 4 | 23.2 | 73.1 | -49.9*** | 23.6 | 69.7 | -46.1*** |
| Quarter 5 | 28.5 | 77.1 | -48.5*** | 31.6 | 69.5 | -37.9*** |
| Quarter 6 | 34.2 | 77.2 | -42.9*** | 37.8 | 72.9 | -35.1*** |
| Quarter 7† | 40.8 | 81.9 | -41.1*** | 42.2 | 68.7 | -26.5*** |
| Quarter 8† | 44.8 | 82.8 | -38.0*** | 46.7 | 71.5 | -24.8*** |
| Quarter 9† | 50.5 | 81.6 | -31.1*** | 51.7 | 69.7 | -18.1*** |
| Quarter 10† | 53.6 | 81.6 | -28.0*** | 54.2 | 68.4 | -14.2*** |
| Quarter 11† | 54.6 | 81.4 | -26.8*** | 58.0 | 65.8 | -7.9*** |
| Quarter 12† | 57.9 | 78.1 | -20.2*** | 60.0 | 63.2 | -3.2 |
| Quarter 13† | 60.2 | 77.2 | -17.0*** | 63.6 | 62.4 | 1.2 |
| Quarter 14 | 63.3 | 75.8 | -12.5*** | 65.2 | 66.9 | -1.7 |
| Quarter 15 | 63.8 | 77.6 | -13.8*** | 64.8 | 66.7 | -1.9 |
| Quarter 16† | 64.0 | 78.6 | -14.6*** | 65.0 | 63.7 | 1.4 |
| Weeks of Employment | | | | | | |
| Quarters 1 – 4 | 6.8 | 27.0 | -20.2*** | 5.8 | 25.5 | -19.7*** |
| Quarters 5 – 8† | 17.9 | 41.1 | -23.2*** | 19.3 | 36.3 | -17.0*** |
| Quarters 9 – 12† | 27.1 | 42.1 | -14.9*** | 28.5 | 35.2 | -6.7*** |
| Quarters 13 – 16† | 32.5 | 40.2 | -7.7*** | 33.2 | 33.0 | 0.2 |
| Sample Size, Quarters 1-12 | 558 | 530 | | 1,438 | 1,086 | |
| Sample Size, Quarters 13-16 | 558 | 530 | | 783 | 555 | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Differences in impacts of TAA among subgroups are statistically significant at the 0.05 level.

## Table VII-30: Impacts on Earnings, by Year of UI Claim (Survey Data)

| | UI Claim Year in 2004 or 2005 | | | UI Claim Year in 2006 or Later | | |
|---|---|---|---|---|---|---|
| | TAA Participants | Comparison Group | Impact | TAA Participants | Comparison Group | Impact |
| Quarterly Earnings ($2006) | | | | | | |
| Quarter 1 | 537 | 2,014 | -1,476*** | 279 | 1,980 | -1,701*** |
| Quarter 2 | 679 | 3,844 | -3,165*** | 502 | 3,922 | -3,420*** |
| Quarter 3 | 882 | 5,061 | -4,179*** | 835 | 4,697 | -3,861*** |
| Quarter 4 | 1,218 | 5,734 | -4,515*** | 1,377 | 5,259 | -3,882*** |
| Quarter 5 | 1,648 | 6,026 | -4,378*** | 1,930 | 5,498 | -3,567*** |
| Quarter 6 | 2,069 | 6,187 | -4,118*** | 2,417 | 5,632 | -3,215*** |
| Quarter 7 | 2,478 | 6,265 | -3,787*** | 2,664 | 5,529 | -2,865*** |
| Quarter 8† | 2,785 | 6,259 | -3,474*** | 2,909 | 5,411 | -2,501*** |
| Quarter 9† | 3,042 | 6,310 | -3,268*** | 3,185 | 5,335 | -2,150*** |
| Quarter 10† | 3,260 | 6,232 | -2,972*** | 3,363 | 5,142 | -1,779*** |
| Quarter 11† | 3,437 | 6,199 | -2,762*** | 3,554 | 5,099 | -1,545*** |
| Quarter 12† | 3,611 | 5,991 | -2,381*** | 3,712 | 4,935 | -1,223*** |
| Quarter 13† | 3,918 | 5,832 | -1,914*** | 3,893 | 4,344 | -451 |
| Quarter 14† | 3,877 | 5,730 | -1,854*** | 4,037 | 4,445 | -409 |
| Quarter 15† | 3,957 | 5,760 | -1,803*** | 4,132 | 4,506 | -374 |
| Quarter 16† | 4,001 | 5,768 | -1,767*** | 4,119 | 4,462 | -343 |
| Annual Earnings ($2006) | | | | | | |
| Quarters 1 – 4 | 3,284 | 16,452 | -13,168*** | 2,970 | 15,681 | -12,711*** |
| Quarters 5 – 8 | 8,891 | 24,669 | -15,778*** | 9,821 | 21,933 | -12,113*** |
| Quarters 9 – 12† | 13,163 | 24,163 | -11,000*** | 13,689 | 20,123 | -6,433*** |
| Quarters 13 – 16† | 15,601 | 22,865 | -7,264*** | 16,090 | 17,668 | -1,578 |
| Sample Size, Quarters 1-12 | 558 | 530 | | 1,438 | 1,086 | |
| Sample Size, Quarters 13-16 | 558 | 530 | | 783 | 555 | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Differences in impacts of TAA among subgroups are statistically significant at the 0.05 level.

## Table VII-31: Treatment-Comparison Differences in Industry Growth Category at Job Loss, for Original and New Kernel Weights

| | TAA Participants | Comparison Group | Difference | Standard Error |
|---|---|---|---|---|
| **Original Kernel Weight** | | | | |
| Average Percent Change in Employment in Job Loss Industry | | | | |
| -100 to -43.5 | 25.9 | 14.5 | 11.4*** | 2.6 |
| -43.5 to -29.7 | 24.5 | 18.6 | 5.9** | 2.6 |
| -29.7 to -17.7 | 24.8 | 28.5 | -3.7 | 2.8 |
| -17.7 to 19.5 | 24.8 | 38.4 | -13.6*** | 3.0 |
| (Average Percent Change in Employment) | -31.0 | -24.7 | -6.3*** | 1.0 |
| **New Kernel Weight Adjusting for Industry Decline** | | | | |
| Average Percent Change in Employment in Job Loss Industry | | | | |
| -100 to -43.5 | 25.9 | 25.9 | 0.0 | 2.9 |
| -43.5 to -29.7 | 24.5 | 23.1 | 1.4 | 2.9 |
| -29.7 to -17.7 | 24.8 | 24.6 | 0.2 | 2.9 |
| -17.7 to 19.5 | 24.8 | 26.5 | -1.6 | 2.9 |
| (Average Percent Change in Employment) | -31.0 | -30.3 | -0.7 | 1.1 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Baseline and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Standard errors account for the two-stage sampling design. State-level industry employment measures from the Quarterly Census of Employment and Wages, 2004 and 2009. Industry employment is measured at the 3-digit industry level.

*/**/*** Difference between TAA participants and comparisons is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

TAA = Trade Adjustment Assistance.

## Table VII-32: Impacts on Employment, by Kernel Weight (Survey Data)

| | Original Kernel Weight | | Kernel Weight Adjusting for State-Level Industry Decline | |
|---|---|---|---|---|
| | Comparison Group | Impact | Comparison Group | Impact |
| Employed | | | | |
| Quarter 1 | 37.5 | -30.3*** | 35.0 | -27.6*** |
| Quarter 2 | 49.7 | -40.2*** | 49.4 | -39.8*** |
| Quarter 3 | 62.1 | -46.7*** | 61.7 | -46.2*** |
| Quarter 4 | 70.1 | -46.6*** | 69.4 | -45.8*** |
| Quarter 5 | 71.4 | -40.6*** | 71.1 | -40.3*** |
| Quarter 6 | 74.6 | -37.8*** | 73.5 | -36.6*** |
| Quarter 7 | 73.6 | -31.8*** | 73.4 | -31.5*** |
| Quarter 8 | 75.1 | -28.9*** | 74.1 | -27.9*** |
| Quarter 9 | 73.3 | -22.0*** | 72.4 | -21.0*** |
| Quarter 10 | 71.9 | -17.9*** | 72.3 | -18.2*** |
| Quarter 11 | 70.4 | -13.3*** | 71.5 | -14.3*** |
| Quarter 12 | 68.0 | -8.5*** | 70.1 | -10.6*** |
| Quarter 13 | 68.0 | -5.7** | 70.8 | -9.2*** |
| Quarter 14 | 68.9 | -4.4* | 70.5 | -7.7*** |
| Quarter 15 | 69.8 | -5.4** | 70.9 | -7.4*** |
| Quarter 16 | 69.0 | -4.3* | 70.3 | -5.6** |
| Weeks of Employment | | | | |
| Quarters 1 – 4 | 25.5 | -19.4*** | 25.3 | -19.2*** |
| Quarters 5 – 8 | 37.8 | -18.9*** | 37.6 | -18.6*** |
| Quarters 9 – 1 | 37.1 | -9.0*** | 38.1 | -9.9*** |
| Quarters 13 – 16 | 35.0 | -2.0 | 36.0 | -3.1** |
| Sample Size | | 3,741 | | 3,741 |

Source: Mathematica TAA Baseline and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

TAA = Trade Adjustment Assistance.

## Table VII-33: Impacts on Earnings, by Kernel Weight (Survey Data)

| | Original Kernel Weight | | Kernel Weight Adjusting for State-Level Industry Decline | |
|---|---|---|---|---|
| | Comparison Group | Impact | Comparison Group | Impact |
| Earnings (2006$) | | | | |
| Quarter 1 | 1,964 | -1,616*** | 2,008 | -1,663*** |
| Quarter 2 | 3,838 | -3,289*** | 3,789 | -3,243*** |
| Quarter 3 | 4,732 | -3,885*** | 4,715 | -3,873*** |
| Quarter 4 | 5,353 | -4,018*** | 5,313 | -3,984*** |
| Quarter 5 | 5,589 | -3,734*** | 5,607 | -3,758*** |
| Quarter 6 | 5,730 | -3,406*** | 5,721 | -3,402*** |
| Quarter 7 | 5,712 | -3,097*** | 5,689 | -3,078*** |
| Quarter 8 | 5,657 | -2,781*** | 5,640 | -2,766*** |
| Quarter 9 | 5,586 | -2,439*** | 5,587 | -2,440*** |
| Quarter 10 | 5,371 | -2,036*** | 5,432 | -2,097*** |
| Quarter 11 | 5,306 | -1,783*** | 5,421 | -1,897*** |
| Quarter 12 | 5,173 | -1,489*** | 5,303 | -1,616*** |
| Quarter 13 | 4,866 | -964*** | 5,113 | -1,293*** |
| Quarter 14 | 4,821 | -841*** | 5,078 | -1,250*** |
| Quarter 15 | 4,852 | -782*** | 4,978 | -1,091*** |
| Quarter 16 | 4,839 | -761*** | 4,790 | -830*** |
| Annual Earnings (2006$) | | | | |
| Quarters 1 – 4 | 15,728 | -12,674*** | 15,575 | -12,539*** |
| Quarters 5 – 8 | 22,561 | -12,987*** | 22,481 | -12,927*** |
| Quarters 9 – 1 | 20,999 | -7,451*** | 20,799 | -7,285*** |
| Quarters 13 – 16 | 19,189 | -3,273*** | 17,459 | -4,045*** |
| Sample Size | | 3,741 | | 3,741 |

Source: Mathematica TAA Baseline and Follow-up Surveys.

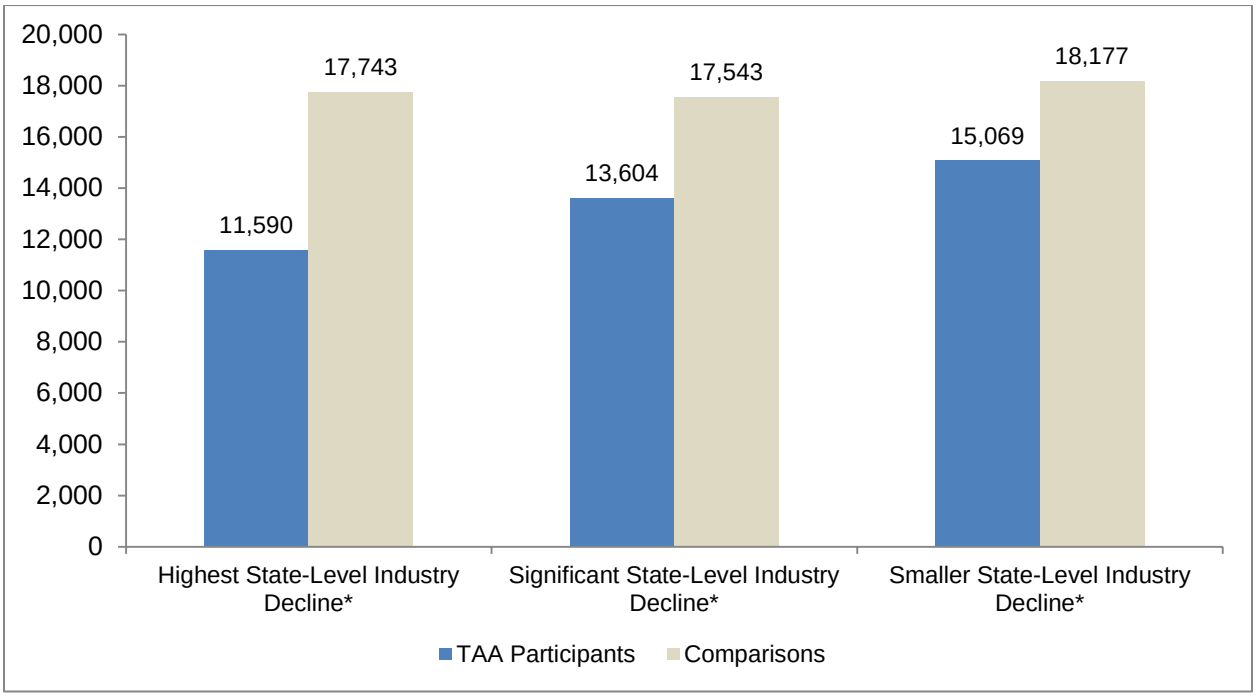
Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

TAA = Trade Adjustment Assistance.

The estimated year 4 earnings impacts were negative and statistically significant for each of the three industry subgroups (Exhibit VII-7). The difference in impacts across the three subgroups was not statistically significant. However, there is some evidence that impacts were most negative for the subgroup of workers who were employed in the highest decline industries. Comparison earnings were relatively constant across the three subgroups, but the treatment group earnings in year 4 were lower for the subgroup in the highest decline industries. We also examined employment and earnings impacts across ten, more finely grained, industry-change subgroups. For this analysis, the subgroup sample sizes were small and the impact estimates had large standard errors with no evident pattern across the subgroups (not shown).

**Exhibit VII-7: Impacts on Year 4 Earnings, by Percent Change in State-Level Industry Employment (Survey Data)**




Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. State-level industry employment measures are from the Quarterly Census of Employment and Wages, 2004 and 2009. Industry employment is measured at the 3-digit industry level.

## F. IMPACTS FOR OTHER SUBGROUPS

Differences across subgroups in program experiences, skills, backgrounds, and labor market opportunities may lead to subgroup differences in program success. This section examines whether two key employment outcomes—weeks worked and annual earnings in the fourth year after job loss—differed significantly across subgroups as defined by demographic and local area characteristics. We chose to focus on the fourth year because it is the best indication that we have of longer-term program impacts.

We expect groups with high training enrollment rates to have lower labor market outcomes in the short-term, particularly if these workers come from demographic groups where the opportunity costs of enrolling in training programs are relatively high (for example, males, prime-age workers, non-minorities, and workers with higher levels of education). The real question of interest is the impact of the TAA program on future labor market outcomes, and the fourth year after job loss is our best available estimate. In addition to the age and trainee subgroups discussed in the previous sections, we examined impacts for subgroups defined by race and ethnicity, gender, family composition, education, self-reported health status, metropolitan area residence status, and local unemployment rates, all measured at the time of job loss.

We found no significant differences in impacts by subgroups defined by gender, family composition, or education level at program entry (Table VII-34). We did, however, find some impact differences by race/ethnicity, health status, and key local area characteristics. The impact of TAA on average weeks worked in the fourth year after job loss was significantly different across racial and ethnic groups, with TAA causing a reduction in weeks worked for white participants (6 weeks) and no significant difference for any other group (Exhibit VII-8). There were also significant differences in employment rate impacts for workers with different self-reported levels of health at the time of job loss. For participants and comparisons in poor health at job loss, there was no impact on employment. In contrast, TAA participants in good health at the time of job loss worked five weeks less in year 4 than comparisons in good health (Table VII-34).

Impacts on earnings were also significantly different across groups defined by race and ethnicity, as well as by self-reported health at job loss. White TAA participants earned $6,269 less than white comparisons in the fourth year after job loss, while program impacts on earnings were not significant for Blacks, Hispanics, and those workers in other racial and ethnic groups (Table VII-35). The negative earnings impacts were also limited to workers who were in good health at job loss. While TAA had no impact on the year 4 earnings of workers in poor health, TAA participants in good health at job loss earned an average of $4,210 less than their comparisons in year 4 (Exhibit VII-9).

Although the impacts of the TAA program on weeks of employment in the fourth year did not differ significantly by workers' local area characteristics, we did find significant differences in year 4 earnings impacts across several of these subgroups. First, for this year 4 earnings measure, the TAA program was less successful for workers living outside of metropolitan areas. TAA participants in non-metropolitan areas earned an average of $5,694 less than comparisons, whereas TAA had no significant effect on the earnings of participants living in metropolitan areas (Exhibit VII-10). Second, the negative earnings impacts were limited to TAA participants living in areas with higher unemployment at the time of job loss. TAA participants living in areas with unemployment rates higher than 5.1 percent earned significantly less than their comparisons, whereas the TAA program

did not impact the fourth year earnings of workers living in areas with lower unemployment rates at the time of job loss (Exhibit VII-11).

One possible explanation for these local area impact findings is that training impacts were smaller in non-metropolitan and high unemployment areas than in other areas. Another possibility is that it may have been more difficult for participants who completed their training programs to find jobs that matched their newly-acquired skills in local areas with fewer job opportunities than in areas with greater job demand and variety.

## G. IMPACTS FOR ALL TAA-ELIGIBLE WORKERS

The impact findings on employment and earnings presented thus far pertain to TAA participants who received a significant TAA service as defined for the study. While the TAA-eligible nonparticipants in our sample received less intensive services than the TAA participants, we hypothesized at the outset of the study that the TAA nonparticipants could have been affected by the TAA program, because they may have received Rapid Response services, other early intervention services funded by the Workforce Investment Act (WIA) or Employment Services (ES), and One-Stop Career Center core services that could have obviated their need for TAA. [40] Furthermore, the TAA program devotes resources to these workers.

Consequently, it is of policy relevance to estimate impacts for the full sample of TAA-eligible workers by *combining* the TAA participant and non-participant samples and comparing their outcomes to those of their combined matched comparison samples. Thus, the resulting impact estimates will be a weighted average of the separate impact estimates for the TAA participants and the TAA-eligible non-participants.

It is important to note that the use of the combined sample also has methodological appeal (as a sensitivity analysis), because potential sample selection biases discussed earlier for the separate samples might be somewhat offsetting using the combined sample. For example, if the TAA participants decided to enroll in TAA because they were less marketable than their matched comparisons along unobservable dimensions, one would expect that the TAA-eligible non-participants who chose not to receive services to be more marketable than their matched comparisons. Thus, the direction of selection biases should be opposite for the nonparticipant and participant impacts, which could be partially offsetting for the combined impacts.

The employment and earnings impact findings for TAA-eligible workers are displayed in (1) Tables VII-36 and VII-37 for the survey sample using the survey data (for the eight-quarter follow-up period that is available for the nonparticipants and their comparisons) and (2) Tables VII-38 and VII-39 for the certified worker administrative records sample using the UI wage records (for the

[40] We have prepared a separate report that presents impact findings for TAA-eligible nonparticipants by comparing their outcomes to those of their matched comparisons.

twelve-quarter follow-up period). Because the estimated labor market impacts for the nonparticipants were small (see Section C.5 above) and the TAA participation rate among TAA-eligible workers is about 50 percent,, the estimated impacts for TAA-eligible workers were about *half* the estimated impacts for the participants. For example, using the survey sample, the estimated impact on the quarter 8 employment rate was -16.5 percentage points for TAA-eligible workers (Table VII-36), compared to -28.9 percentage points for TAA participants (Table VII-2) and both were statistically significant. Similarly, using the certified worker administrative sample and UI wage records, the estimated earnings impact in quarter 12 was a statistically significant -$610 for TAA-eligible workers (Table VII-39), compared to a statistically significant -$1,054 for TAA participants (Table VII-25). Statistical significance levels of the impact estimates are the same for the eligible and participant samples, except that the quarter 12 employment impact based on the administrative records sample became statistically insignificant using for the eligible sample.

## Table VII-34: Impacts on Weeks of Employment in Year Four for Subgroups (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 33.0 | 35.0 | -2.1 | 1.3 |
| Age† | | | | |
| 16 to 29 | 39.4 | 34.7 | 4.7 | 5.1 |
| 30 to 39 | 40.4 | 43.5 | -3.0 | 2.3 |
| 40 to 49 | 37.4 | 40.6 | -3.2** | 1.4 |
| 50 to 59 | 30.4 | 32.7 | -2.3 | 2.3 |
| 60 or over | 10.4 | 20.9 | -10.4** | 4.7 |
| Race† | | | | |
| White | 32.4 | 38.4 | -5.9*** | 1.4 |
| Black | 35.4 | 32.5 | 2.8 | 2.7 |
| Hispanic | 30.0 | 24.0 | 6.0 | 4.0 |
| Other | 28.7 | 28.3 | 0.4 | 6.4 |
| Gender | | | | |
| Female | 32.6 | 34.8 | -2.2 | 1.8 |
| Male | 33.4 | 34.7 | -1.2 | 1.7 |
| Family Composition | | | | |
| Married w/kids | 39.1 | 42.0 | -2.9 | 1.8 |
| Unmarried w/kids | 34.7 | 28.5 | 6.2** | 2.7 |
| Married no kids | 28.7 | 32.5 | -3.7* | 2.1 |
| Unmarried no kids | 28.3 | 32.0 | -3.7 | 2.4 |
| Education | | | | |
| Less Than High School | 24.7 | 30.7 | -6.0** | 2.8 |
| High School Diploma | 34.8 | 37.5 | -2.7* | 1.5 |
| Some College | 34.5 | 36.2 | -1.6 | 2.0 |
| Bachelors or More | 36.7 | 48.2 | -11.5*** | 3.4 |
| Health Status at Job Loss† | | | | |
| Good Health | 34.2 | 39.0 | -4.8*** | 1.4 |
| Poor Health | 29.0 | 27.7 | 1.4 | 2.5 |
| Metropolitan Area | | | | |
| Non-Metropolitan Area | 34.5 | 36.5 | -2.0 | 2.0 |
| Metropolitan Area | 32.1 | 34.0 | -2.0 | 1.6 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 35.5 | 36.8 | -1.3 | 1.9 |
| 4.4 to 5.1 | 32.3 | 27.4 | 4.9** | 2.3 |
| 5.1 to 6.0 | 31.0 | 37.2 | -6.3*** | 2.0 |

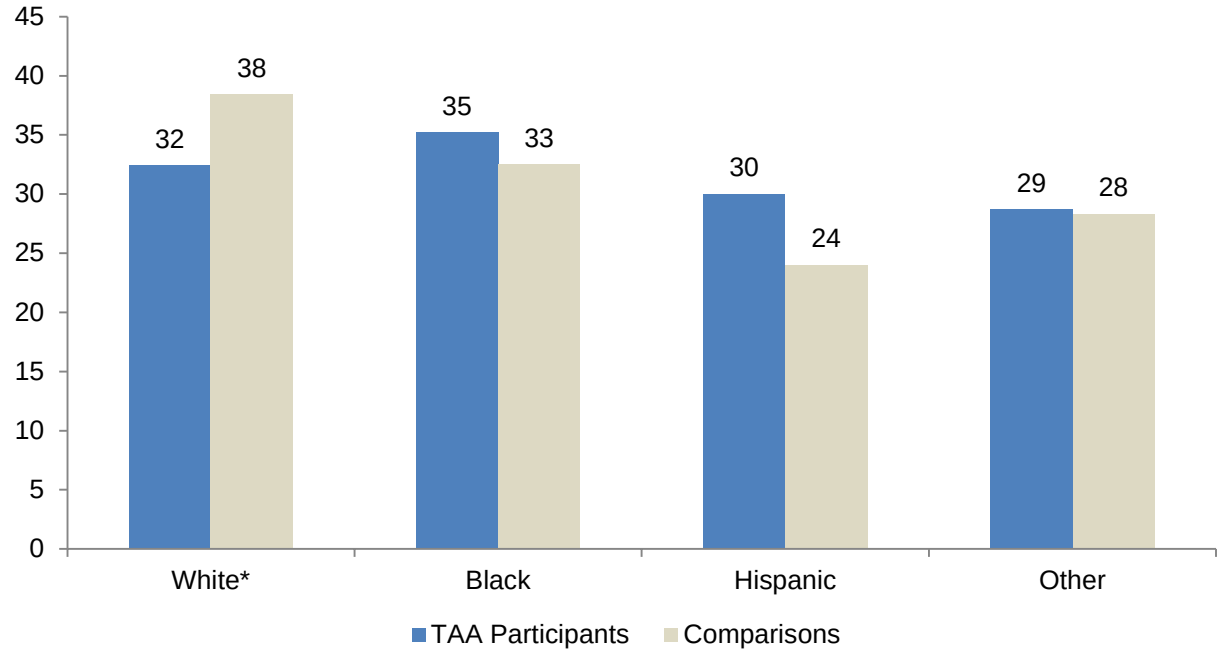
Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: The outcome for all rows is weeks of employment in the fourth year after job loss. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Exhibit VII-8: Impacts on Weeks Worked in Year 4, by Race and Ethnicity




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

## Table VII-35: Impacts on Annual Earnings in Year Four for Subgroups (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 15,917 | 19,171 | -3,255*** | 893 |
| Age | | | | |
| 16 to 29 | 18,709 | 19,400 | -691 | 3,303 |
| 30 to 39 | 19,453 | 25,710 | -6,257*** | 1,927 |
| 40 to 49 | 18,835 | 22,817 | -3,982*** | 1,252 |
| 50 to 59 | 13,835 | 16,554 | -2,719** | 1,301 |
| 60 or over | 2,957 | 9,078 | -9,078*** | 2,055 |
| Race† | | | | |
| White | 16,423 | 22,693 | -6,269*** | 1,040 |
| Black | 15,160 | 15,000 | 160 | 1,488 |
| Hispanic | 12,257 | 12,814 | -557 | 2,234 |
| Other | 16,516 | 21,690 | -5,174 | 7,851 |
| Gender | | | | |
| Female | 12,898 | 14,866 | -1,968** | 956 |
| Male | 19,320 | 23,142 | -3,822*** | 1,212 |
| Family Composition | | | | |
| Married w/kids | 21,584 | 23,673 | -2,090 | 1,548 |
| Unmarried w/kids | 14,798 | 13,743 | 1,055 | 1,579 |
| Married no kids | 12,590 | 16,343 | -3,753*** | 1,235 |
| Unmarried no kids | 12,725 | 17,335 | -4,611*** | 1,387 |
| Education | | | | |
| Less Than High School | 8,771 | 12,685 | -3,913*** | 1,039 |
| High School Diploma | 15,316 | 18,558 | -3,242*** | 997 |
| Some College | 20,248 | 22,443 | -2,195*** | 1,912 |
| Bachelors or More | 27,887 | 41,419 | -13,532*** | 6,139 |
| Health Status at Job Loss† | | | | |
| Good Health | 16,991 | 21,201 | -4,210*** | 1,044 |
| Poor Health | 12,441 | 12,681 | -240 | 1,243 |
| Metropolitan Area† | | | | |
| Non-Metropolitan Area | 14,861 | 20,555 | -5,694*** | 1,239 |
| Metropolitan Area | 16,540 | 17,703 | -1,163 | 978 |
| Local Unemployment Rate at UI Claim† | | | | |
| Less than 4.4 | 19,941 | 22,149 | -2,208 | 1,696 |
| 4.4 to 5.1 | 16,690 | 15,246 | 1,445 | 1,567 |
| 5.1 to 6.0 | 13,778 | 22,475 | -8,697*** | 1,339 |
| Greater than 6.0 | 14,326 | 19,562 | -5,236*** | 1,426 |

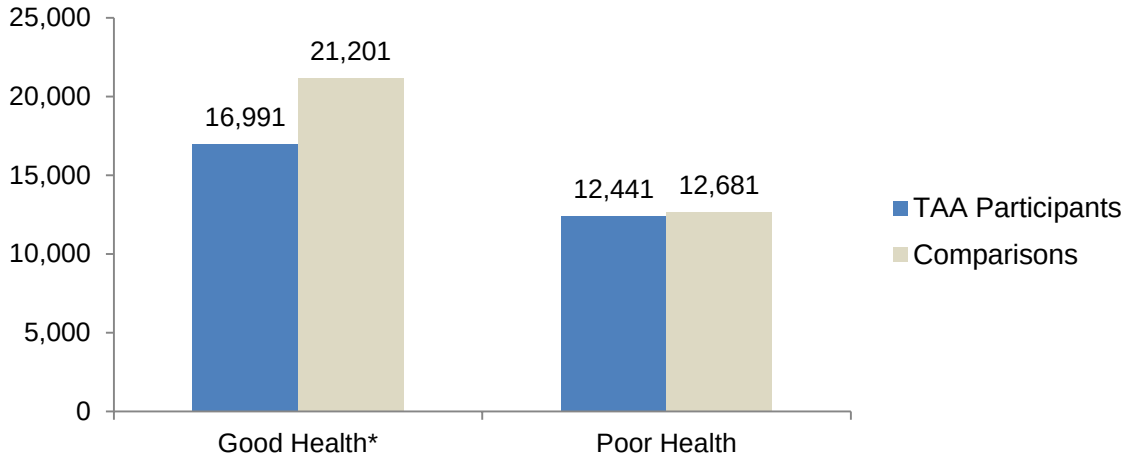
Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: The outcome for all rows is annual earnings in 2006 dollars in the fourth year after job loss. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Exhibit VII-9: Impact on Annual Earnings in Year 4, by Self-Reported Health Status at Job Loss

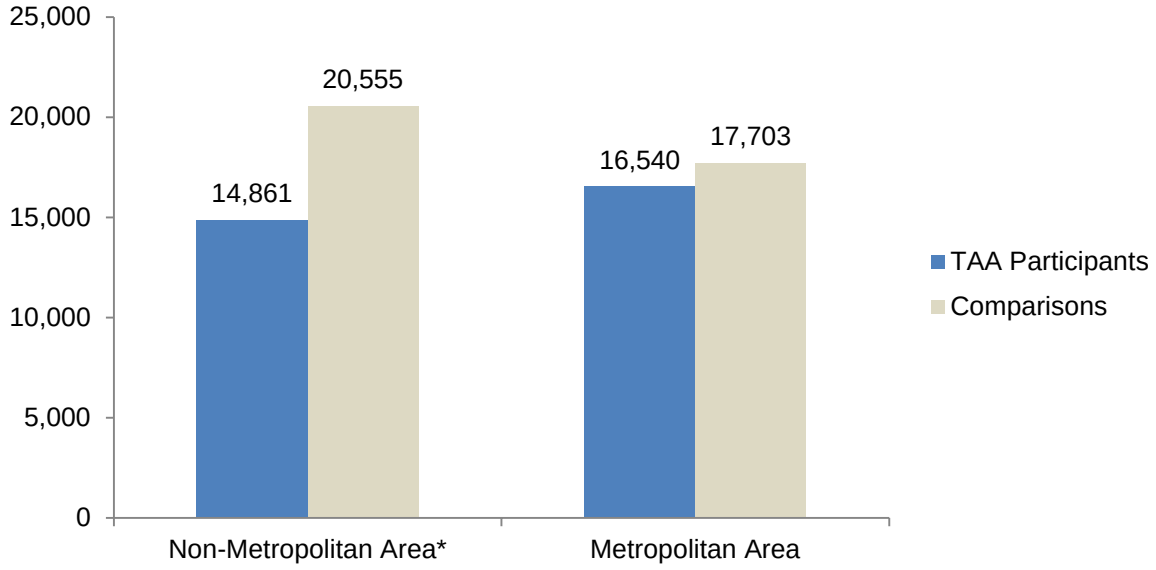



Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

## Exhibit VII-10: Impact on Annual Earnings in Year 4, by Metropolitan Area Residence

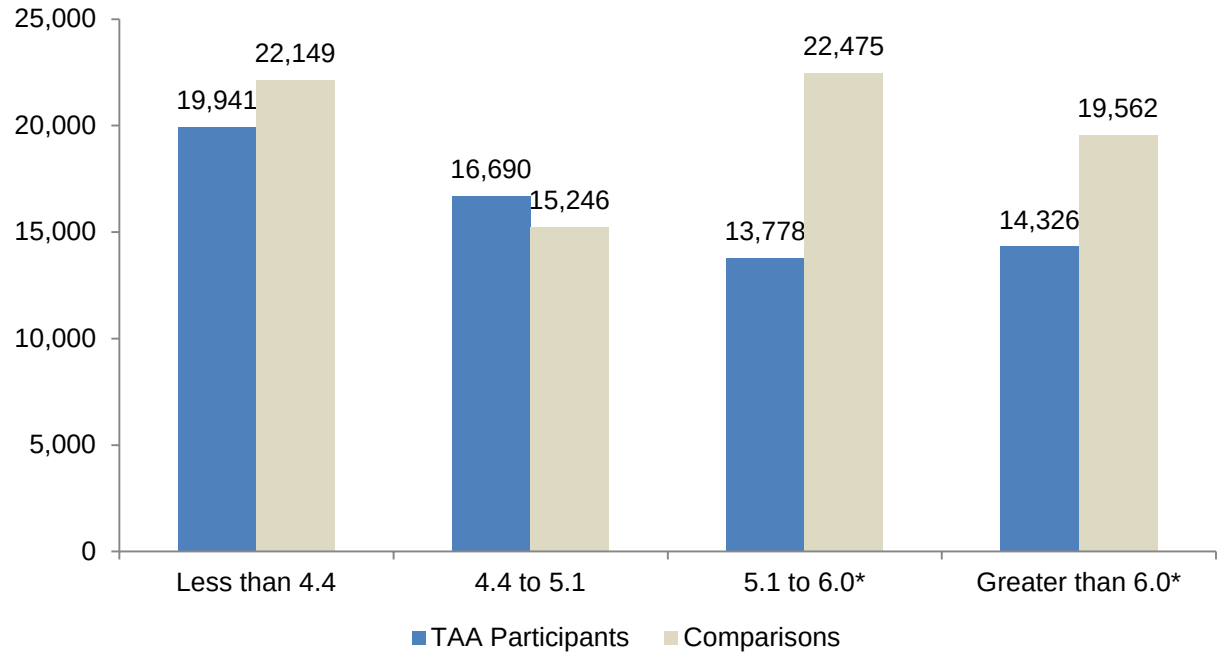



Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

## Exhibit VII-11: Impact on Annual Earnings in Year 4, by Local Unemployment Rate in Year of UI Claim




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

## Table VII-36: Impacts on Employment for TAA Eligible Workers (Survey Data)

| | TAA Eligible Workers | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Employed | | | | |
| Quarter 1 | 21.2 | 35.7 | -14.6*** | 2.0 |
| Quarter 2 | 30.7 | 50.6 | -20.0*** | 2.3 |
| Quarter 3 | 38.1 | 61.8 | -23.7*** | 2.4 |
| Quarter 4 | 44.4 | 68.2 | -23.9*** | 2.3 |
| Quarter 5 | 50.3 | 71.5 | -21.2*** | 2.3 |
| Quarter 6 | 54.1 | 74.9 | -20.9*** | 2.3 |
| Quarter 7 | 57.1 | 75.0 | -17.9*** | 2.3 |
| Quarter 8 | 58.5 | 75.0 | -16.5*** | 2.1 |
| Weeks of Employment | | | | |
| Quarters 1 – 4 | 15.4 | 25.1 | -9.7*** | 0.9 |
| Quarters 5 – 8 | 26.6 | 37.1 | -10.5*** | 1.1 |
| Sample Size | 2,667 | 3,012 | | |

Source: Mathematica TAA Baseline Survey.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA eligibility is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

TAA = Trade Adjustment Assistance.

## Table VII-37: Impacts on Earnings for TAA Eligible Workers (Survey Data)

| | TAA Eligible Workers | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Quarterly Earnings ($2006) | | | | |
| Quarter 1 | 1,161 | 2,034 | -873*** | 149 |
| Quarter 2 | 2,127 | 3,832 | -1,705*** | 211 |
| Quarter 3 | 2,727 | 4,695 | -1,968*** | 208 |
| Quarter 4 | 3,211 | 5,251 | -2,040*** | 210 |
| Quarter 5 | 3,631 | 5,503 | -1,873*** | 210 |
| Quarter 6 | 3,854 | 5,701 | -1,847*** | 212 |
| Quarter 7 | 3,965 | 5,686 | -1,721*** | 219 |
| Quarter 8 | 4,074 | 5,513 | -1,439*** | 203 |
| Annual Earnings ($2006) | | | | |
| Quarters 1 – 4 | 9,118 | 15,550 | -6,432*** | 669 |
| Quarters 5 – 8 | 14,865 | 21,549 | -6,683*** | 806 |
| Sample Size | 2,667 | 3,012 | | |

Source: Mathematica TAA Baseline Survey.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA eligibility is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

TAA = Trade Adjustment Assistance.

## Table VII-38: Impacts on Employment for TAA Eligible Workers in the Certified-Worker Administrative Sample (Administrative Data)

| | TAA Eligible Workers | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Employed | | | | |
| Quarter 1 | 58.4 | 65.6 | -7.2*** | 2.0 |
| Quarter 2 | 52.8 | 69.8 | -17.0*** | 2.2 |
| Quarter 3 | 53.7 | 71.9 | -18.2*** | 1.9 |
| Quarter 4 | 55.2 | 72.3 | -17.1*** | 1.9 |
| Quarter 5 | 56.0 | 71.7 | -15.7*** | 1.8 |
| Quarter 6 | 56.7 | 71.0 | -14.2*** | 1.9 |
| Quarter 7 | 59.3 | 70.7 | -11.4*** | 1.7 |
| Quarter 8 | 60.5 | 69.6 | -9.1*** | 1.6 |
| Quarter 9 | 61.7 | 68.2 | -6.5*** | 1.7 |
| Quarter 10 | 62.4 | 67.5 | -5.0*** | 1.6 |
| Quarter 11 | 62.9 | 66.2 | -3.3** | 1.7 |
| Quarter 12 | 62.9 | 64.9 | -2.0 | 1.6 |
| Sample Size | 18,898 | 30,021 | | |

Source: Administrative UI Claims Files and UI Wage Records.

Notes: Treatment group weights account for sample design, and comparison group weights are constructed using the original matching triads. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA eligibility is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

TAA = Trade Adjustment Assistance.

## Table VII-39: Impacts on Earnings for TAA Eligible Workers in the Certified-Worker Administrative Sample (Administrative Data)

| | TAA Eligible Workers | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Quarterly Earnings ($2006) | | | | |
| Quarter 1 | 3,883 | 4,451 | -569* | 317 |
| Quarter 2 | 3,947 | 4,995 | -1,048*** | 223 |
| Quarter 3 | 3,427 | 5,329 | -1,902*** | 203 |
| Quarter 4 | 3,682 | 5,252 | -1,570*** | 213 |
| Quarter 5 | 4,086 | 5,401 | -1,315*** | 165 |
| Quarter 6 | 3,794 | 5,377 | -1,583*** | 172 |
| Quarter 7 | 3,973 | 5,297 | -1,324*** | 197 |
| Quarter 8 | 4,066 | 5,092 | -1,026*** | 179 |
| Quarter 9 | 4,090 | 4,963 | -873*** | 179 |
| Quarter 10 | 4,103 | 5,003 | -899*** | 211 |
| Quarter 11 | 4,153 | 4,798 | -645*** | 229 |
| Quarter 12 | 4,197 | 4,807 | -610*** | 233 |
| Sample Size | 18,898 | 30,021 | | |

Source: Administrative UI Claims Files and UI Wage Records.

Notes: Treatment group weights account for sample design, and comparison group weights are constructed using the original matching triads. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA eligibility is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

TAA = Trade Adjustment Assistance.

# VIII. CHARACTERISTICS OF JOBS

Chapter VII showed that while in training, many TAA participants worked and earned significantly less than similar workers who were not eligible for TAA. As they finished training and returned to work, however, the earnings gap between them and their comparisons narrowed, though it did not close completely; TAA participants still earned about $3,000 less on average than comparisons in the final follow-up year. By this time, however, there was no significant difference in weeks of employment for TAA participants and comparisons.

Two individuals with the same amount in annual earnings could have very different labor market experiences, however. To obtain a more complete picture of the overall impacts of TAA participation on labor market outcomes, it is important to examine the characteristics of the jobs held by TAA participants and their comparisons. For example, individual workers could have held different numbers of jobs, had different hourly wages, worked different numbers of hours per week, and had access to different employment benefits. And, importantly, one individual could be in a high growth occupation, while another could be in a field that is declining.

This chapter looks at the characteristics of the jobs held by TAA participants and comparisons immediately following job loss and in the final year of the follow-up period, and addresses the following research questions:

- How quickly do TAA participants and comparisons become reemployed after their initial job losses? Does time to reemployment vary by the age of the worker or other key variables defined by demographic, program, and local area characteristics?

- What are the characteristics of first reemployment jobs? Are TAA participants more or less likely than comparisons to change industries? How do hourly wages compare to hourly wages in pre-UI jobs?

- Do the jobs held by TAA participants and comparisons in the final year of the follow-up survey differ on key characteristics, such as wages, benefits, or occupation?

- Do job characteristics differ for participants who enrolled in TAA-funded training and for TRA-only participants? Are trainees working in the occupations for which they were trained?

These questions were addressed using the certified worker follow-up survey sample and interview data. The TRA-beneficiary and certified worker administrative records samples do not include the detailed job information necessary for this analysis. Importantly, unlike the employment and earnings impacts from the previous chapter, the analysis of job characteristics is *conditional* on being employed, and thus, differences between TAA participants and comparisons should not be thought of as impacts of the TAA program.

Consistent with results from the previous chapter, we found that comparisons became reemployed significantly faster than TAA participants (36 weeks on average, compared to 84 weeks).

Furthermore, when TAA participants returned to work, they had significantly lower wages and fewer available fringe benefits than comparisons.

By the end of the follow-up period, the gap in job quality had decreased, but had not completely closed. TAA participants still had lower average hourly wages than comparisons, but the gap had narrowed from $1.69 to $0.78 per hour. TAA participants also increased their access to employer-provided benefits, although they continued to lag behind comparison workers. We found few significant differences in program effects across subgroups defined by worker demographics and local area characteristics.

Trainees fared better than TRA-only participants, although trainees still earned significantly lower average hourly wages than their matched comparisons in the final follow-up year. These suggestive results are consistent with findings from Chapter VII that showed that the participant-comparison earnings gap narrowed more quickly for trainees than for TRA-only workers. In addition, we found that 37 percent of occupational trainees were employed in their training fields in the final follow-up year, and that program effects for these trainees were somewhat better than for the average trainee.

Even though participants were catching up to comparisons in terms of the quality of their jobs, by the end of the four-year follow-up period participants still held jobs that paid significantly less and offered fewer fringe benefits than those held by comparison workers. One possible explanation for these findings is that TAA participants (especially women) were significantly more likely to have switched industries and occupations, and thus, may have been more likely than comparisons to have started in lower level positions. In addition, TAA participants, and trainees in particular, were slower to reenter the labor market than comparisons, and thus, many may have entered a different labor market than the comparisons had faced two years earlier. With high rates of unemployment, it may have been unusually difficult for trainees to find quality jobs that matched their skills. The current follow-up period is too short to determine if TAA participants moved into jobs with higher potential for long-term wage growth.

The rest of this chapter provides details on these findings. The first section examines differences in the initial reemployment of TAA participants and comparisons. In the second section, we present differences in the characteristics of jobs held in the final year of the follow-up period and explore the link between training and recent job characteristics.

## A. INITIAL REEMPLOYMENT

This section examines the distribution of time between the UI claim date and the initial reemployment date for TAA participants and their matched comparisons. This section also compares the characteristics of the first post-UI job for the two research groups.

### 1. Weeks to Reemployment

About 79 percent of both participants and comparisons ever became reemployed during the four-year follow-up period. Consistent with the overall employment impact findings presented in Chapter VII, we found that among those who became reemployed, comparisons returned to work much more quickly than TAA participants. For comparison workers, an average of 36 weeks

elapsed between their UI claims and initial reemployment dates, compared to 84 weeks for TAA participants (a significant difference of 48 weeks; Exhibit VIII-1). Similarly, within the first year after their UI claims, 80 percent of the comparisons who ever became reemployed during the follow-up period had returned to work, compared to about 30 percent for TAA participants (Exhibit VIII-2). While the share of comparisons who had found employment increased steeply in the first year, the time pattern of reemployment for TAA participants was quite different—the growth of their reemployment rates was more evenly spaced across the first three years after job loss.

We found that both the TAA trainees and the TRA-only participants took significantly longer than their matched comparisons to become reemployed, but the timing of reemployment differed across the two service subgroups (Exhibit VIII-1). TAA trainees became reemployed an average of 95 weeks after job loss, compared to 62 weeks for TRA-only workers. This result is consistent with the finding from Chapter VI that the average TAA trainee received 75 weeks of training.

We found large significant differences in the time to reemployment for all subgroups defined by demographic and local area characteristics, though in almost all cases, there were no significant differences in program effects across these subgroups (not shown). The only exception was for the subgroup defined by the workers' levels of education at program entry. Program effects on delaying reemployment were smaller for college graduates than for workers with lower levels of education (Exhibit VIII-3).

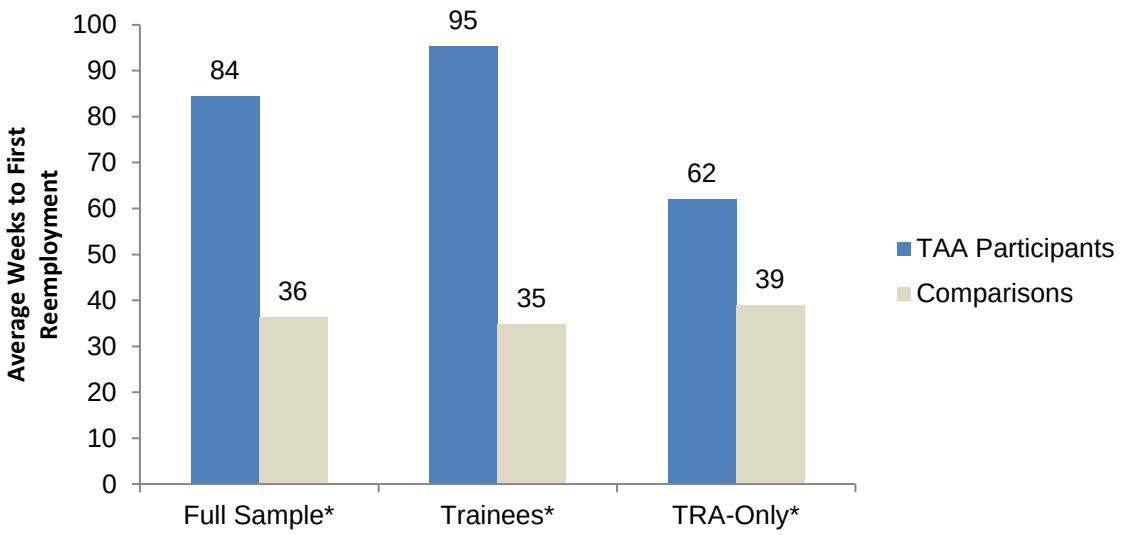
While the large participant-comparison differences in the time to reemployment are consistent with a program model that emphasizes training, we were concerned about the possibility that members of the comparison sample were drawn from industries where the possibility of recall was greater. As discussed in the previous chapter, we found that comparisons were significantly more likely to be recalled to their jobs than TAA participants (12.6 percent for comparisons, compared to 6.9 percent for participants). But, when we estimated employment impacts without recalled workers, the results did not change. As a second sensitivity analysis, we examined the time to reemployment using detailed 3-digit NAICS industry codes. Despite small sample sizes, we found that across almost every industry, comparisons were reemployed faster than TAA participants (not shown). Finally, using the sample of UI exhaustees, we also found that comparisons returned to work significantly faster than participants, where sample selection issues regarding worker "marketability" may have favored the participants (see Chapter VII).

## 2. Characteristics of Initial Jobs

In this section, we examine the hourly wages and other characteristics of the first jobs held by TAA participants and comparisons after their trigger job losses. From the previous section, we know that these first jobs were not necessarily held at the same point (because comparisons typically became reemployed faster than TAA participants), but they nevertheless provide a picture of workers' reentry into the labor market.

This analysis included all sample members who held at least one new job after their UI claims. Because we only included sample members with employment, and because TAA participation reduced employment rates, participant-comparison differences in the initial job characteristics should not be interpreted as impacts of the program.

## Exhibit VIII-1: Differences in Average Weeks to First Reemployment, for the Full Sample and by Service Subgroup (Survey Data)

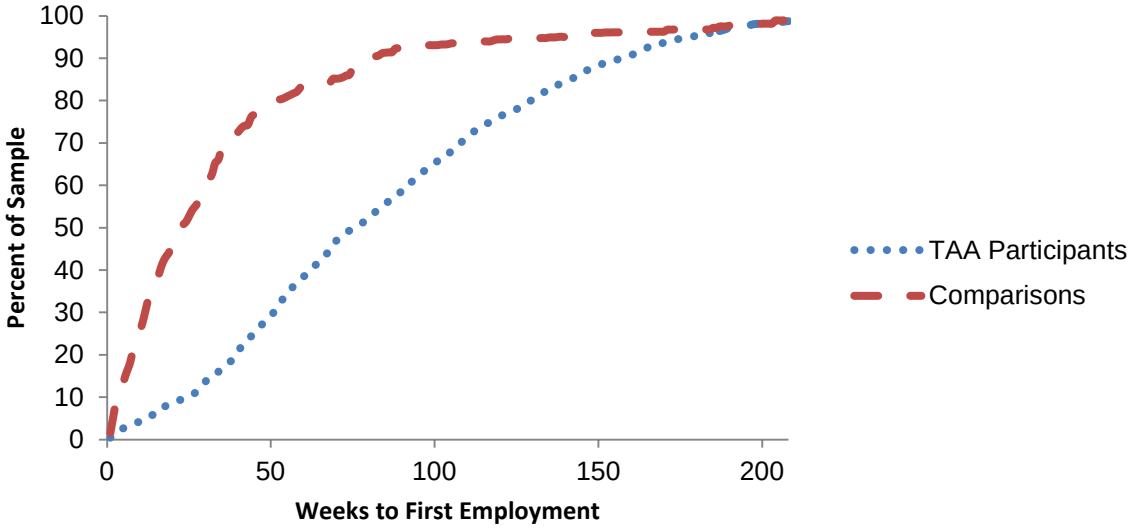



Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Difference is significantly different from zero at the 0.05 level, two-tailed test.

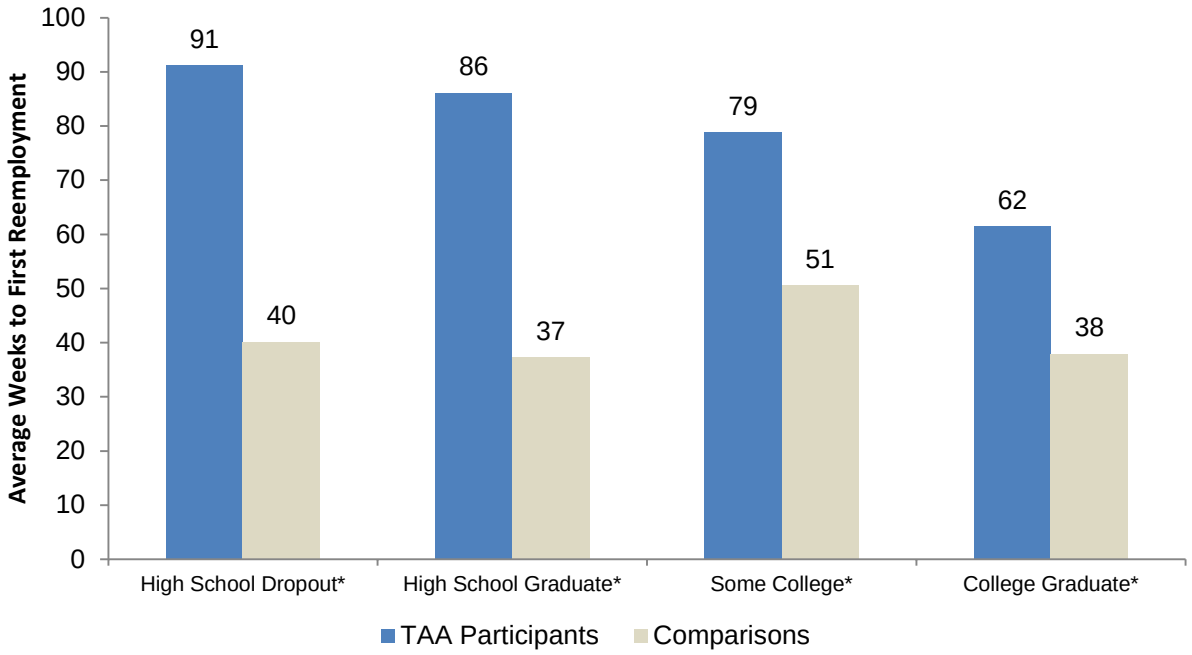
## Exhibit VIII-2: Timing of Reemployment (Survey Data)




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm.

**Exhibit VIII-3: Differences in Average Weeks to First Reemployment, by Level of Education (Survey Data)**




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Difference is significantly different from zero at the 0.05 level, two-tailed test.

In their initial post-UI jobs, the average hourly wage was $1.69 lower for TAA participants than for comparisons ($11.68, compared to $13.37; Table VIII-1). TAA participants were significantly more likely to earn under $8 per hour and significantly less likely to earn between $11 and $15, $15 to $22, and above $22 per hour.

These differences in hourly wage rates led to a significant difference in the wage replacement rate. In the first post-UI job, the average reemployed comparison earned 93 percent of his or her pre-UI wages, compared to 83 percent for the average reemployed participant (a significant difference of -10 percentage points). TAA participants also worked significantly fewer hours per week than comparisons (an average of 36 versus 40 hours); thus, participant-comparison differences in hourly wages were compounded by differences in hours worked per week.

The availability of job benefits is another important indicator of job quality. A majority of comparisons found initial reemployment in jobs that offered health insurance (71 percent), paid time off (73 percent), and retirement benefits (56 percent; Table VIII-1). TAA participants were significantly less likely to have each of these benefits available to them in their first post-UI jobs. For example, 57 percent of reemployed participants were in jobs that offered health insurance, compared to 71 percent for reemployed comparisons.

## Table VIII-1: Differences in Characteristics of Initial Reemployment Jobs (Survey Data)

| | TAA Participants | Comparison Group | Difference | Standard Error |
|---|---|---|---|---|
| Weeks to First Reemployment | 84.3 | 36.3 | 48.0*** | 2.8 |
| Hourly Wage (Percent) † | | | | |
| Less than $8.00 | 26.9 | 18.4 | 8.5*** | 2.2 |
| $8.00 – 10.99 | 31.7 | 28.4 | 3.2 | 2.4 |
| $11.00 – 14.99 | 22.4 | 28.7 | -6.3*** | 2.2 |
| $15.00 – 21.99 | 14.0 | 16.7 | -2.7* | 1.4 |
| $22.00 or More | 5.0 | 7.7 | -2.7*** | 1.0 |
| Average Hourly Wage (2006$) | 11.68 | 13.37 | -1.69*** | 0.33 |
| Ratio of Hourly Wage in Recent Job to Hourly Wage in Pre-UI Job | 83.0 | 92.5 | -9.5*** | 1.8 |
| Average Hours per Week | 36.4 | 39.8 | -3.4*** | 0.6 |
| Unionized | 7.3 | 11.0 | -3.7*** | 1.4 |
| Employer Provided Benefits | | | | |
| Job Offered Health Insurance | 56.8 | 71.3 | -14.5*** | 2.4 |
| Job Offered Paid Leave | 59.7 | 73.4 | --13.7*** | 2.5 |
| Job Offered Retirement Benefits | 43.4 | 56.0 | -12.6*** | 2.6 |
| Sample Size | 1,617 | 1,381 | | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Difference is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Effects of TAA on distribution of categories are statistically significant at the 0.05 level.

One possible reason that TAA participants had lower wages and fewer fringe benefits in their initial positions may be that participants, and particularly trainees, were accepting entry level positions in new industries and occupations. We did, in fact, find evidence that the TAA program shifted participants into new industries and occupations (Table VIII-2). In their first reemployment, the majority of comparisons (51 percent) were still employed in the manufacturing industry, compared to 31 percent of TAA participants (a statistically significant difference of -20 percentage points). TAA participants had moved out of manufacturing and into health care and social services, construction, educational services, and accommodation and food services industries.

A similar shift was seen in the occupational distributions of jobs held by TAA participants and comparisons (Table VIII-2). Significantly fewer TAA participants found their first jobs in production occupations (26 percent, compared to 41 percent for comparisons). TAA participants were also less likely to be working in office and administrative support positions or in management positions. Instead, they were significantly more likely to find reemployment in health care occupations, including healthcare support and practitioner positions. They also found reemployment working in the fields of building and grounds cleaning, construction, food preparation, and personal care occupations.

## B. RECENT EMPLOYMENT

Although four years is a relatively short follow-up period given the intensive nature of TAA-funded training programs, job quality may still have changed during this time frame. In this section, we examine the differences in characteristics of the most recent jobs held by TAA participants and comparisons. We limited the analysis to individuals who worked in the fourth year after job loss, because these jobs are the best indication that we have of longer-term program effects. If a worker held only one job during the follow-up period, the initial reemployment job and the most recent employment were equivalent. About 71 percent of participants and 76 percent of comparisons were employed in year 4.

### 1. Characteristics of the Most Recent Job in Year 4

In the final year of the follow-up period, employed TAA participants earned less per hour than comparisons (Table VIII-3). TAA participants reported an average hourly wage of $11.81, compared to $12.59 for comparisons, a significant difference of -$0.78. While the average wage of TAA participants was still below the average hourly wage of comparisons, the gap was smaller than at initial reemployment, primarily because the hourly wages of comparisons declined during the follow-up period (perhaps due to the recession; Exhibit VIII-4). For both TAA participants and comparisons, hourly wages in the fourth year did not replace pre-job-loss hourly wages. On average, comparisons earned 88 percent of their previous hourly wages, while TAA participants earned 84 percent (a significant difference of -4 percentage points).

## Table VIII-2: Differences in Occupations and Industries for Initial Reemployment Jobs (Survey Data)

| | TAA Participants | Comparison Group | Difference | Standard Error |
|---|---|---|---|---|
| Industry at Initial Reemployment† | | | | |
| Manufacturing | 31.6 | 51.3 | -19.6*** | 2.5 |
| Health Care and Social Assistance | 16.0 | 6.1 | 9.9*** | 1.3 |
| Administrative and Support and Waste Management and Remediation services | 9.0 | 8.9 | 0.1 | 1.4 |
| Retail Trade | 10.3 | 10.2 | 0.1 | 1.5 |
| Construction | 4.8 | 2.7 | 2.1*** | 0.8 |
| Professional, Scientific, and Technical Services | 3.0 | 3.1 | -0.2 | 0.7 |
| Educational Services | 5.0 | 2.9 | 2.1** | 0.8 |
| Transportation and Warehousing | 3.5 | 2.4 | 1.1 | 0.7 |
| Wholesale Trade | 2.5 | 1.6 | 0.9 | 0.6 |
| Accommodation and Food Services | 2.6 | 1.3 | 1.3** | 0.6 |
| Other | 11.6 | 9.4 | 2.2 | 1.5 |
| Occupation at Initial Reemployment† | | | | |
| Production | 25.5 | 40.5 | -15.0*** | 2.2 |
| Transportation and Material Moving | 13.4 | 12.3 | 1.1 | 1.5 |
| Office and Administrative Support | 11.9 | 15.0 | -3.1** | 1.5 |
| Installation, Maintenance, and Repair | 5.2 | 4.6 | 0.6 | 1.1 |
| Sales and Related | 5.9 | 5.5 | 0.5 | 0.9 |
| Building and Grounds Cleaning and Maintenance | 6.0 | 3.8 | 2.3** | 1.1 |
| Construction and Extraction | 2.8 | 1.8 | 1.0** | 0.5 |
| Management | 1.1 | 2.7 | -1.6*** | 0.6 |
| Healthcare Support | 5.3 | 2.0 | 3.3*** | 0.6 |
| Food Preparation and Serving | 4.3 | 2.3 | 2.0*** | 0.6 |
| Business and Financial Operations | 1.3 | 0.6 | 0.7*** | 0.3 |
| Healthcare Practitioner and Technical | 4.3 | 0.6 | 3.7*** | 0.6 |
| Personal Care and Services | 2.3 | 0.6 | 1.7*** | 0.5 |
| Architecture and Engineering | 1.1 | 1.3 | -0.3 | 0.3 |
| Education, Training, and Library | 2.6 | 1.7 | 0.9* | 0.6 |
| Protective Services | 1.2 | 0.5 | 0.7** | 0.3 |
| Other | 5.8 | 4.2 | 1.6* | 0.9 |
| Sample Size | 1,617 | 1,381 | | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Difference is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Effects of TAA on distribution of categories is statistically significant at the 0.05 level.

## Table VIII-3: Differences in Characteristics of Most Recent Jobs (Survey Data)

| | TAA Participants | Comparison Group | Difference | Standard Error |
|---|---|---|---|---|
| Hourly Wage (Percent) † | | | | |
| Less than $8.00 | 24.6 | 18.6 | 6.0*** | 2.0 |
| $8.00 – 10.99 | 29.4 | 33.6 | -4.2* | 2.5 |
| $11.00 – 14.99 | 25.8 | 25.7 | 0.1 | 2.4 |
| $15.00 – 21.99 | 14.4 | 15.3 | -0.9 | 1.7 |
| $22.00 or More | 5.8 | 6.8 | -1.0 | 1.0 |
| Average Hourly Wage (2006$) | 11.81 | 12.59 | -0.78*** | 0.27 |
| Ratio of Hourly Wage in Recent Job to Hourly Wage in Pre-UI Job | 83.8 | 88.3 | -4.5*** | 1.6 |
| Average Hours per Week | 37.2 | 39.5 | -2.3*** | 0.8 |
| Unionized | 8.3 | 13.9 | -5.6*** | 1.8 |
| Employer Provided Benefits | | | | |
| Job Offered Health Insurance | 67.3 | 75.9 | -8.5*** | 2.5 |
| Job Offered Paid Leave | 72.3 | 79.1 | -6.7*** | 2.5 |
| Job Offered Retirement Benefits | 56.2 | 65.2 | -9.0*** | 2.8 |
| Sample Size | 1,373 | 1,175 | | |

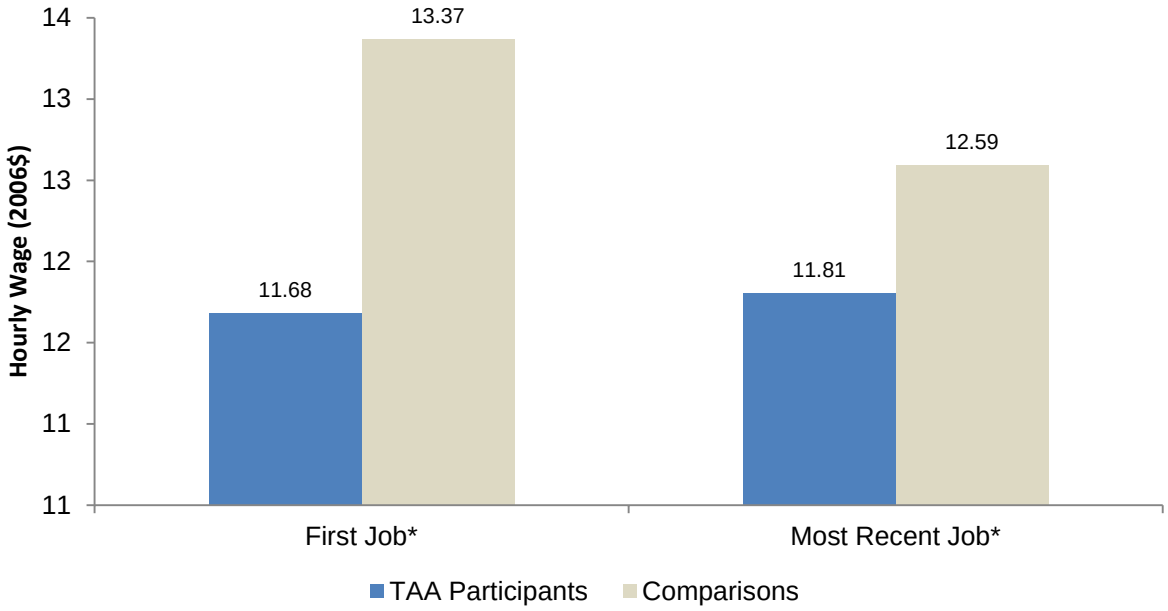
Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Difference is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Difference is significantly different across subgroups at the 0.05 level, two-tailed test.

**Exhibit VIII-4: Differences in Average Hourly Wages at Initial Reemployment and Most Recent Jobs (Survey Data)**




Source: Mathematica TAA Initial and Follow-up Surveys.

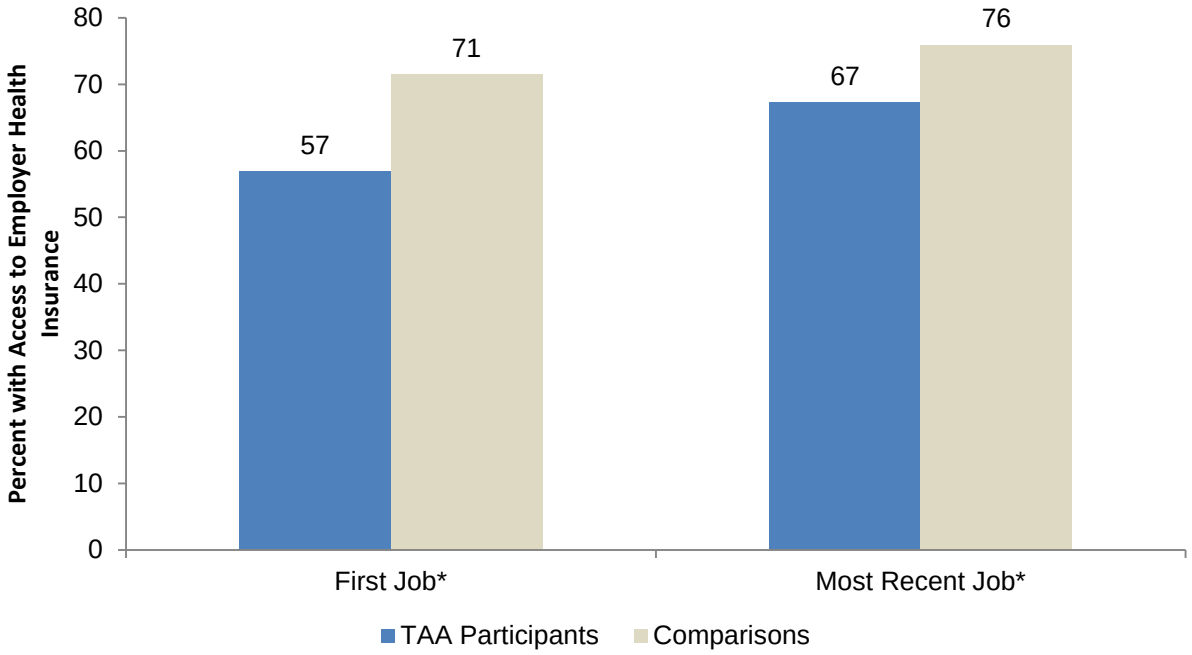
Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Difference is significantly different from zero at the 0.05 level, two-tailed test.

Similarly, the treatment-comparison gap in the percentage of workers with access to employer-provided benefits narrowed over time. In the initial reemployment position, TAA participants were approximately 14 percentage points less likely than comparisons to report access to health insurance, paid time off, or retirement benefits at work. For the most recent job, the gap in available benefits was smaller. The share of employed TAA participants with access to employer provided health insurance increased from 57 percent at initial reemployment to 67 percent at the last observed job (Exhibit VIII-5). While the participant-comparison differences in fringe benefits remained significant, the gap had narrowed somewhat.

We found significant differences in program effects on average hourly wages for subgroups defined by race/ethnicity and gender (Table VIII-4). For Black and Hispanic participants, the TAA program had no significant effect on the average hourly wage in the workers' most recent jobs. In contrast, white TAA participants earned $1.34 less per hour than their comparisons. In addition, while there was no significant difference between the average hourly wages of female TAA participants and their comparisons, male TAA participants earned significantly less than their comparisons. We did not, however, find subgroup differences in program effects for subgroups defined by age, education level, family composition, health status, or local area characteristics.

**Exhibit VIII-5: Differences in Availability of Employer Provided Health Insurance at Initial Reemployment and Most Recent Jobs (Survey Data)**




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Difference is significantly different from zero at the 0.05 level, two-tailed test.

There was not a large shift in occupations between workers' initial jobs and year 4 jobs for either the TAA participants or their matched comparisons (Table VIII-5). Thus, we continue to find significant treatment-comparison differences in the occupational distributions of the jobs that were held in year 4. Comparisons were more likely to be employed in production jobs, and TAA participants were significantly more likely to report a variety of other occupations, including jobs in healthcare support and as healthcare practitioners.

The year 4 occupational patterns were different for female and male TAA participants (Table VIII-6). Female participants were much less likely than their comparisons to have worked in production occupations, but this was not the case for male participants. In addition, the reductions in employment rates in production occupations for female participants were largely offset by increases in their employment rates in healthcare and food service and preparation occupations.

## Table VIII-4: Differences in Average Hourly Wage at Most Recent Jobs, by Worker Subgroup (Survey Data)

| | TAA Participants | Comparison Group | Difference | Standard Error |
|---|---|---|---|---|
| Full Sample | 11.81 | 12.59 | -0.78 | 0.30 |
| Age | | | | |
| 16 to 29 | 11.37 | 11.97 | -0.60 | 0.57 |
| 30 to 39 | 12.47 | 11.88 | 0.59 | 0.51 |
| 40 to 49 | 11.88 | 12.57 | -0.69* | 0.38 |
| 50 to 59 | 11.48 | 12.38 | -0.90* | 0.47 |
| 60 or over | 9.79 | 10.97 | -1.18 | 2.07 |
| Race† | | | | |
| White | 12.18 | 13.52 | -1.34*** | 0.34 |
| Black | 10.75 | 10.81 | -0.06 | 0.46 |
| Hispanic | 11.23 | 11.63 | -0.40 | 0.64 |
| Other | 14.04 | 18.5 | -4.5 | 4.32 |
| Gender† | | | | |
| Female | 10.23 | 10.19 | 0.03 | 0.25 |
| Male | 13.46 | 14.82 | -1.37*** | 0.43 |
| Family Composition | | | | |
| Married w/kids | 12.81 | 13.36 | -0.55 | 0.57 |
| Unmarried w/kids | 11.21 | 11.13 | 0.08 | 0.45 |
| Married no kids | 11.69 | 12.99 | -1.30*** | 0.42 |
| Unmarried no kids | 10.91 | 12.50 | -1.59*** | 0.40 |
| Education | | | | |
| Less Than High School | 9.69 | 9.84 | -0.15 | 0.38 |
| High School Diploma | 11.05 | 11.73 | -0.68*** | 0.26 |
| Some College | 14.01 | 14.46 | -0.45 | 0.57 |
| Bachelor's or More | 18.60 | 19.06 | -0.45 | 2.01 |
| Health Status at Job Loss | | | | |
| Good Health | 12.04 | 12.97 | -0.93*** | 0.30 |
| Poor Health | 10.76 | 10.68 | 0.08 | 0.43 |
| Metropolitan Area | | | | |
| Non-Metropolitan Area | 10.67 | 11.54 | -0.88** | 0.34 |
| Metropolitan Area | 12.53 | 13.41 | -0.88** | 0.41 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 12.95 | 13.22 | -0.27 | 0.64 |
| 4.4 to 5.1 | 12.84 | 14.18 | -1.35*** | 0.46 |
| 5.1 to 6.0 | 11.49 | 12.72 | -1.27** | 0.58 |
| Greater than 6.0 | 10.39 | 11.59 | -1.19*** | 0.35 |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: The outcome for all rows is the average hourly wage at the respondents' most recent jobs. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Difference is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Effect of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table VIII-5: Differences in Occupations in Most Recent Jobs (Survey Data)

| | TAA Participants | Comparison Group | Difference | Standard Error |
|---|---|---|---|---|
| Occupation in Most Recent Job† | | | | |
| Production | 22.1 | 34.7 | -12.6*** | 2.5 |
| Transportation and Material Moving | 11.1 | 11.7 | -0.6 | 1.5 |
| Office and Administrative Support | 13.8 | 14.3 | -0.4 | 2.0 |
| Installation, Maintenance, and Repair | 5.4 | 5.5 | -0.1 | 1.2 |
| Sales and Related | 6.4 | 5.6 | 0.8 | 1.2 |
| Building and Grounds Cleaning and Maintenance | 5.8 | 5.0 | 0.8 | 1.2 |
| Construction and Extraction | 2.3 | 0.7 | 1.6 | 1.0 |
| Management | 1.7 | 2.5 | -0.8 | 0.6 |
| Healthcare Support | 5.3 | 3.0 | 2.3** | 1.1 |
| Food Preparation and Serving | 4.4 | 2.4 | 2.1*** | 0.8 |
| Business and Financial Operations | 2.2 | 2.0 | 0.2 | 0.5 |
| Healthcare Practitioner and Technical | 5.3 | 1.5 | 3.8*** | 0.9 |
| Personal Care and Services | 0.0 | 0.0 | 2.9* | 1.6 |
| Architecture and Engineering | 1.7 | 0.9 | 0.8*** | 0.3 |
| Education, Training, and Library | 2.4 | 1.6 | 0.8 | 0.8 |
| Protective Services | 1.6 | 2.1 | -0.5 | 0.6 |
| Other | 6.4 | 7.5 | -1.1 | 1.2 |
| Sample Size | 1,364 | 1,171 | | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Difference is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Effect of TAA on distribution of categories is statistically significant at the 0.05 level.

## Table VIII-6: Differences in Occupations in Most Recent Jobs, by Gender (Survey Data)

| | Females | | | Males | | |
|---|---|---|---|---|---|---|
| | TAA Participants | Comparison Group | Difference | TAA Participants | Comparison Group | Difference |
| Occupation in Most Recent Job† | | | | | | |
| Production | 20.3 | 43.9 | -23.6*** | 24.2 | 24.4 | -0.2 |
| Transportation and Material Moving | 4.9 | 6.5 | -1.6 | 18.0 | 20.1 | -2.2 |
| Office and Administrative Support | 21.4 | 26.7 | -5.3 | 5.5 | 3.4 | 2.0 |
| Installation, Maintenance, and Repair | 0.8 | 0.7 | 0.0 | 10.5 | 11.7 | -1.1 |
| Sales and Related | 8.9 | 7.1 | 1.8 | 3.6 | 1.8 | 1.8 |
| Building and Grounds Cleaning and Maintenance | 0.1 | 0.0 | 0.1 | 6.7 | 7.0 | -0.2 |
| Construction and Extraction | 0.3 | 0.3 | 0.0 | 4.5 | 2.3 | 2.2 |
| Management | 1.3 | 1.3 | 0.0 | 2.1 | 5.0 | -2.9** |
| Healthcare Support | 9.1 | 5.1 | 4.0 | 1.1 | 0.0 | 1.0 |
| Food Preparation and Serving | 6.5 | 3.9 | 2.5* | 2.2 | 3.2 | -0.9 |
| Business & Financial Operations | 0.0 | 0.0 | 0.0 | 2.4 | 1.7 | 0.7 |
| Healthcare Practitioner and Technical | 7.5 | 1.3 | 6.2*** | 2.8 | 1.2 | 1.6 |
| Personal Care and Services | 0.1 | 0.0 | 0.0 | 0.8 | 1.3 | -0.5 |
| Architecture and Engineering | 0.0 | 0.2 | -0.2 | 3.7 | 2.6 | 1.0 |
| Education, Training, and Library | 3.2 | 3.2 | -0.1 | 1.6 | 2.1 | -0.5 |
| Protective Services | 0.7 | 2.2 | -1.6*** | 2.6 | 2.9 | -0.3 |
| Other | 5.1 | 0.7 | 4.4** | 7.8 | 9.2 | -1.4 |
| Sample Size | 694 | 590 | | 670 | 581 | |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Difference is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Effects of TAA on distribution of categories are statistically significant at the 0.05 level and the effect of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## 2. Job Characteristics for Trainees and TRA-Only Workers

As described in Chapter VI, TAA-funded training can include classroom training, on-the-job training, customized training designed to meet the needs of a specific employer or group of employers, apprenticeship programs, postsecondary education and remedial education. Through any of these forms, this training aims to increase workers' general human capital as well as prepare them for specific occupations.

While our results for the service receipt subgroups are only suggestive, we found that by the end of the follow-up period, participant-comparison differences in job characteristics were smaller for TAA trainees than for TRA-only participants (Table VIII-7). The program effect on the average hourly wage for the most recent job was still negative and statistically significant for trainees (-$0.60 per hour), but the wage gap was half the size of the gap for TRA-only participants (-$1.30 per hour) (Exhibit VIII-6). However, there were no significant differences between program effects on access to employer-provided fringe benefits across the trainee and TRA-worker subgroups.

We also examined the variation in program effects on recent hourly wages for trainees enrolled in the six most common occupational training programs: office and administrative support; healthcare support; installation, maintenance, and repair; healthcare practitioners; production; and transportation and material moving. Trainees who enrolled in office and administrative support or healthcare support training programs earned significantly less than their matched comparisons (Table VIII-8). For all other training programs, program effects on average hourly wages were not significant, although small sample sizes led to imprecise estimates.

It is important to note the limitations of this analysis. First, as discussed in Chapter II, because of potential selection biases due to decisions made by both participants and TAA counselors regarding the types of program services that participants received, the program service subgroup findings must be viewed as merely suggestive of true program effects. This is especially true for the analysis of program effects by occupational choice, because TAA participants were not randomly assigned to training programs. Certain programs, like healthcare practitioner courses, likely have steeper entry requirements than many other training programs. Thus, there are likely to be differences between trainees who select different training occupations that are unobservable and difficult to model. A second caveat is that training programs vary significantly in duration (Berk 2011). For example, the average healthcare practitioner training program was 13 months, compared to 2 months for a transportation training program. The variation in duration means that certain trainees have had more time to adjust back into the labor market.

An important policy issue is the extent to which workers who enrolled in occupational training were able to find employment in their intended occupations. We found that among TAA occupational trainees who were working in the final year of follow-up, 37 percent were employed in the occupations that they trained for (Table VIII-9). Importantly, the likelihood that an occupational trainee was employed in his or her training field varied by the occupational focus of the training program. Approximately one third of trainees who enrolled in programs for office and administrative support, healthcare support, or installation, maintenance and repair found employment in their training fields. In contrast, more than 50 percent of trainees in the other three most common programs—healthcare practitioners, production, or transportation and material moving—were likely to be employed in those fields.

## Table VIII-7: Characteristics of Most Recent Jobs, by Program Service Subgroup (Survey Data)

| | All TAA Participants | | Trainees | | TRA Only | |
|---|---|---|---|---|---|---|
| | Comparison Group | Difference | Comparison Group | Difference | Comparison Group | Difference |
| Hourly Wage (Percent) | | | | | | |
| Less than $8.00 | 18.6 | 6.0*** | 17.6 | 6.0*** | 20.4 | 6.9** |
| $8.00 – 10.99 | 33.6 | -4.2* | 31.5 | -3.1 | 34.3 | -2.0 |
| $11.00 – 14.99 | 25.7 | 0.1 | 27.8 | -1.7 | 21.6 | 3.5 |
| $15.00 – 21.99† | 15.3 | -0.9 | 17.3 | -0.7 | 15.6 | -6.0*** |
| $22.00 or More | 6.8 | -1.0 | 5.9 | -0.5 | 8.0 | -2.4* |
| Average Hourly Wage (2006$) † | 12.59 | -0.78*** | 12.60 | -0.62** | 12.50 | -1.31*** |
| Ratio of Hourly Wage in Recent Job to Hourly Wage in Pre-UI Job | 88.3 | -4.5*** | 88.6 | -3.8** | 89.1 | -6.9*** |
| Average Hours Worked per Week | 39.5 | -2.3*** | 39.8 | -2.2*** | 39.3 | -2.5*** |
| Unionized | 13.9 | -5.6*** | 15.1 | -5.8*** | 11.0 | -3.6* |
| Employer Provided Benefits | | | | | | |
| Job Offered Health Insurance | 75.9 | -8.5*** | 76.5 | -8.6*** | 72.9 | -4.8 |
| Job Offered Paid Leave | 79.1 | -6.7*** | 80.3 | -7.8*** | 75.7 | -2.2 |
| Job Offered Retirement Benefits | 65.2 | -9.0*** | 66.5 | -9.0*** | 62.8 | -6.8* |
| Sample Size | 1,373 [a] | | 885 [a] | | 353 [a] | |

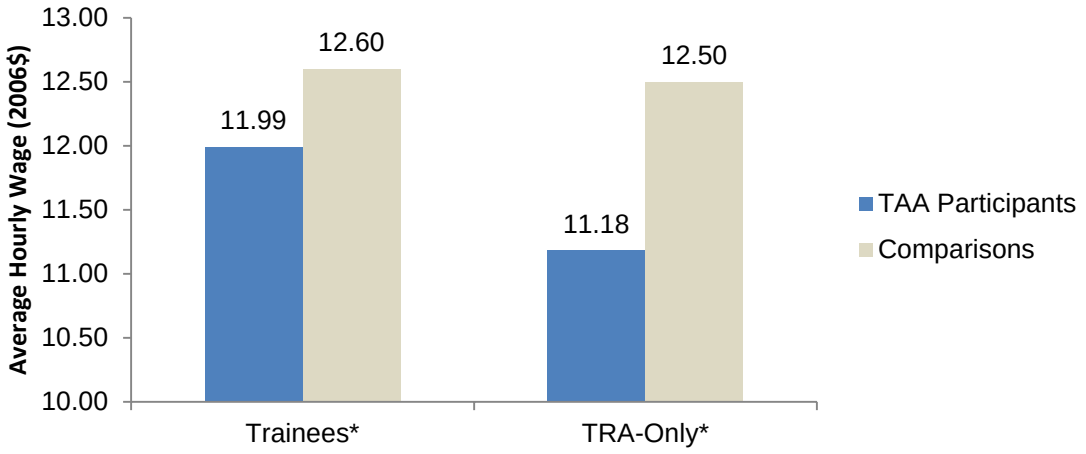
Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]Sample size for the TAA participant subgroup.

*/**/*** Difference is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Exhibit VIII-6: Differences in Average Hourly Wage in Most Recent Job, by Program Service Subgroup (Survey Data)




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Difference is significantly different from zero at the 0.05 level, two-tailed test.

## Table VIII-8: Differences in Average Hourly Wage in Most Recent Job, by Occupational Training Program (Survey Data)

| | TAA Participants | Comparison Group | Difference | Standard Error |
|---|---|---|---|---|
| Full Sample | 11.81 | 12.62 | -0.81*** | 0.27 |
| All Trainees | 11.99 | 12.60 | -0.62** | 0.29 |
| Occupational Trainees† | | | | |
| Office and Administrative Support | 11.64 | 12.92 | -1.27** | 0.64 |
| Healthcare Support | 10.65 | 13.04 | -2.38*** | 0.59 |
| Installation, Maintenance, and Repair | 13.32 | 12.95 | 0.38 | 0.66 |
| Healthcare Practitioners and Technical | 12.78 | 13.64 | -0.86 | 0.85 |
| Production | 14.27 | 12.74 | 1.54 | 1.08 |
| Transportation and Material Moving | 11.92 | 12.94 | -1.02 | 0.75 |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Difference is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Effect of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table VIII-9: Percentage of Trainees Employed in Their Training Fields, by Occupational Focus of Training Program (Survey Data)

| | Number of Trainees | Percentage of TAA Trainees Employed in their Training Field |
|---|---|---|
| All Occupational Trainees | 654 | 36.6 |
| Occupational Focus of Training Program | | |
| Office and Administrative Support | 131 | 29.8 |
| Healthcare Support | 115 | 29.5 |
| Installation, Maintenance, and Repair | 94 | 33.3 |
| Healthcare Practitioners and Technical | 82 | 55.6 |
| Production | 52 | 66.3 |
| Transportation and Material Moving | 58 | 50.0 |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: The analysis is limited to TAA participants who enrolled in occupationally-focused training programs after their trigger job losses. Treatment group weights account for sample design and nonresponse.

An important policy issue is whether program effects on job quality were more favorable for trainees who became employed in their training fields than for other trainees. We found this to be case: for occupational trainees working in their respective fields, there was no significant difference in the average hourly wages between the trainees and their matched comparisons, whereas these effects were significant for all trainees (Table VIII-10). Moreover, there were no significant differences in average hours worked or access to fringe benefits for trainees employed in their fields of training. Again, these results must be interpreted carefully because of the sample selection issues discussed above. However, the results do suggest that longer-term follow-up is needed in order to assess the full returns of the TAA program on labor market activity for trainees in general and for the subgroup of trainees who found jobs in their fields of training.

## Table VIII-10: Differences in Job Characteristics for Most Recent Jobs, for Trainees Employed in Their Training Fields (Survey Data)

| | All TAA Participants | | All Trainees | | Occupational Trainees Employed in Training Fields in Year 4 | |
|---|---|---|---|---|---|---|
| | Comparison Group | Difference | Comparison Group | Difference | Comparison Group | Difference |
| Hourly Wage (Percent) | | | | | | |
| Less than $8.00 | 18.6 | 6.0*** | 17.6 | 6.0*** | 12.7 | -3.5 |
| $8.00 – 10.99 | 33.6 | -4.2* | 31.5 | -3.1 | 28.5 | -1.8 |
| $11.00 – 14.99 | 25.7 | 0.1 | 27.8 | -1.7 | 31.1 | 4.9 |
| $15.00 – 21.99 | 15.3 | -0.9 | 17.3 | -0.7 | 18.3 | 2.3 |
| $22.00 or More | 6.8 | -1.0 | 5.9 | -0.5 | 9.4 | -1.9 |
| Average Hourly Wage (2006$) | 12.59 | -0.78*** | 12.60 | -0.62** | 13.29 | 0.01 |
| Ratio of Hourly Wage in Recent Job to Hourly Wage in Pre-UI Job | 88.3 | -4.5*** | 88.6 | -3.8** | 88.2 | 1.7 |
| Average Hours Worked per Week | 39.5 | -2.3*** | 39.8 | -2.2*** | 40.7 | -0.6 |
| Unionized | 13.9 | -5.6*** | 15.1 | -5.8*** | 14.8 | -2.2 |
| Employer Provided Benefits | | | | | | |
| Job Offered Health Insurance | 75.9 | -8.5*** | 76.5 | -8.6*** | 79.0 | -2.3 |
| Job Offered Paid Leave | 79.1 | -6.7*** | 80.3 | -7.8*** | 81.0 | 0.3 |
| Job Offered Retirement Benefits | 65.2 | -9.0*** | 66.5 | -9.0*** | 68.6 | -3.7 |
| Sample Size | | 1,373[a] | | 885[a] | | 248[a] |

Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]Sample size for the TAA participant subgroup.

*/**/*** Difference is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

# IX. IMPACTS ON OTHER OUTCOMES

Job loss has the potential to affect many aspects of workers' lives. Family members of displaced workers may begin new jobs or work longer hours in order to offset the reduction in household income. Workers may also be eligible for various public benefits or pensions, and health insurance coverage, frequently obtained through an employer, may become difficult to maintain after job loss.

In addition to its core purpose of helping trade-affected displaced workers find reemployment in suitable lines of work, some provisions of the TAA program *directly* aim to reduce the burdens on these workers after job loss. TRA payments are intended to provide continued unemployment benefits to trainees and workers who need more time to become reemployed than regular UI payments allow. The HCTC aims to assist workers in maintaining health care coverage after job loss. Relocation allowances may encourage participants to move in order to seek training or new employment.

TAA may also have indirect effects on financial, family composition, and health-related outcomes. TRA benefits may lessen workers' overall financial burdens in the short term, which may reduce the need to draw on other income sources. On the other hand, additional time spent in training may have the opposite effect by delaying more stable income streams. Furthermore, the impact of TAA on job outcomes may lead to indirect effects on total income and health insurance coverage in the long term. Given that the previous chapter documented a negative impact of TAA on employment and earnings, we might expect participants to have lower total incomes and less health insurance coverage throughout the study period. This chapter explores the overall impact of participation in TAA services on these outcomes.

This chapter addresses three primary research questions:

- How does TAA affect sources of income other than the workers' earnings in the years after job loss?
- Does TAA participation influence family structure, housing status, or mobility?
- What is the overall impact of the TAA program on participants' health and health insurance status?

We addressed these questions using survey and administrative data for the survey sample of TAA participants and their comparisons. We used survey reports of public benefit and pension receipt, spouse and partner employment status, marital status, household size, overall health status, health conditions that limit work, and the types and duration of health insurance. Administrative records also provided outcomes for UI and TRA claims payments received by participants and their comparisons.

We found that TAA had significant impacts on some sources of income. Participants collected more in UI claims payments and were more likely to exhaust their benefits, most likely because TRA enabled them to receive additional payments while they completed training or sought employment.

However, TAA had a negative impact on total income, suggesting that the TRA payments did not fully compensate for the lower earnings that participants experienced during the study period when many were completing training.

TAA did not impact family structure; participants and their comparisons tended to retain the same family structures and housing throughout the study period. Despite the availability of HCTC benefits (which only 14 percent of participants used), the loss of health insurance coverage from jobs reduced health insurance coverage among TAA participants, especially for trainees. This negative impact on the availability of health insurance decreased somewhat later in the study period. There were no impacts on coverage for those in the worst health, and we did not find any program impacts on workers' self-reported health status.

In general, we found that the impacts on other study outcomes did not vary across demographic, local area, and program service subgroups. This suggests that the full-sample findings discussed above hold true for a broad range of population subgroups.

The rest of this chapter provides more detail on these findings. The first section presents impacts on income sources other than participants' earnings, including UI and TRA payments, other public benefits, pension benefits, and spouses' and partners' earnings. The second section examines outcomes related to family structure, housing, and mobility, while the third section presents impacts on health and health insurance. The final section further explores impacts on these outcomes for specific subgroups.

## A. IMPACTS ON RECEIPT OF OTHER INCOME SOURCES

TAA participants and their matched comparisons likely faced a difficult financial reality upon job loss. The average household income of both groups was about $42,000 in the year before job loss, $28,000 of which came from the worker's earnings that year. Forty percent reported having an employed spouse at the time, suggesting that the lost job was likely the primary source of household income. That sharp drop in income, possibly combined with the loss of job-sponsored benefits and the threat of long unemployment spells, meant that these workers needed to identify other financial resources to cover their living expenses.

Displaced workers typically have several options for offsetting lost income. Unemployment Insurance is available for workers meeting various requirements related to prior employment and current activity. Families may also be eligible for other government benefits such as food stamps (Supplemental Nutrition Assistance Program, or SNAP), cash assistance, or public housing programs. Many workers may also have access to retirement benefits accrued through previous jobs, such as a pension or 401(k) plan. Family members may also be able to begin working or increase the number of hours worked in order to increase total household income.

TAA may affect the receipt of these benefits through multiple channels. TRA benefits are intended to offset lost wages while participants complete training programs after regular UI benefits are exhausted. This additional income source may reduce the need for other financial assistance. However, Chapter VII reported that TAA participants experienced worse employment outcomes than their matched comparisons, so we might expect that these participants would be more likely to seek these other income sources.

## 1. Impacts on UI and TRA receipt

UI benefits are intended to provide temporary assistance to workers who become unemployed involuntarily. Eligible workers can collect UI benefits for up to 26 weeks in most states, provided they meet eligibility requirements and remain unemployed through no fault of their own. Workers collect benefits weekly until they exhaust a maximum benefit amount determined by their earnings before job loss. TRA benefits become available to TAA-eligible workers (typically for an additional 78 weeks for trainees) after UI benefits are exhausted. While TAA participants may receive other TAA services without receiving TRA, 93 percent of our sample received some TRA benefits according to the UI/TRA claims data.

The comparison sample faced similar job and earnings losses as the treatment sample, and both research samples were UI recipients. Furthermore, as discussed in Chapter II, the workers' maximum UI benefit amounts were used in the matching models, so the treatment and comparison samples were balanced on this baseline benefit amount (which was, on average, just under $8,000). Comparisons were not eligible for TRA benefits, but may have been eligible for other UI-related programs in their states. As shown in Exhibit IX-1 and Table IX-1, we found that the average matched comparison worker collected about $8,200 in total UI benefits during the 12 quarters after job loss. This total reflects any UI benefits that were received both during and after their initial UI claims. However, the majority of UI payments were collected during the first two quarters following the job loss. Just over half exhausted their first claims (Table IX-1), as determined by UI/TRA claims data indicating a zero remaining claim balance.

TAA participants showed a similar pattern of UI benefit receipt, but collected significantly more, on average, than the comparison group during the quarter of job loss and each of the subsequent six quarters (Exhibit IX-1 and Table IX-1). During the trigger quarter and the twelve-quarter follow-up period, participants collected nearly $3,000 more in UI benefits than comparisons, including the initial and subsequent claims ($11,154 versus $8,221). Furthermore, TAA participation increased the likelihood of exhausting UI benefits by 37 percentage points; 83 percent of participants collected their maximum benefit, compared to fewer than half of comparisons. This result was expected because most TAA participants received TRA payments.

Exhibit IX-2 and Table IX-2 show the impact of TAA on the combined receipt of UI and TRA benefits. Because comparisons were not eligible for TRA, only UI benefits were included in the calculations for the comparisons.[41] The results indicate that TAA participants collected significantly more in combined benefits than comparisons up to the tenth quarter after job loss. The inclusion of TRA benefits nearly doubled the total unemployment payments that participants received to almost $20,000 over 13 quarters, or $11,000 more than the total UI benefits collected by comparisons.

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[41] The sample for the combined UI/TRA benefit outcomes is limited to states that provided complete UI and TRA data for the period of interest; thus, the comparison group differs slightly between the two lines in Exhibit IX-2 and the means in Tables IX-1 and IX-2.

## Table IX-1: Impacts on Receipt of Unemployment Insurance (Administrative Data)

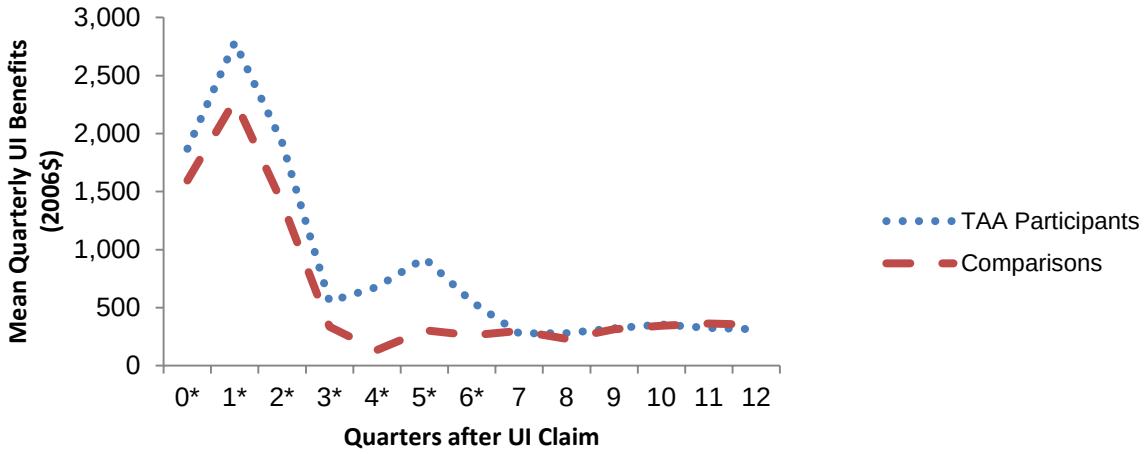
| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Average Benefits Received (2006$) | | | | |
| Trigger Quarter | 1,869 | 1,597 | 270** | 133 |
| Quarter 1 | 2,786 | 2,286 | 500*** | 113 |
| Quarter 2 | 1,920 | 1,403 | 517*** | 110 |
| Quarter 3 | 554 | 334 | 221*** | 55 |
| Quarter 4 | 677 | 135 | 542*** | 47 |
| Quarter 5 | 926 | 305 | 621*** | 65 |
| Quarter 6 | 550 | 262 | 287*** | 52 |
| Quarter 7 | 277 | 299 | -22 | 54 |
| Quarter 8 | 280 | 231 | 49 | 44 |
| Quarter 9 | 322 | 312 | 10 | 54 |
| Quarter 10 | 356 | 343 | 14 | 48 |
| Quarter 11 | 325 | 362 | -37 | 45 |
| Quarter 12 | 314 | 352 | -38 | 46 |
| Total Benefits Received, Trigger – Q12 (2006$) | 11,154 | 8,221 | 2,933*** | 363 |
| Exhausted UI Benefits (Percent) | 82.6 | 45.3 | 37.3*** | 2.5 |
| Sample Size | 1,958 | 1,581 | | |

Source: State UI Administrative Data.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the second follow-up survey and for whom UI administrative data provide complete information for all quarters.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## Exhibit IX-1: Mean UI Benefits Received

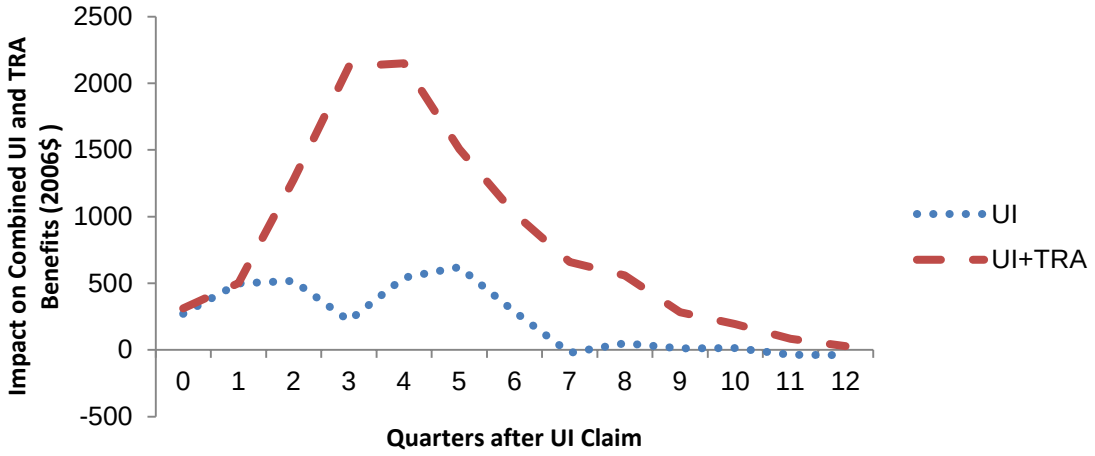



Source: State UI Administrative Data.

Notes: The sample is limited to participants for whom UI administrative data are available for the trigger quarter and subsequent 12 quarters. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

## Exhibit IX-2: Impact on UI and TRA Benefits Received




Source: State UI and TRA Administrative Data.

Notes: For the UI outcome, the sample is limited to participants for whom UI administrative data are available for the trigger quarter and subsequent 12 quarters. For combined UI/TRA outcome, the sample is further restricted to states for which TRA data are available for this time. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Impacts are regression adjusted. Standard errors account for the two-stage sampling design. The impact of TAA on combined UI/TRA is significantly different from zero at the 0.05 level for the trigger quarter and subsequent 10 quarters.

## Table IX-2: Impacts on Receipt of Combined UI and TRA Benefits (Administrative Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Average Benefits Received (2006$) | | | | |
| Trigger Quarter | 2,046 | 1,735 | 311** | 154 |
| Quarter 1 | 2,897 | 2,395 | 502*** | 113 |
| Quarter 2 | 2,733 | 1,456 | 1,277*** | 111 |
| Quarter 3 | 2,427 | 301 | 2,126*** | 112 |
| Quarter 4 | 2,285 | 134 | 2,151*** | 101 |
| Quarter 5 | 1,870 | 364 | 1,506*** | 93 |
| Quarter 6 | 1,333 | 309 | 1,024*** | 87 |
| Quarter 7 | 996 | 336 | 660*** | 81 |
| Quarter 8 | 815 | 257 | 558*** | 68 |
| Quarter 9 | 639 | 356 | 283*** | 71 |
| Quarter 10 | 593 | 401 | 193*** | 64 |
| Quarter 11 | 502 | 417 | 84 | 59 |
| Quarter 12 | 433 | 405 | 28 | 59 |
| Total Benefits Received, Trigger – Q12 (2006$) | 19,569 | 8,866 | 10,703*** | 512 |
| Sample Size | 1,566 | 1,437 | | |

Source: State UI and TRA Administrative Data.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the second follow-up survey and for whom UI and TRA administrative data provide complete information for all quarters.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

## 2. Impacts on Other Public Assistance Benefits

Food stamp (SNAP), cash assistance,[42] and public housing benefits are available to families with limited income and assets and who meet other requirements. While job loss is not a prerequisite for receiving these means-tested benefits, displaced workers may find that their loss of income makes them eligible. Some cash assistance benefits, such as Social Security Retirement, are only eligible to individuals who are no longer working and meet the age requirements. TAA may affect participation in these programs because of program effects on earnings and total income.

Few workers in our sample participated in public assistance programs in the calendar year *before* job loss (see Chapter II). Only three percent of comparisons collected food stamp benefits at that time, while 10 percent collected some form of cash assistance. This is not surprising, because most sample members had household incomes during the pre-UI period that disqualified them from being eligible for food stamps, TANF, or General Assistance benefits. However, the sharp drop in family income noted above had the potential to make workers in our sample eligible for such benefits after they lost their jobs.

Table IX-3 shows that 15 percent of comparisons reported collecting food stamp benefits at any time between job loss and the follow-up interview date. The average comparison worker received benefits for two months, totaling about $500; those who received any benefits collected $3,400 on average.

A slightly larger proportion of TAA participants than comparisons—18 percent versus 15 percent—collected food stamp benefits between job loss and the follow-up survey (Table IX-3). This 3 percentage point impact is statistically significant at the 1 percent level. TAA participation increased total benefit receipt by $200 and average duration of receipt by one month. Both participants and comparisons were more likely to collect food stamps later in the period than earlier, perhaps suggesting that some workers' financial situations worsened in the years after job loss.

Table IX-4 shows that almost 30 percent of comparisons ever collected cash assistance benefits between job loss and the follow-up survey. As with food stamp receipt, more comparisons received cash assistance later in the follow-up period than earlier. While the survey data do not provide information that can be used to distinguish between different forms of cash assistance, Social Security retirement benefits are likely to account for a substantial fraction, as 90 percent of comparisons age 60 and over reported collecting assistance, far more than for other age groups. The average comparison worker collected cash assistance benefits for 8 months, totaling almost $6,000.

[42] Here, cash assistance refers to TANF, welfare, Social Security retirement benefits, Supplementary Security Income, and General Assistance.

## Table IX-3: Impacts on Receipt of Food Stamp (SNAP) Benefits (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Received During First Follow-Up Period (Percent) | 10.3 | 7.4 | 2.9*** | 1.1 |
| Received During Second Follow-Up Period (Percent) | 16.1 | 13.2 | 2.9* | 1.5 |
| Received Since Job Loss (Percent) | 18.0 | 14.9 | 3.1** | 1.5 |
| Months Received† (Percent) | | | | |
| 0-3 months | 82.0 | 86.5 | -4.4*** | 1.3 |
| 3-6 months | 4.6 | 4.0 | 0.5 | 0.8 |
| 6-12 months | 3.5 | 4.3 | -0.9 | 0.7 |
| More than 12 months | 9.9 | 5.2 | 4.8*** | 1.0 |
| Average Months Received | 3.2 | 2.2 | 1.0*** | 0.4 |
| Total Amount Received† (Percent) | | | | |
| None | 82.0 | 85.7 | -3.7*** | 1.4 |
| $1- 1,999 | 7.7 | 7.9 | -0.2 | 1.1 |
| $2,000-9,999 | 8.9 | 5.4 | 3.5*** | 1.0 |
| $10,000 or more | 1.3 | 0.9 | 0.4* | 0.2 |
| Average Amount Received (2006$) | 699 | 498 | 201** | 97 |
| Average Amount Received, Food Stamp Recipients[a] (2006$) | 3,890 | 3,440 | 450 | 408 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The first follow-up period is the time between job loss and the first follow-up interview and is an average of 29 months. The second follow-up period is the time between the first and second follow-up interviews and is an average of 22 months.

[a]This value is conditional on food stamp receipt, so the difference cannot be interpreted as an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

## Table IX-4: Impacts on Receipt of Cash Assistance (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Received During First Follow-Up Period (Percent) | 15.2 | 16.6 | -1.4 | 1.6 |
| Received During Second Follow-Up Period (Percent) | 23.1 | 26.8 | -3.7** | 1.8 |
| Received Since Job Loss (Percent) | 26.3 | 29.5 | -3.2* | 1.9 |
| Months Received (Percent) | | | | |
| 0-3 months | 73.8 | 71.0 | 2.9* | 1.7 |
| 3-6 months | 2.4 | 3.6 | -1.2 | 0.8 |
| 6-12 months | 2.0 | 1.6 | 0.4 | 0.4 |
| More than 12 months | 21.8 | 23.8 | -2.0 | 1.6 |
| Average Months Received | 7.4 | 8.4 | -1.1 | 0.7 |
| Total Amount Received (Percent) | | | | |
| None | 73.8 | 72.0 | 1.8 | 1.7 |
| $1- 4,999 | 8.0 | 9.0 | -1.0 | 1.3 |
| $5,000-19,999 | 8.6 | 8.2 | 0.3 | 1.1 |
| $20,000 or more | 9.6 | 10.8 | -1.2 | 1.1 |
| Average Amount Received (2006$) | 4,683 | 5,612 | -929 | 687 |
| Average Amount Received, Cash Assistance Recipients[a] (2006$) | 17,889 | 18,289 | -400 | 1,331 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The first follow-up period is the time between job loss and the first follow-up interview and is an average of 29 months. The second follow-up period is the time between the first and second follow-up interviews and is an average of 22 months.

[a]This value is conditional on cash assistance receipt, so the difference cannot be interpreted as an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

TAA participants were three percentage points less likely to have collected any cash assistance benefits during the four-year follow-up period, although this difference is not statistically significant at the 5 percent level (Table IX-4). TAA did not have a statistically significant impact on total cash assistance benefit receipt or duration of receipt.

## 3. Impacts on Pension Benefit Receipt

Displaced workers may have access to pension benefits from their former employers. Just over 80 percent of our sample reported being offered retirement or pension benefits while employed at the jobs leading to their trigger claims, and earlier jobs may have also offered similar benefits. While pension benefits are designed to be used during retirement, in many cases some or all of the benefits may be collected or withdrawn earlier, sometimes for a fee or penalty. While we do not have data on the types of pension benefits these workers had or any early withdrawal penalties they faced, claiming pension benefits before normal retirement age may indicate that a worker is having difficulty covering living expenses with other income sources. Since participants and comparisons were laid off from similar jobs in similar industries, their opportunities to collect pension benefits were likely similar at the time of job loss.

Thirty percent of comparisons collected pension benefits between job loss and the follow-up interview (Table IX-5). While pension receipt was most common among workers age 60 and over, a substantial fraction of younger workers also reported collecting benefits, suggesting that some workers withdrew pension benefits early (see section D).

TAA had no overall impact on the receipt of pension benefits during the four-year follow-up period. During this time, 30 percent of comparisons ever received pension benefits, compared to 32 percent of participants (Table IX-5). We found, however, that participants were more likely to receive pension benefits between job loss and the *initial* interview (26 percent, compared to 20 percent for comparisons), but this difference became statistically insignificant in the more recent period. There were also no significant impacts on the average number of months that workers received pension benefits (about 4.5 months for each research group) or on the average pension amount received ($5,400 for participants and $4,400 for comparisons).

## 4. Impacts on Spouse and Partner Income

Family members of workers who lose their jobs may be able to change their own employment status to compensate for lost family income. In particular, a spouse or partner may be able to find a job if not already employed, or work additional hours if already employed. TAA services are offered to eligible individuals, and benefits do not vary based on the marital or employment status of other family members. However, participation in TAA may influence family members' employment decisions through its impact on overall family income.

## Table IX-5: Impacts on Receipt of Pension Benefits (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Received During First Follow-Up Period (Percent) | 26.0 | 19.6 | 6.5*** | 2.2 |
| Received During Second Follow-Up Period (Percent) | 20.1 | 18.6 | 1.5 | 1.8 |
| Received Since Job Loss (Percent) | 32.1 | 29.6 | 2.4 | 2.3 |
| Months Received† (Percent) | | | | |
| Lump sum payment | 67.9 | 72.2 | -4.2** | 2.0 |
| 0-6 months (excluding lump sum payments) | 14.2 | 11.1 | 3.1* | 1.6 |
| 6-12 months | 5.0 | 3.8 | 1.2 | 0.9 |
| More than 12 months | 12.9 | 12.9 | -0.1 | 1.3 |
| Average Months Received | 4.4 | 4.6 | -0.2 | 0.6 |
| Total Amount Received (Percent) | | | | |
| None | 68.4 | 71.9 | -3.5* | 2.0 |
| $1- 4,999 | 10.6 | 10.7 | 0.0 | 1.6 |
| $5,000-19,999 | 12.9 | 10.4 | 2.5* | 1.4 |
| $20,000 or more | 8.0 | 7.0 | 1.0 | 1.0 |
| Average Amount Received (2006$) | 5,386 | 4,406 | 980 | 627 |
| Average Amount Received, Pension Recipients[a] (2006$) | 17,071 | 16,603 | 467 | 1,588 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The first follow-up period is the time between job loss and the first follow-up interview and is an average of 29 months. The second follow-up period is the time between the first and second follow-up interviews and is an average of 22 months.

[a]This value is conditional on pension receipt, so the difference cannot be interpreted as an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

Forty percent of comparisons reported having an employed spouse or partner at the time they lost their jobs. On average, comparisons reported little net change in spouses' or partners' employment status at the time of the follow-up interview. Table IX-6 shows that 39 percent of comparisons reported having a spouse or partner working, mostly full-time, and on average earning just under $3,000 per month.

TAA participants reported outcomes very similar to comparisons on spouses' and partners' earnings, with no statistically significant differences (Table IX-6). Thus, TAA does not appear to influence the labor market participation of spouses and partners.

### 5. Impacts on Total Income

The potential impacts of TAA on specific income sources make its effect on total household income difficult to predict. Chapters VI and VII showed that TAA substantially increased participants' training receipt, but reduced their earnings in the years following job loss. However, TRA payments and other income sources have the potential to offset this difference.

The average comparison group member reported receiving $42,000 in total household income in the calendar year before the UI trigger claim. As shown in Table IX-7, these same workers reported an average total annual income of $39,000 in both 2007 and 2009, approximately one and three years after the UI claim, respectively.

TAA reduced total annual income by about $5,000 in both 2007 and 2009 (Table IX-7).[43] This is generally consistent with the finding that TAA reduced average annual earnings by about $13,000 in the first four quarters after job loss, but that UI and TRA payments made up for about $6,000 of that difference. Similarly, the negative impact on earnings in quarters 9 through 12 dropped to about $7,000, but the impact on UI and TRA payments was less than $1,000 during that time.

## B. IMPACTS ON FAMILY STRUCTURE, HOUSING, AND MOBILITY

Although there are no provisions in TAA that explicitly aim to affect housing or family structure, its impacts on income and employment may indirectly affect these outcomes. For financial reasons, job loss may affect participants' housing types, and financial and personal factors associated with job loss may impact important family decisions.

[43] Although the majority of our analyses of earnings are based on quarters after job loss, survey respondents were asked for total income in two calendar years in order to obtain more accurate responses.

## Table IX-6: Impacts on Spouse and Partner Earnings (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Spouse/Partner Working for Pay At Second Interview (Percent) | 38.2 | 38.7 | -0.5 | 2.1 |
| Hours Per Week Worked By Spouse/Partner[a]† (Percent) | | | | |
| Less than 40 | 23.7 | 18.1 | 5.7** | 2.2 |
| 40 | 59.2 | 59.7 | -0.5 | 2.9 |
| More than 40 | 17.1 | 22.3 | -5.1** | 2.3 |
| Average Hours | 39.2 | 40.2 | -1.0 | 0.8 |
| Monthly Earnings of Spouse/Partner[a] (2006$, Percent) | | | | |
| Less than $2,000 | 42.5 | 41.5 | 1.0 | 3.2 |
| $2,000-$3,999 | 42.1 | 45.2 | -3.1 | 3.5 |
| $4,000 or more | 15.4 | 13.3 | 2.1 | 2.0 |
| Average Earnings (2006$) | 2,614 | 2,788 | -174 | 127 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]Sample limited to individuals who have a spouse or partner working for pay at second follow-up interview.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

## Table IX-7: Impacts on Total Income (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Total Income, 2007† (2006$, Percent) | | | | |
| Less than $20,000 | 39.0 | 31.7 | 7.3*** | 1.9 |
| $20,000-$39,999 | 41.4 | 43.0 | -1.6 | 2.2 |
| $40,000-$59,999 | 9.7 | 8.6 | 1.1 | 1.1 |
| $60,000 or more | 9.9 | 16.7 | -6.8*** | 1.3 |
| Average Amount (2006$) | 34,321 | 39,124 | -4,803*** | 1,180 |
| Total Income, 2009 (2006$, Percent) | | | | |
| Less than $20,000 | 36.9 | 33.7 | 3.1 | 2.2 |
| $20,000-$39,999 | 41.3 | 41.6 | -0.2 | 2.5 |
| $40,000-$59,999 | 10.6 | 10.7 | -0.1 | 1.3 |
| $60,000 or more | 11.2 | 14.0 | -2.8** | 1.2 |
| Average Amount (2006$) | 33,658 | 38,795 | -5,137*** | 1,833 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

Another outcome that TAA might influence is mobility, because of changes in participants' income and marketability, and because TAA offers funding for relocation and job search allowances. Mobility is an important outcome for the evaluation, because eligible TAA workers are more likely than other workers to come from small urban areas with low population growth (Dolfin and Berk 2010). Thus, many TAA participants and their matched comparisons may have limited employment opportunities in their home areas.

At the time of job loss, 60 percent of comparison group members were married, and the average household size was 2.6 people (see Chapter II). More than half had no dependent children, and most of the others had one or two children. Almost three quarters owned their homes (including those who had mortgages), 22 percent lived in rented units, and 5 percent reported some other type of living arrangement.

Despite the substantial life changes that this group underwent, family structure and housing status for the comparison group remained very similar at the time of the follow-up interview, about four years after job loss. As shown in Table IX-8, 57 percent of comparisons were married, and the average number of children in their households was about one (Table IX-9), which is similar to the figure in the period prior to job loss. Average household size remained at 2.6 people (Table IX-10). In addition, roughly the same proportion owned homes and rented as before job loss, although a slightly higher percentage reported another living arrangement (such as living with friends or family) (Table IX-11). Furthermore, mobility for the comparison group was rare. As shown in Table IX-12, less than seven percent of these workers reported living, working, or going to school or training in other states or countries, and 93 percent lived in the same zip codes as they had at the time of job loss.

We found no impacts of TAA participation on marital status or type of residence. However, participants were more likely than comparisons to live in households with four or more individuals (24 percent, compared to 18 percent; Table IX-10), which could be a reflection of their lower incomes. Although a slightly greater proportion of participants reported living in public housing, this type of living arrangement was very rare (less than one tenth of one percent) and the treatment-comparison difference is not economically meaningful (Table IX-11).

TAA had a small negative impact on mobility, with only four percent of participants reporting living, working, or going to school or training in other states or countries, compared to seven percent for comparisons (Table IX-12). This higher mobility rate among the comparisons could reflect their higher employment rates.

## C. IMPACTS ON HEALTH STATUS AND HEALTH INSURANCE COVERAGE

Job loss may affect health in a number of ways. While some employment may have adverse effects on workers' self-reported health status, periods of unemployment and the stress that may be associated with job loss could also have an effect. In addition to indirect effects through employment, TAA may have a positive impact on health and well-being through the additional services it provides.

Health insurance coverage is closely tied to employment in the United States. Over 60 percent of the nonelderly population was covered by an employer-sponsored group health plan in 2008 (Fronstin 2009). Employer-sponsored health insurance is often heavily subsidized: employees pay on average only 16 percent of the total monthly premium for an individual plan or 26 percent for family coverage (Kaiser/HRET 2009). For many individuals, then, losing a job may lead to loss of health insurance. While the Consolidated Omnibus Budget Reconciliation Act of 1985 (COBRA) enables displaced workers to continue their employers' health coverage for up to 18 months after job separation, this unsubsidized option may be prohibitively expensive for some individuals and families.

## Table IX-8: Impacts on Marital Status (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Marital Status (Percent) | | | | |
| Married | 57.4 | 56.9 | 0.5 | 1.3 |
| Living together unmarried | 4.3 | 4.9 | -0.7 | 0.7 |
| Separated | 3.8 | 6.1 | -2.3** | 1.0 |
| Divorced | 16.9 | 15.0 | 1.9* | 1.1 |
| Widowed | 4.9 | 4.5 | 0.4 | 0.7 |
| Never married | 12.8 | 12.6 | 0.2 | 0.7 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Status is measured at the time of the second follow-up interview. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

## Table IX-9: Impacts on Number of Children (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Number of Dependent Children Under 18 (Percent) | | | | |
| None | 62.8 | 65.5 | -2.6 | 1.7 |
| 1 | 18.5 | 18.1 | 0.3 | 1.7 |
| 2 | 11.9 | 10.5 | 1.4 | 1.2 |
| 3 or more | 6.8 | 5.9 | 0.9 | 0.9 |
| Average Number | 0.7 | 0.6 | 0.0 | 0.0 |
| Number of Dependent Children 18 or Older (Percent) | | | | |
| None | 78.1 | 79.9 | -1.8 | 1.7 |
| 1 | 16.3 | 13.8 | 2.5 | 1.6 |
| 2 | 4.1 | 4.5 | -0.4 | 0.7 |
| 3 or more | 1.5 | 1.8 | -0.4 | 0.4 |
| Average Number | 0.3 | 0.3 | 0.0 | 0.0 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: All outcomes in this table are measured at the time of the second follow-up interview. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

## Table IX-10: Impacts on Household Size (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Household Size (Percent)† | | | | |
| 1 | 20.4 | 18.9 | 1.5 | 1.7 |
| 2 | 36.7 | 40.4 | -3.6 | 2.5 |
| 3 | 18.9 | 23.1 | -4.3** | 1.9 |
| 4 or more | 24.0 | 17.6 | 6.4*** | 1.6 |
| Average Number | 2.6 | 2.5 | 0.1* | 0.1 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Household size is measured at the time of the second follow-up interview. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

## Table IX-11: Impacts on Housing Status (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Type of Residence at Second Interview (Percent) | | | | |
| Owner-occupied | 72.3 | 71.3 | 1.0 | 1.3 |
| Rental | 20.8 | 21.2 | -0.4 | 1.3 |
| Other arrangement | 6.9 | 7.5 | -0.6 | 1.1 |
| House Under Foreclosure at Second Interview (Percent)[a] | 0.3 | 0.3 | 0.0 | 0.0 |
| Lived in Public Housing at Time of First Interview | 0.032 | 0.034 | -0.003*** | 0.0 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]Sample is limited to homeowners. This is a conditional outcome, and the difference between the outcomes of TAA participants and the comparison group is not an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

**Table IX-12: Impacts on Mobility (Survey Data)**

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Lived, Worked, or Went to School or Training in Another State/Country Since Job Loss (Percent) | 4.2 | 6.7 | -2.5** | 1.2 |
| Distance Moved From Zip Code at Time of Job Loss (Percent) | | | | |
| Did not move | 93.7 | 93.2 | 0.5 | 1.0 |
| Moved 50 miles or less | 4.9 | 5.5 | -0.5 | 0.9 |
| Moved more than 50 miles | 1.4 | 1.3 | 0.1 | 0.4 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: All outcomes in this table are measured at the time of the second follow-up interview. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

The TAA program's Health Coverage Tax Credit (HCTC) is intended to reduce the potential negative impact of job loss on health insurance coverage. Workers enrolled in eligible health insurance plans receive a 65 percent subsidy on plan premiums. While this subsidy is substantial, the remaining employee share is still greater than the average for currently employed individuals, which could explain the low take-up rate of HCTC for our participant sample. Furthermore, a family's financial situation during unemployment may affect health insurance decisions. Lack of health insurance would increase out-of-pocket medical costs for each service received, but uninsured individuals may delay care in order to avoid such expenses. Delaying care may, in turn, lead to worse health.

## 1. Impacts on Health

Sample members were relatively healthy while they were still working. Most of the comparison group reported being in good or excellent health at the time their jobs ended, with 18 percent reporting fair health and 4 percent reporting poor health. Nearly all (95 percent) had been offered health insurance through their jobs, and 90 percent were enrolled in some type of health insurance at the time of job loss.

The comparison group reported being in similar health during the two follow-up periods, with 30 percent reporting better overall health status at the follow-up interview than at job loss, and 13 percent reporting worse overall health (Table IX-13). Two thirds were in good or excellent health at both interviews. Table IX-14 shows that just over one in ten reported that they had a health condition that limited the amount of work that they could do, most commonly a physical disability or illness. The majority of these conditions had developed before job loss (Table IX-15).

TAA had no impact on improvements or declines in self-reported health status, and two thirds of both participants and comparisons reported being in good or excellent health at the follow-up interview (Table IX-13). However, TAA had a 3 percentage point positive impact on the fraction of individuals reporting a work-limiting condition at the follow-up interview (16 percent for participants, compared to 13 percent for comparisons; Table IX-14), and among those with a condition, participants were more likely than comparisons to report having had that condition for less than two years.

## 2. Impacts on Health Insurance and Medical Expenditures

Fewer members of the comparison group had health insurance after job loss than when they were still working. Only half were covered by health insurance continuously throughout the first follow-up period (between job loss and the initial interview), and almost 20 percent did not have any health insurance during that time (Table IX-16). The situation was similar for the second follow-up period (Table IX-17), and more than one in 10 remained uninsured for the entire follow-up period (Table IX-18). The average comparison worker was covered by health insurance for about three years during the four-year follow-up period (Table IX-18).

Among comparisons covered by health insurance during the first follow-up period, about half were insured through their employers, 30 percent through family members, and a small fraction each through individual private plans or government sponsored plans (Table IX-16). Coverage through government plans was slightly more common during the second follow-up period (Table IX-17). Comparisons reported spending an average of $1,200 in out-of-pocket medical expenses in the 12 months before the first interview and $1,400 in the 12 months before the second interview (Table IX-19).

TAA had a negative impact on health insurance coverage overall. Participants were six percentage points less likely to be covered at any time in the full follow-up period (Table IX-18), although they had caught up to comparisons by the second interview (Table IX-17). Just over one third of participants were continuously insured, six percentage points less than for the comparisons. TAA reduced health insurance coverage by an average of five months during the full follow-up period.

## Table IX-13: Impacts on Self-Reported Health (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Self-Reported Health At First Interview (Percent) | | | | |
| Excellent | 19.5 | 17.5 | 2.1* | 1.2 |
| Good | 54.0 | 58.0 | -4.0** | 2.0 |
| Fair | 20.2 | 19.3 | 0.9 | 1.5 |
| Poor | 6.3 | 5.2 | 1.1 | 0.8 |
| Self-Reported Health At Second Interview (Percent)† | | | | |
| Excellent | 20.3 | 13.6 | 6.7*** | 1.6 |
| Good | 49.3 | 59.1 | -9.8*** | 2.5 |
| Fair | 23.2 | 21.7 | 1.5 | 1.8 |
| Poor | 7.2 | 5.6 | 1.6 | 1.0 |
| Self-Reported Health Since Job Loss (Percent) | | | | |
| Good or excellent at both interviews | 62.4 | 64.3 | -1.8 | 2.2 |
| Poor at either interview | 10.0 | 8.2 | 1.8 | 1.3 |
| Better at second interview than at job loss | 29.5 | 30.1 | -0.6 | 2.1 |
| Worse at second interview than at job loss | 15.4 | 13.3 | 2.1 | 1.4 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

## Table IX-14: Impacts on Health Conditions at Second Interview (Survey Data)

| | TAA Participants | Comparison Group | Difference | Standard Error |
|---|---|---|---|---|
| Had Health Condition That Limits Work At Second Interview (Percent) | 16.2 | 12.9 | 3.3** | 1.6 |
| Type of Condition[a] (Percent) | | | | |
| Physical disability or illness | 90.3 | 93.9 | -3.6 | 2.3 |
| Emotional or mental health problem | 22.3 | 25.1 | -2.8 | 4.0 |
| Drug or alcohol problem | 1.0 | 1.2 | -0.2*** | 0.1 |
| Learning disability | 13.5 | 12.9 | 0.5 | 2.8 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]Sample limited to individuals reporting a health condition at respective interview. This is a conditional outcome, and the difference between the outcomes of TAA participants and the comparison group is not an impact. Individuals may report more than one condition.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Difference in distribution of categories between TAA participants and comparison group is statistically significant at the 0.05 level.

## Table IX-15: Duration of Health Conditions (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Longest Duration of Health Problem[a]† (Percent) | | | | |
| Less than 2 years | 17.1 | 11.1 | 6.0*** | 2.3 |
| 2-5 years | 25.7 | 32.7 | -7.0* | 4.2 |
| 5-10 years | 17.2 | 15.4 | 1.8 | 2.9 |
| More than 10 years | 40.0 | 40.8 | -0.8 | 3.9 |
| Average Duration[a] (Years) | 13.7 | 15.5 | -1.8 | 1.7 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]Sample limited to individuals reporting a health condition at either interview. This is a conditional outcome, and the difference between the outcomes of TAA participants and the comparison group is not an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Difference in distribution of categories between TAA participants and comparison group is statistically significant at the 0.05 level.

## Table IX-16: Impacts on Health Insurance During First Follow-Up Period (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Covered By Health Insurance At Any Time (Percent) | 69.9 | 81.5 | -11.6*** | 2.0 |
| Covered By Health Insurance For Entire Period (Percent) | 43.3 | 51.2 | -7.8*** | 2.2 |
| Months Covered By Health Insurance | 15.7 | 20.0 | -4.2*** | 0.7 |
| Primary Type of Health Insurance (Percent)[a]† | | | | |
| Employer, union, or school | 45.6 | 55.5 | -9.9*** | 2.4 |
| Through family member | 34.6 | 30.5 | 4.1** | 2.1 |
| Individual private plan | 7.3 | 7.1 | 0.3 | 1.2 |
| Government-sponsored | 12.4 | 6.9 | 5.5*** | 1.3 |
| Sample Size | 1,803 | 1,602 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Individuals who completed the follow-up survey but not the initial survey are excluded. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The first follow-up period is the time between job loss and the first follow-up interview and is an average of 29 months. The second follow-up period is the time between the first and second follow-up interviews and is an average of 22 months.

[a]Sample limited to individuals with any health insurance. This is a conditional outcome, and the difference between the outcomes of TAA participants and the comparison group is not an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Difference in distribution of categories between TAA participants and comparison group is statistically significant at the 0.05 level.

## Table IX-17: Health Insurance During Second Follow-Up Period (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Covered By Health Insurance At Any Time (Percent) | 74.9 | 75.8 | -0.8 | 2.2 |
| Covered By Health Insurance For Entire Period (Percent) | 54.8 | 62.3 | -7.5*** | 2.5 |
| Months Covered By Health Insurance | 15.0 | 15.3 | -0.4 | 0.6 |
| Primary Type of Health Insurance (Percent)[b]† | | | | |
| Employer, union, or school | 48.1 | 50.8 | -2.7 | 2.6 |
| Through family member | 25.9 | 27.6 | -1.7 | 2.1 |
| Individual private plan | 7.0 | 2.4 | 4.6*** | 1.0 |
| Government-sponsored | 19.1 | 19.2 | -0.2 | 2.0 |
| Sample Size | 1,803 | 1,602 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Individuals who completed the follow-up survey but not the initial survey are excluded. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The first follow-up period is the time between job loss and the first follow-up interview and is an average of 29 months. The second follow-up period is the time between the first and second follow-up interviews and is an average of 22 months.

[a]Sample limited to individuals with any health insurance. This is a conditional outcome, and the difference between the outcomes of TAA participants and the comparison group is not an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Difference in distribution of categories between TAA participants and comparison group is statistically significant at the 0.05 level.

## Table IX-18: Impacts on Health Insurance Coverage Since Job Loss (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Covered By Health Insurance At Any Time (Percent) | 82.7 | 88.6 | -5.9*** | 1.4 |
| Covered By Health Insurance For Entire Period (Percent) | 35.4 | 41.7 | -6.3*** | 2.1 |
| Months Covered By Health Insurance | 30.1 | 34.9 | -4.8*** | 0.9 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The first follow-up period is the time between job loss and the first follow-up interview and is an average of 29 months. The second follow-up period is the time between the first and second follow-up interviews and is an average of 22 months.

[a]Sample limited to individuals with any health insurance. This is a conditional outcome, and the difference between the outcomes of TAA participants and the comparison group is not an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

## Table IX-19: Impacts on Medical Expenditures (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Out-of-Pocket Medical Expenses in 12 Months Before First Follow-Up Interview[a] (2006$) | 1,096 | 1,210 | -115 | 96 |
| Out-of-Pocket Medical Expenses in 12 Months before Second Follow-Up (2006$) | 1,233 | 1,425 | -193 | 136 |
| Sample Size | 2,054 | 1,796 | | |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

[a]Individuals who completed the follow-up survey but not the initial survey are excluded.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impacts of TAA on distribution of categories are statistically significant at the 0.05 level.

Among those who had health insurance, participants and comparisons acquired insurance from similar sources (Tables IX-16 and IX-17). However, participants were less likely to report insurance through employers, unions, or schools, and more likely to report government-sponsored plans during the first follow-up period. These findings suggest that the negative impact of TAA on employment, coupled with the associated negative impact on employer-sponsored insurance, may have led to the negative effect of TAA on health insurance. The difference in take-up of government-sponsored plans might partly reflect take-up of the HCTC by 14 percent of the participants (Table IX-20).[44]

TAA did not affect out-of-pocket medical expenditures (Table IX-19). Annual expenditures were about $1,100 to $1,400 for both participants and comparisons.

## D. SUBGROUP ANALYSES

This section examines whether the impacts of TAA on income, public assistance receipt, family structure, health-related, and mobility outcomes differed significantly across subgroups defined by demographic and local area characteristics. In addition to the key age and trainee subgroups, we examined impacts for subgroups defined by race and ethnicity, gender, family composition, education, self-reported health status, metropolitan area residence status, and the local unemployment rate, all measured at the time of job loss.

In general, we found that the impacts on the considered outcomes did not vary across subgroups, suggesting that the full-sample findings discussed above hold true for a broad range of population subgroups. In the remainder of this section, we discuss isolated deviations from this general trend, where no clear patterns emerge.

### 1. Impacts on Other Income Sources

We found some differences in program impacts on the receipt of UI and TRA benefits by age and trainee status. The impact of TAA on the receipt of UI benefits was also significantly different across racial and ethnic groups, with TAA causing an increase in payments of $3,300 for whites, $2,700 for blacks, and less than $2,000 for Hispanics and other races (Table IX-21). This pattern likely reflects base earnings differences across race and ethnicity subgroups that determined the workers' UI benefit payments.

[44] Although the HCTC provides a government-sponsored subsidy for health plans provided through other sources, survey respondents who claimed the HCTC may have reported having a government-sponsored health plan.

## Table IX-20: Health Coverage Tax Credit Take-Up (Survey Data)

| | TAA Participant Mean | Standard Error |
|---|---|---|
| Health Coverage Tax Credit (HCTC) participation (Percent)[a] [sample: TAA participants] | | |
| Received HCTC | 14.5 | 0.9 |
| Applied but did not receive | 3.5 | 0.5 |
| Eligible but did not apply | 39.3 | 1.3 |
| Not eligible or not aware | 42.7 | 1.3 |
| Amount of Credit[b] | | |
| Less than $200 | 17.5 | 3.2 |
| $200-$499 | 31.2 | 3.8 |
| $500-$999 | 18.8 | 3.0 |
| $1,000-$1,999 | 12.0 | 2.9 |
| $2,000 or more | 20.4 | 3.5 |
| Average Amount of Credit ($) | 1,396 | 198 |
| Reason for Not Applying[c] | | |
| Had other coverage | 45.7 | 2.1 |
| Too expensive even after credit | 36.9 | 2.2 |
| Program rules or paperwork too complicated | 5.2 | 0.8 |
| Other | 12.1 | 1.4 |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: Treatment group weights account for sample design and nonresponse. Standard errors account for the two-stage sampling design. The total number of participants is N=2,054.

[a]Sample is limited to TAA participants.

[b]Sample is limited to TAA participants receiving HCTC.

[c]Sample is limited to TAA participants eligible for and aware of HCTC but did not apply.

## Table IX-21: Impacts on Receipt of Unemployment Insurance by Subgroup (Administrative Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 11,154 | 8,221 | 2,933*** | 363 |
| Training Status[1] | | | | |
| Trainees | 11,811 | 8,381 | 3,430*** | 403 |
| TRA only | 9,931 | 7,898 | 2,033*** | 383 |
| Age† | | | | |
| 16 to 29 | 10,189 | 8,807 | 1,383* | 814 |
| 30 to 39 | 10,835 | 8,024 | 2,811*** | 547 |
| 40 to 49 | 11,462 | 7,083 | 4,379*** | 516 |
| 50 to 59 | 11,683 | 9,820 | 1,863*** | 570 |
| 60 or over | 10,236 | 7,018 | 3,218*** | 866 |
| Race† | | | | |
| White | 11,542 | 8,212 | 3,329*** | 403 |
| Black | 10,433 | 7,711 | 2,722*** | 533 |
| Hispanic | 10,242 | 8,719 | 1,523** | 647 |
| Other | 10,798 | 8,974 | 1,825* | 1,044 |
| Gender | | | | |
| Female | 9,952 | 7,707 | 2,245*** | 429 |
| Male | 12,481 | 9,020 | 3,460*** | 516 |
| Family Composition | | | | |
| Married w/kids | 11,564 | 7,301 | 4,263*** | 604 |
| Unmarried w/kids | 10,501 | 8,940 | 1,561*** | 570 |
| Married no kids | 11,129 | 8,583 | 2,546*** | 548 |
| Unmarried no kids | 11,193 | 7,995 | 3,197*** | 602 |
| Education | | | | |
| Less Than High School | 9,700 | 6,643 | 3,057*** | 406 |
| High School Diploma | 11,286 | 8,372 | 2,915*** | 427 |
| Some College | 11,835 | 9,114 | 2,721*** | 683 |
| Bachelor's or More | 12,033 | 7,722 | 4,311*** | 1,211 |
| Health | | | | |
| Good or Excellent | 11,287 | 8,181 | 3,106*** | 431 |
| Fair or Poor | 10,605 | 8,634 | 1,971*** | 590 |

## Table IX-21 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area | | | | |
| Metro | 11,745 | 8,881 | 2,864*** | 461 |
| Non-Metro | 10,185 | 7,147 | 3,038*** | 445 |
| Local Unemployment Rate at UI Claim† | | | | |
| Less than 4.4 | 10,455 | 7,471 | 2,984*** | 540 |
| 4.4 to 5.1 | 12,516 | 9,339 | 3,177*** | 585 |
| 5.1 to 6.0 | 11,586 | 6,903 | 4,683*** | 536 |
| Greater than 6.0 | 10,001 | 7,668 | 2,333*** | 523 |

Source: State UI Administrative Data.

Notes: The outcome for all rows is the total Unemployment Insurance payment receipt for the trigger quarter and subsequent 12 quarters, in 2006 dollars. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey and for whom UI administrative data provide complete information for all quarters.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

Trainees collected $13,000 more in total benefits than their matched comparisons, compared with the $8,000 difference between the TRA-only subsample and their comparisons (Table IX-22). This impact difference is consistent with the finding that trainees spent more time unemployed after their initial job losses than the TRA-only participants, who TAA program staff determined to be capable of returning to work without receiving training. TAA had a significantly different impact across age groups, although no clear pattern emerged. The impact on combined UI and TRA payments was greatest for workers age 40 to 49. Participation in TAA also generally had a greater impact on UI and combined UI and TRA benefit receipt for those in areas with higher local unemployment rates.

Impacts on the receipt of food stamps, cash assistance, and pensions varied little across subgroups (Tables IX-23 through IX-25). There were no significant impact differences in food stamp receipt by age and trainee status. However, TAA did increase the receipt of food stamps for workers in non-metropolitan areas (Table IX-23). This difference is driven largely by the fact that only 11 percent of comparisons in non-metropolitan areas collected food stamp benefits after job loss, whereas 18 percent of comparisons in metropolitan areas collected these benefits.

Older workers were more likely to collect pension benefits than younger workers, although a substantial proportion of younger workers reported receiving some benefit. Neither trainees nor TRA-only participants differed significantly from their matched comparisons in pension receipt (Table IX-25). While there was a negative and marginally significant difference for the oldest workers, we did not find a statistically significant difference across age groups at the 5 percent level.

Trainees, their comparisons, and workers in their 30s and 40s were more likely than their counterpart workers to report having spouses or partners working at the follow-up interview (Table IX-26). However, participant-comparison differences in the fraction of workers with spouses or partners working did not vary across training or age subgroups. Workers in poor health at the time of job loss experienced a greater impact of TAA on having a spouse or partner working at the follow-up interview, possibly because these workers were not optimistic about returning to work even after training. Married participants with no children also experienced a larger impact on spouses' earnings than did other groups.

While the impact of participation on 2007 income levels was not significantly different across age groups, workers in their 30s and 40s appear to be responsible for the negative impact on 2009 income, as shown in Exhibit IX-3. This is because comparisons aged 30 to 49 generally reported the highest incomes, while participants' incomes were more similar across age groups. Exhibit IX-4 shows that the trainees lost $5,200 relative to their comparisons in 2007 income, larger than the $3,200 difference between TRA-only recipients and their comparisons. Trainees and TRA-only participants lagged equally behind their comparisons in 2009, consistent with our finding that most TAA participants had completed training by the end of the study period.

## Table IX-22: Impacts on Receipt of UI and TRA Payments by Subgroup (Administrative Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 19,569 | 8,841 | 10,728*** | 515 |
| Training Status[1] | | | | |
| Trainees | 21,916 | 9,247 | 12,669*** | 647 |
| TRA only | 16,175 | 8,616 | 7,559*** | 493 |
| Age† | | | | |
| 16 to 29 | 17,927 | 7,097 | 10,830*** | 1,400 |
| 30 to 39 | 20,374 | 10,058 | 10,316*** | 1,097 |
| 40 to 49 | 20,181 | 7,548 | 12,633*** | 794 |
| 50 to 59 | 19,708 | 10,225 | 9,483*** | 744 |
| 60 or over | 17,336 | 6,533 | 10,803*** | 995 |
| Race† | | | | |
| White | 20,053 | 8,287 | 11,766*** | 580 |
| Black | 18,744 | 9,829 | 8,914*** | 727 |
| Hispanic | 17,488 | 9,915 | 7,573*** | 1,472 |
| Other | 16,229 | 12,111 | 4,118** | 1,574 |
| Gender | | | | |
| Female | 18,437 | 8,229 | 10,209*** | 693 |
| Male | 20,752 | 9,701 | 11,050*** | 636 |
| Family Composition | | | | |
| Married w/kids | 19,833 | 8,260 | 11,572*** | 827 |
| Unmarried w/kids | 19,222 | 8,639 | 10,583*** | 1,007 |
| Married no kids | 19,270 | 9,111 | 10,159*** | 735 |
| Unmarried no kids | 19,901 | 8,248 | 11,653*** | 944 |
| Education | | | | |
| Less Than High School | 16,698 | 7,381 | 9,317*** | 814 |
| High School Diploma | 19,776 | 8,503 | 11,273*** | 623 |
| Some College | 21,236 | 10,225 | 11,011*** | 973 |
| Bachelors or More | 20,371 | 7,533 | 12,839*** | 1,297 |
| Health | | | | |
| Good or Excellent | 19,765 | 8,725 | 11,040*** | 572 |
| Fair or Poor | 18,658 | 9,315 | 9,343*** | 815 |

## Table IX-22 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area | | | | |
| Metro | 20,604 | 9,468 | 11,136*** | 644 |
| Non-Metro | 18,045 | 7,853 | 10,193*** | 577 |
| Local Unemployment Rate at UI Claim† | | | | |
| Less than 4.4 | 19,370 | 7,362 | 12,008*** | 825 |
| 4.4 to 5.1 | 21,275 | 10,403 | 10,872*** | 805 |
| 5.1 to 6.0 | 20,977 | 7,931 | 13,045*** | 803 |
| Greater than 6.0 | 16,880 | 8,490 | 8,390*** | 680 |

Source: State UI Administrative Data.

Notes: The outcome for all rows is the total Unemployment Insurance and Trade Readjustment Allowance payment receipt for the trigger quarter and subsequent 12 quarters, in 2006 dollars. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey and for whom UI and TRA administrative data provide complete information for all quarters.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table IX-23: Impacts on Receipt of Food Stamp Benefits by Subgroup (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 18.0 | 14.9 | 3.1** | 1.5 |
| Training Status[1] | | | | |
| Trainees | 20.7 | 15.7 | 4.9*** | 1.8 |
| TRA only | 14.2 | 13.4 | 0.7 | 2.0 |
| Age | | | | |
| 16 to 29 | 33.4 | 27.1 | 6.4 | 9.8 |
| 30 to 39 | 24.4 | 29.1 | -4.7 | 3.0 |
| 40 to 49 | 19.9 | 14.3 | 5.6*** | 2.0 |
| 50 to 59 | 11.6 | 9.0 | 2.5 | 1.9 |
| 60 or over | 7.8 | 7.8 | 0.0 | 0.1 |
| Race | | | | |
| White | 13.7 | 11.5 | 2.1 | 1.6 |
| Black | 29.9 | 17.0 | 12.9*** | 3.7 |
| Hispanic | 20.5 | 21.8 | -1.3 | 19.4 |
| Other | 11.6 | 9.7 | 1.9*** | 0.6 |
| Gender | | | | |
| Female | 21.0 | 17.2 | 3.8* | 2.2 |
| Male | 14.8 | 10.8 | 3.9** | 1.7 |
| Family Composition | | | | |
| Married w/kids | 16.8 | 13.0 | 3.8* | 2.0 |
| Unmarried w/kids | 42.3 | 35.0 | 7.4 | 4.8 |
| Married no kids | 6.7 | 2.7 | 4.0*** | 1.1 |
| Unmarried no kids | 16.4 | 11.6 | 4.8** | 2.2 |
| Education | | | | |
| Less Than High School | 26.5 | 24.3 | 2.2 | 3.7 |
| High School Diploma | 17.9 | 13.1 | 4.8*** | 1.8 |
| Some College | 16.1 | 11.8 | 4.3 | 3.2 |
| Bachelor's or More | 3.4 | 3.4 | 0.0*** | 0.0 |
| Health† | | | | |
| Good or Excellent | 16.8 | 11.2 | 5.6*** | 1.5 |
| Fair or Poor | 23.3 | 23.3 | 0.0 | 2.4 |

## Table IX-23 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area† | | | | |
| Metro | 16.2 | 17.5 | -1.3 | 1.8 |
| Non-Metro | 21.0 | 10.6 | 10.4*** | 2.4 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 14.7 | 12.6 | 2.1 | 2.0 |
| 4.4 to 5.1 | 13.1 | 14.7 | -1.6 | 2.8 |
| 5.1 to 6.0 | 19.9 | 15.7 | 4.1* | 2.1 |
| Greater than 6.0 | 23.1 | 16.5 | 6.6** | 2.8 |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: The outcome for all rows is the percentage of workers who reported collecting food stamp benefits after job loss. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table IX-24: Impacts on Receipt of Cash Assistance Benefits by Subgroup (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 26.3 | 29.5 | -3.2* | 1.9 |
| Training Status[1] | | | | |
| Trainees | 20.5 | 23.1 | -2.7 | 2.0 |
| TRA only | 36.6 | 40.4 | -3.8 | 2.5 |
| Age | | | | |
| 16 to 29 | 10.2 | 25.2 | -15.0*** | 3.9 |
| 30 to 39 | 13.1 | 11.6 | 1.6 | 2.4 |
| 40 to 49 | 12.8 | 19.6 | -6.7*** | 2.2 |
| 50 to 59 | 32.7 | 36.1 | -3.4 | 3.3 |
| 60 or over | 81.8 | 90.2 | -8.3* | 4.3 |
| Race | | | | |
| White | 28.5 | 29.8 | -1.3 | 2.0 |
| Black | 25.1 | 24.3 | 0.8 | 5.5 |
| Hispanic | 16.6 | 17.4 | -0.8 | 2.6 |
| Other | 19.7 | 16.9 | 2.8*** | 0.8 |
| Gender | | | | |
| Female | 29.3 | 34.1 | -4.7* | 2.6 |
| Male | 22.9 | 23.6 | -0.7 | 2.0 |
| Family Composition | | | | |
| Married w/kids | 12.9 | 12.3 | 0.6 | 1.8 |
| Unmarried w/kids | 19.4 | 29.5 | -10.2*** | 3.4 |
| Married no kids | 41.2 | 43.8 | -2.6 | 2.9 |
| Unmarried no kids | 28.2 | 30.7 | -2.6 | 2.9 |
| Education | | | | |
| Less Than High School | 38.7 | 39.9 | -1.2 | 3.3 |
| High School Diploma | 23.5 | 27.3 | -3.9* | 2.2 |
| Some College | 25.6 | 24.3 | 1.3 | 2.1 |
| Bachelor's or More | 21.7 | 37.6 | -15.9*** | 3.3 |
| Health | | | | |
| Good or Excellent | 24.3 | 26.9 | -2.5 | 2.2 |
| Fair or Poor | 33.6 | 35.5 | -1.9 | 3.9 |

## Table IX-24 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area | | | | |
| Metro | 25.3 | 28.9 | -3.6* | 2.1 |
| Non-Metro | 27.9 | 30.4 | -2.5 | 2.9 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 25.0 | 30.0 | -5.0* | 2.8 |
| 4.4 to 5.1 | 24.1 | 31.6 | -7.5*** | 2.6 |
| 5.1 to 6.0 | 27.1 | 30.4 | -3.3 | 3.3 |
| Greater than 6.0 | 28.3 | 24.6 | 3.6 | 2.6 |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: The outcome for all rows is the percentage of workers who reported collecting cash assistance benefits since job loss. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table IX-25: Impacts on Receipt of Pension Benefits by Subgroup (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 32.1 | 29.6 | 2.4 | 2.3 |
| Training Status[1] | | | | |
| Trainees | 30.1 | 26.6 | 3.5 | 2.5 |
| TRA only | 37.7 | 36.6 | 1.1 | 2.9 |
| Age | | | | |
| 16 to 29 | 15.3 | 25.0 | -9.7 | 9.7 |
| 30 to 39 | 26.8 | 16.6 | 10.2*** | 3.5 |
| 40 to 49 | 26.0 | 21.6 | 4.4 | 3.0 |
| 50 to 59 | 37.8 | 35.7 | 2.1 | 3.4 |
| 60 or over | 54.9 | 65.3 | -10.3* | 5.5 |
| Race | | | | |
| White | 33.9 | 32.0 | 1.9 | 2.7 |
| Black | 30.1 | 23.1 | 7.0* | 4.1 |
| Hispanic | 25.4 | 9.9 | 15.5*** | 4.9 |
| Other | 22.8 | 22.8 | 0.0 | 0.0 |
| Gender | | | | |
| Female | 31.8 | 29.8 | 2.0 | 3.1 |
| Male | 32.4 | 31.0 | 1.4 | 2.6 |
| Family Composition | | | | |
| Married w/kids | 27.4 | 18.6 | 8.8*** | 3.3 |
| Unmarried w/kids | 30.0 | 26.1 | 3.9 | 4.1 |
| Married no kids | 39.3 | 37.8 | 1.4 | 3.3 |
| Unmarried no kids | 30.0 | 31.0 | -1.1 | 3.7 |
| Education | | | | |
| Less Than High School | 32.8 | 31.5 | 1.3 | 3.4 |
| High School Diploma | 31.2 | 29.1 | 2.1 | 2.5 |
| Some College | 37.7 | 21.1 | 16.6*** | 4.3 |
| Bachelor's or More | 26.8 | 29.3 | -2.5 | 3.5 |
| Health | | | | |
| Good or Excellent | 31.7 | 29.6 | 2.1 | 2.6 |
| Fair or Poor | 34.1 | 35.4 | -1.3 | 3.9 |

## Table IX-25 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area | | | | |
| Metro | 31.2 | 32.2 | -1.0 | 2.7 |
| Non-Metro | 33.6 | 27.4 | 6.3** | 2.9 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 32.6 | 34.3 | -1.7 | 3.5 |
| 4.4 to 5.1 | 33.8 | 31.1 | 2.6 | 3.4 |
| 5.1 to 6.0 | 29.3 | 26.0 | 3.4 | 3.4 |
| Greater than 6.0 | 33.0 | 25.6 | 7.4** | 3.0 |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: The outcome for all rows is the percentage of workers who reported collecting pension benefits after job loss. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table IX-26: Impacts on Percent with Spouse or Partner Working by Subgroup (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 38.2 | 38.7 | -0.5 | 2.1 |
| Training Status[1] | | | | |
| Trainees | 41.8 | 41.8 | 0.1 | 2.3 |
| TRA only | 33.0 | 32.2 | 0.9 | 2.3 |
| Age | | | | |
| 16 to 29 | 38.0 | 36.4 | 1.6 | 6.6 |
| 30 to 39 | 46.1 | 46.9 | -0.9 | 3.3 |
| 40 to 49 | 41.7 | 42.9 | -1.2 | 3.1 |
| 50 to 59 | 37.5 | 29.9 | 7.6*** | 2.5 |
| 60 or over | 17.1 | 31.9 | -14.8 | 12.9 |
| Race | | | | |
| White | 41.3 | 42.5 | -1.2 | 2.5 |
| Black | 28.7 | 24.6 | 4.0 | 2.8 |
| Hispanic | 40.6 | 36.0 | 4.6 | 5.2 |
| Other | 42.8 | 40.1 | 2.7 | 7.9 |
| Gender | | | | |
| Female | 35.4 | 30.9 | 4.6* | 2.5 |
| Male | 41.3 | 43.3 | -2.0 | 2.6 |
| Family Composition† | | | | |
| Married w/kids | 65.1 | 59.7 | 5.4* | 3.1 |
| Unmarried w/kids | 14.2 | 16.8 | -2.6 | 3.2 |
| Married no kids | 46.9 | 39.0 | 7.9*** | 2.9 |
| Unmarried no kids | 11.1 | 11.7 | -0.6 | 2.1 |
| Education | | | | |
| Less Than High School | 30.5 | 25.3 | 5.3 | 3.7 |
| High School Diploma | 37.1 | 36.2 | 0.8 | 3.0 |
| Some College | 41.2 | 40.5 | 0.6 | 3.5 |
| Bachelor's or More | 54.8 | 40.3 | 14.5** | 6.7 |
| Health† | | | | |
| Good or Excellent | 38.1 | 40.5 | -2.4 | 2.2 |
| Fair or Poor | 38.1 | 29.3 | 8.8*** | 3.0 |

## Table IX-26 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area | | | | |
| Metro | 37.4 | 40.0 | -2.6 | 2.3 |
| Non-Metro | 39.6 | 36.0 | 3.6 | 2.6 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 39.4 | 41.8 | -2.4 | 3.0 |
| 4.4 to 5.1 | 38.5 | 42.4 | -3.9 | 2.8 |
| 5.1 to 6.0 | 35.7 | 35.3 | 0.4 | 2.7 |
| Greater than 6.0 | 39.9 | 32.8 | 7.1** | 2.9 |

Source: Mathematica TAA Initial and Follow-Up Surveys.

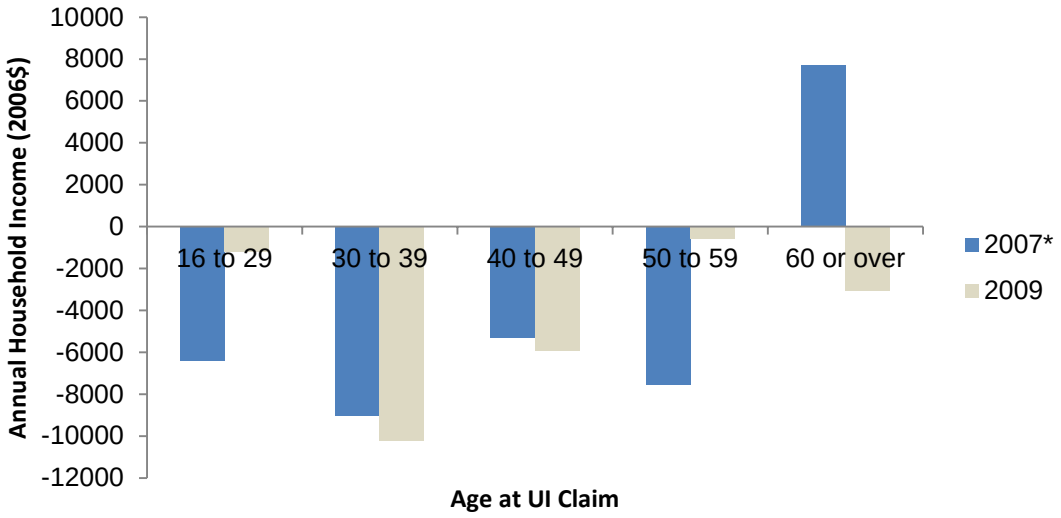
Notes: The outcome for all rows is the percentage of workers who reported having a spouse or partner working at the time of the follow-up survey. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Exhibit IX-3: Impact on Annual Household Income, by Age and Year

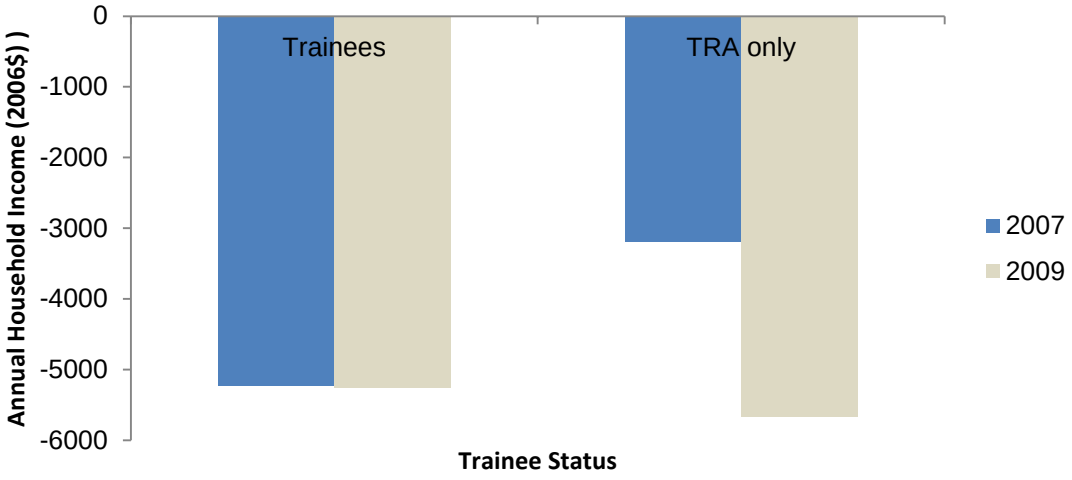



Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different across age subgroups at the 0.05 level, two-tailed test.

## Exhibit IX-4: Participant-Comparison Difference in Annual Household Income, by Trainee Status and Year




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means are regression adjusted. Standard errors account for the two-stage sampling design.

Participation in TAA reduced total household income in 2007 and 2009 more for males than for females, primarily reflecting the higher incomes of males in the comparison group (Tables IX-27 and IX-28). We also found that there was a greater negative impact on 2009 income for those in non-metropolitan areas and those in areas with the highest local unemployment rates. A possible explanation is that training is less effective in these areas, where other job opportunities may be especially limited.

## 2. Family Structure, Household Composition, and Mobility

Although TAA did not have an overall impact on marital status at the follow-up interview, we found that participation had a differential effect on marriage for the family composition subgroups (Table IX-29). Workers age 60 and older show a negative impact of two percentage points on being married at the follow-up interview, but the treatment-comparison difference is small, and no other age groups show a statistically significant effect. Among those who were married with children at program entry, marital rates at follow-up were higher for participants than for their comparisons (90 percent, compared to 82 percent). However, impacts on marital rates were negative for those who were married without children at program entry (88 percent for participants, compared to 93 percent for comparisons). This suggests that TAA had a positive impact on preventing separation for married couples with children, but a negative impact for married couples without children.

Impacts on household size did not vary by age group. However, we found that TAA participation had a positive impact on household size for participants who were married with children at the time of job loss, but not for other types of households (Table IX-30). Similar results (not shown) indicate that the impact on the number of dependent children is greatest for married participants with children, suggesting that some families with children decided to have an additional child (or alter the timing of fertility) while in training. We also found that workers in non-metropolitan areas and in areas with higher unemployment rates experienced a positive impact on household size. This impact may be caused by additional children or adults in the household.

Younger workers were more likely to have moved, but TAA did not have a differential impact on mobility across age groups. Mobility rates were low for both participants and comparisons, and we also did not find any differences in impacts across any other subgroups (Table IX-31).

## 3. Health Status and Health Insurance Coverage

We found few differences in impacts on health outcomes across subgroups. Although younger workers, trainees, and comparisons of trainees were more likely than other groups to report being in good or excellent health at both interviews, treatment-comparison differences did not vary significantly by age or trainee group (Table IX-32). Impacts on the incidence of work-limiting conditions were similarly small across age and trainee groups (Table IX-33). We found a significant difference in impacts of TAA participation on the fraction of workers reporting work-limiting conditions at the time of the follow-up interview across only one subgroup—education (Table IX-33). Participants without high school diplomas and those with some college were more likely to report disabilities than their comparisons, while there were little or no impacts on participants with high school diplomas or college degrees. These mixed results do not suggest any clear policy implications.

Although older workers were more likely than younger workers to maintain health insurance coverage, the impact of TAA on coverage was not statistically different across age groups (Table IX-34). The comparison groups of trainees and TRA-only participants had similar rates of coverage, but only trainees had significantly lower rates of continuous coverage than their comparisons. Furthermore, TAA did not reduce health insurance coverage rates for those who reported being in fair or poor health at the time of job loss, as shown in Exhibit IX-5.

In addition to the differential impact of TAA on health insurance coverage by baseline health status, we also found a significant difference in impacts by race and ethnicity (Table IX-34). Participation in TAA reduced the percentage of white workers who were continuously covered by health insurance since job loss from 53 to 41 percent, but increased coverage for black participants, to 25 percent from 19 percent. No impacts were found, however, for Hispanics or those in other racial or ethnic groups. The large difference in coverage between black and white comparison groups was likely due to financial and other demographic differences in those subpopulations.

**Table IX-27: Impacts on 2007 Total Household Income by Subgroup (Survey Data)**

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 34,321 | 39,124 | -4,803*** | 1,180 |
| Training Status[1] | | | | |
| Trainees | 34,363 | 39,596 | -5,233*** | 1,531 |
| TRA only | 34,809 | 37,997 | -3,188** | 1,563 |
| Age | | | | |
| 16 to 29 | 25,968 | 32,381 | -6,413*** | 2,110 |
| 30 to 39 | 34,656 | 43,688 | -9,032*** | 1,723 |
| 40 to 49 | 35,493 | 40,797 | -5,304*** | 1,895 |
| 50 to 59 | 33,908 | 41,465 | -7,558*** | 1,725 |
| 60 or over | 38,743 | 31,044 | 7,698* | 4,109 |
| Race | | | | |
| White | 37,909 | 42,387 | -4,478*** | 1,440 |
| Black | 24,266 | 27,735 | -3,469** | 1,761 |
| Hispanic | 30,754 | 33,435 | -2,681 | 2,462 |
| Other | 37,204 | 33,273 | 3,931 | 6,763 |
| Gender† | | | | |
| Female | 31,175 | 34,217 | -3,042** | 1,303 |
| Male | 37,535 | 43,934 | -6,399*** | 1,684 |

## Table IX-27 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Family Composition† | | | | |
| Married w/kids | 40,493 | 50,117 | -9,623*** | 2,113 |
| Unmarried w/kids | 21,482 | 27,202 | -5,720*** | 1,696 |
| Married no kids | 42,425 | 45,888 | -3,463 | 2,416 |
| Unmarried no kids | 23,688 | 26,695 | -3,008* | 1,739 |
| Education | | | | |
| Less Than High School | 27,387 | 31,456 | -4,069 | 2,541 |
| High School Diploma | 32,232 | 37,437 | -5,206*** | 1,191 |
| Some College | 37,227 | 40,328 | -3,101 | 2,312 |
| Bachelor's or More | 62,120 | 62,546 | -425 | 6,133 |
| Health | | | | |
| Good or Excellent | 35,336 | 40,575 | -5,239*** | 1,293 |
| Fair or Poor | 29,847 | 32,788 | -2,941 | 2,242 |
| Metropolitan Area | | | | |
| Metro | 36,716 | 40,384 | -3,668** | 1,583 |
| Non-Metro | 30,322 | 39,499 | -9,177*** | 1,687 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 40,984 | 40,478 | 506 | 2,482 |
| 4.4 to 5.1 | 35,620 | 44,651 | -9,031*** | 1,894 |
| 5.1 to 6.0 | 32,366 | 38,364 | -5,999*** | 2,096 |
| Greater than 6.0 | 29,946 | 35,806 | -5,860*** | 1,934 |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: The outcome for all rows is the total household income reported by the worker in 2007, measured in 2006 dollars. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table IX-28: Impacts on 2009 Total Household Income by Subgroup (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 33,658 | 38,795 | -5,137*** | 1,833 |
| Training Status[1] | | | | |
| Trainees | 34,830 | 40,089 | -5,259*** | 1,943 |
| TRA only | 31,579 | 37,249 | -5,671*** | 1,883 |
| Age† | | | | |
| 16 to 29 | 29,594 | 30,792 | -1,198 | 4,756 |
| 30 to 39 | 38,103 | 48,316 | -10,212*** | 3,110 |
| 40 to 49 | 33,907 | 39,860 | -5,953** | 2,395 |
| 50 to 59 | 33,837 | 34,406 | -570 | 1,573 |
| 60 or over | 25,585 | 28,662 | -3,077* | 1,740 |
| Race | | | | |
| White | 36,305 | 42,861 | -6,556*** | 2,117 |
| Black | 26,426 | 27,155 | -729 | 2,100 |
| Hispanic | 30,040 | 26,789 | 3,252 | 2,945 |
| Other | 36,624 | 41,772 | -5,148 | 4,714 |
| Gender† | | | | |
| Female | 30,439 | 31,941 | -1,502 | 1,527 |
| Male | 36,654 | 44,634 | -7,980*** | 2,178 |
| Family Composition | | | | |
| Married w/kids | 44,205 | 50,195 | -5,991** | 2,372 |
| Unmarried w/kids | 21,437 | 26,664 | -5,227*** | 1,999 |
| Married no kids | 37,969 | 41,582 | -3,613* | 1,983 |
| Unmarried no kids | 23,543 | 24,388 | -845 | 1,466 |
| Education | | | | |
| Less Than High School | 22,596 | 24,441 | -1,845 | 1,631 |
| High School Diploma | 31,281 | 32,574 | -1,292 | 1,125 |
| Some College | 40,111 | 53,394 | -13,283*** | 3,381 |
| Bachelor's or More | 59,373 | 79,942 | -20,569*** | 5,106 |
| Health | | | | |
| Good or Excellent | 34,325 | 41,215 | -6,890*** | 2,106 |
| Fair or Poor | 30,253 | 28,907 | 1,346 | 1,890 |

## Table IX-28 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area† | | | | |
| Metro | 35,072 | 36,686 | -1,614 | 1,515 |
| Non-Metro | 31,228 | 42,700 | -11,472*** | 2,754 |
| Local Unemployment Rate at UI Claim† | | | | |
| Less than 4.4 | 39,149 | 36,302 | 2,848 | 2,566 |
| 4.4 to 5.1 | 35,692 | 41,285 | -5,593** | 2,410 |
| 5.1 to 6.0 | 31,231 | 36,572 | -5,341** | 2,409 |
| Greater than 6.0 | 30,281 | 39,643 | -9,362*** | 2,431 |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: The outcome for all rows is the total household income reported by the worker in 2009, measured in 2006 dollars. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table IX-29: Impacts on Percent Married by Subgroup (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 57.4 | 56.9 | 0.5 | 1.3 |
| Training Status[1] | | | | |
| Trainees | 57.8 | 56.7 | 1.1 | 1.7 |
| TRA only | 57.4 | 56.8 | 0.6 | 1.9 |
| Age† | | | | |
| 16 to 29 | 53.1 | 49.6 | 3.5 | 5.9 |
| 30 to 39 | 53.9 | 53.1 | 0.8 | 2.9 |
| 40 to 49 | 54.1 | 57.3 | -3.2 | 2.5 |
| 50 to 59 | 64.2 | 61.3 | 2.8* | 1.4 |
| 60 or over | 58.0 | 59.5 | -1.6*** | 0.5 |
| Race | | | | |
| White | 60.6 | 62.0 | -1.5 | 1.9 |
| Black | 42.7 | 41.7 | 1.0 | 3.7 |
| Hispanic | 62.9 | 43.7 | 19.2** | 8.8 |
| Other | 70.3 | 58.5 | 11.8*** | 3.4 |
| Gender | | | | |
| Female | 52.3 | 51.5 | 0.8 | 2.0 |
| Male | 62.9 | 64.7 | -1.7 | 1.6 |
| Family Composition† | | | | |
| Married w/kids | 89.8 | 81.7 | 8.1*** | 2.6 |
| Unmarried w/kids | 11.8 | 14.0 | -2.2 | 3.9 |
| Married no kids | 88.3 | 92.9 | -4.6** | 1.9 |
| Unmarried no kids | 9.3 | 7.7 | 1.6 | 1.7 |
| Education | | | | |
| Less Than High School | 59.0 | 57.5 | 1.5 | 3.2 |
| High School Diploma | 53.2 | 51.8 | 1.4 | 1.8 |
| Some College | 60.2 | 60.9 | -0.7 | 3.2 |
| Bachelor's or More | 78.9 | 78.9 | 0.0 | 0.0 |
| Health | | | | |
| Good or Excellent | 57.6 | 58.3 | -0.7 | 1.6 |
| Fair or Poor | 56.3 | 56.4 | -0.1 | 3.3 |

## Table IX-29 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area | | | | |
| Metro | 57.0 | 57.8 | -0.8 | 1.7 |
| Non-Metro | 58.1 | 56.9 | 1.2 | 2.0 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 57.4 | 60.9 | -3.5* | 2.1 |
| 4.4 to 5.1 | 60.2 | 63.3 | -3.1 | 2.6 |
| 5.1 to 6.0 | 55.1 | 55.9 | -0.8 | 2.2 |
| Greater than 6.0 | 57.1 | 52.2 | 4.9** | 2.3 |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: The outcome for all rows is the percentage of workers who were married at the time of the follow-up interview. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table IX-30: Impacts on Household Size by Subgroup (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 2.6 | 2.5 | 0.1* | 0.1 |
| Training Status[1] | | | | |
| Trainees | 2.7 | 2.6 | 0.1 | 0.1 |
| TRA only | 2.4 | 2.2 | 0.2*** | 0.1 |
| Age | | | | |
| 16 to 29 | 3.2 | 3.3 | -0.1 | 0.1 |
| 30 to 39 | 3.4 | 3.1 | 0.2* | 0.1 |
| 40 to 49 | 2.6 | 2.5 | 0.1 | 0.1 |
| 50 to 59 | 2.2 | 2.1 | 0.1 | 0.1 |
| 60 or over | 2.0 | 2.0 | 0.0 | 0.1 |
| Race | | | | |
| White | 2.5 | 2.4 | 0.1** | 0.1 |
| Black | 2.6 | 2.4 | 0.2 | 0.1 |
| Hispanic | 3.3 | 3.8 | -0.5*** | 0.2 |
| Other | 3.1 | 3.5 | -0.4** | 0.2 |
| Gender | | | | |
| Female | 2.6 | 2.4 | 0.2** | 0.1 |
| Male | 2.7 | 2.6 | 0.1 | 0.1 |
| Family Composition† | | | | |
| Married w/kids | 3.7 | 3.3 | 0.4*** | 0.1 |
| Unmarried w/kids | 2.7 | 2.7 | 0.0 | 0.1 |
| Married no kids | 2.2 | 2.2 | 0.0 | 0.1 |
| Unmarried no kids | 1.7 | 1.8 | -0.1 | 0.1 |
| Education | | | | |
| Less Than High School | 2.7 | 2.5 | 0.1 | 0.1 |
| High School Diploma | 2.6 | 2.4 | 0.1* | 0.1 |
| Some College | 2.7 | 2.6 | 0.1 | 0.1 |
| Bachelor's or More | 2.8 | 2.9 | -0.1 | 0.1 |
| Health | | | | |
| Good or Excellent | 2.6 | 2.6 | 0.1 | 0.1 |
| Fair or Poor | 2.6 | 2.3 | 0.2*** | 0.1 |

## Table IX-30 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area† | | | | |
| Metro | 2.7 | 2.7 | 0.0 | 0.1 |
| Non-Metro | 2.6 | 2.3 | 0.3*** | 0.1 |
| Local Unemployment Rate at UI Claim† | | | | |
| Less than 4.4 | 2.7 | 2.7 | 0.0 | 0.1 |
| 4.4 to 5.1 | 2.7 | 2.7 | 0.0 | 0.1 |
| 5.1 to 6.0 | 2.6 | 2.4 | 0.2** | 0.1 |
| Greater than 6.0 | 2.6 | 2.3 | 0.3*** | 0.1 |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: The outcome for all rows is the number of household members at the time of the follow-up interview. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table IX-31: Impacts on Mobility by Subgroup (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 6.4 | 6.8 | -0.4 | 1.1 |
| Training Status[1] | | | | |
| Trainees | 7.1 | 7.1 | 0.0 | 1.2 |
| TRA only | 4.9 | 6.3 | -1.3 | 1.2 |
| Age | | | | |
| 16 to 29 | 11.3 | 29.8 | -18.5*** | 4.9 |
| 30 to 39 | 10.8 | 7.8 | 3.0 | 2.3 |
| 40 to 49 | 5.9 | 10.5 | -4.5** | 1.9 |
| 50 to 59 | 4.1 | 5.6 | -1.5* | 0.9 |
| 60 or over | 2.6 | 2.6 | 0.0 | 0.0 |
| Race | | | | |
| White | 5.0 | 5.8 | -0.8 | 1.0 |
| Black | 9.4 | 0.9 | 8.4** | 3.7 |
| Hispanic | 4.0 | 4.7 | -0.7 | 5.9 |
| Other | 15.3 | 16.0 | -0.7*** | 0.2 |
| Gender | | | | |
| Female | 5.1 | 6.5 | -1.4 | 1.2 |
| Male | 7.8 | 9.9 | -2.1 | 1.7 |
| Family Composition | | | | |
| Married w/kids | 5.9 | 11.5 | -5.5*** | 1.7 |
| Unmarried w/kids | 9.3 | 11.6 | -2.3 | 3.7 |
| Married no kids | 4.0 | 5.9 | -1.9 | 1.9 |
| Unmarried no kids | 7.7 | 9.6 | -1.9 | 1.5 |
| Education | | | | |
| Less Than High School | 8.2 | 14.3 | -6.1 | 5.0 |
| High School Diploma | 5.2 | 5.5 | -0.3 | 1.0 |
| Some College | 7.6 | 9.3 | -1.7 | 1.7 |
| Bachelor's or More | 9.9 | 9.5 | 0.4*** | 0.1 |
| Health | | | | |
| Good or Excellent | 6.4 | 6.9 | -0.5 | 1.3 |
| Fair or Poor | 5.8 | 8.7 | -2.9 | 2.5 |

## Table IX-31 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area | | | | |
| Metro | 7.5 | 8.3 | -0.8 | 1.3 |
| Non-Metro | 4.4 | 6.5 | -2.1 | 1.6 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 7.1 | 5.4 | 1.7 | 1.9 |
| 4.4 to 5.1 | 8.5 | 6.6 | 1.8 | 2.0 |
| 5.1 to 6.0 | 6.2 | 7.3 | -1.1 | 1.6 |
| Greater than 6.0 | 4.0 | 8.4 | -4.4*** | 1.6 |

Source: State UI Administrative Data.

Notes: The outcome for all rows is the percentage of workers who lived in different zip codes at the time of job loss and the time of the follow-up interview. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table IX-32: Impacts on Good or Excellent Health by Subgroup (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 62.4 | 64.3 | -1.8 | 2.2 |
| Training Status[1] | | | | |
| Trainees | 66.1 | 65.8 | 0.3 | 2.5 |
| TRA only | 56.7 | 60.2 | -3.5 | 2.6 |
| Age | | | | |
| 16 to 29 | 76.0 | 93.3 | -17.3 | 14.8 |
| 30 to 39 | 75.4 | 76.6 | -1.2 | 3.6 |
| 40 to 49 | 60.9 | 62.2 | -1.3 | 3.1 |
| 50 to 59 | 54.3 | 58.8 | -4.5 | 2.8 |
| 60 or over | 55.3 | 63.4 | -8.1** | 4.0 |
| Race | | | | |
| White | 64.1 | 69.0 | -4.9** | 2.4 |
| Black | 62.4 | 54.1 | 8.4* | 4.3 |
| Hispanic | 55.0 | 53.5 | 1.5 | 4.9 |
| Other | 60.2 | 76.8 | -16.6* | 9.3 |
| Gender | | | | |
| Female | 62.7 | 64.1 | -1.4 | 2.9 |
| Male | 62.2 | 66.5 | -4.3** | 2.2 |
| Family Composition† | | | | |
| Married w/kids | 69.7 | 71.3 | -1.5 | 2.7 |
| Unmarried w/kids | 61.1 | 54.6 | 6.5 | 4.5 |
| Married no kids | 56.1 | 64.6 | -8.5*** | 3.0 |
| Unmarried no kids | 62.6 | 61.8 | 0.8 | 3.3 |
| Education | | | | |
| Less Than High School | 44.7 | 49.1 | -4.4 | 3.4 |
| High School Diploma | 65.0 | 63.7 | 1.3 | 2.9 |
| Some College | 64.8 | 72.0 | -7.2** | 3.5 |
| Bachelor's or More | 81.5 | 86.6 | -5.1** | 2.3 |
| Health | | | | |
| Good or Excellent | 74.0 | 75.9 | -1.9 | 2.3 |
| Fair or Poor | 16.4 | 20.6 | -4.2 | 2.8 |

## Table IX-32 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area | | | | |
| Metro | 62.7 | 62.9 | -0.2 | 2.3 |
| Non-Metro | 62.0 | 65.2 | -3.2 | 3.2 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 66.6 | 65.3 | 1.3 | 4.2 |
| 4.4 to 5.1 | 64.6 | 63.7 | 0.9 | 2.8 |
| 5.1 to 6.0 | 59.8 | 65.5 | -5.7* | 3.1 |
| Greater than 6.0 | 60.2 | 61.9 | -1.8 | 3.3 |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: The outcome for all rows is the percentage of workers who reported being in good or excellent health at all interviews after job loss. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table IX-33: Impacts on Work Limiting Conditions by Subgroup (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 16.2 | 12.9 | 3.3** | 1.6 |
| Training Status[1] | | | | |
| Trainees | 14.2 | 11.2 | 3.0 | 1.8 |
| TRA only | 21.5 | 17.4 | 4.1** | 2.1 |
| Age | | | | |
| 16 to 29 | 4.9 | 5.1 | -0.1*** | 0.0 |
| 30 to 39 | 7.7 | 9.8 | -2.1 | 1.7 |
| 40 to 49 | 15.7 | 12.8 | 2.9 | 2.1 |
| 50 to 59 | 23.0 | 21.7 | 1.3 | 3.0 |
| 60 or over | 23.3 | 21.5 | 1.8 | 3.5 |
| Race | | | | |
| White | 16.7 | 11.8 | 4.8** | 2.0 |
| Black | 16.3 | 19.3 | -3.0 | 3.2 |
| Hispanic | 12.9 | 14.7 | -1.8 | 83.2 |
| Other | 20.1 | 3.6 | 16.5*** | 3.6 |
| Gender | | | | |
| Female | 15.5 | 12.5 | 3.0 | 2.0 |
| Male | 17.1 | 12.0 | 5.0*** | 1.8 |
| Family Composition | | | | |
| Married w/kids | 9.8 | 7.6 | 2.2 | 1.5 |
| Unmarried w/kids | 15.5 | 16.2 | -0.6 | 3.2 |
| Married no kids | 19.1 | 16.7 | 2.4 | 2.3 |
| Unmarried no kids | 21.1 | 11.9 | 9.2*** | 2.7 |
| Education† | | | | |
| Less Than High School | 25.6 | 16.7 | 8.9** | 3.6 |
| High School Diploma | 14.2 | 12.8 | 1.4 | 1.9 |
| Some College | 17.6 | 5.7 | 11.9*** | 4.0 |
| Bachelors or More | 9.5 | 9.6 | 0.0*** | 0.0 |
| Health | | | | |
| Good or Excellent | 12.3 | 9.0 | 3.3** | 1.4 |
| Fair or Poor | 32.2 | 27.1 | 5.1 | 4.0 |

## Table IX-33 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area | | | | |
| Metro | 16.1 | 12.1 | 4.0** | 1.8 |
| Non-Metro | 16.5 | 14.0 | 2.5 | 2.2 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 14.1 | 13.9 | 0.2 | 2.3 |
| 4.4 to 5.1 | 14.5 | 15.3 | -0.8 | 2.2 |
| 5.1 to 6.0 | 17.7 | 19.2 | -1.5 | 2.4 |
| Greater than 6.0 | 18.0 | 7.0 | 10.9*** | 2.2 |

Source: Mathematica TAA Initial and Follow-Up Surveys.

Notes: The outcome for all rows is the percentage of workers who reported having a condition that limits the ability to work at the time of the follow-up survey. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Table IX-34: Impacts on Health Insurance Coverage by Subgroup (Survey Data)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Full Sample | 35.4 | 41.7 | -6.3*** | 2.1 |
| Training Status[1] | | | | |
| Trainees | 30.4 | 39.4 | -9.1*** | 2.2 |
| TRA only | 44.9 | 45.8 | -0.9 | 2.8 |
| Age | | | | |
| 16 to 29 | 12.2 | 33.1 | -20.8*** | 4.5 |
| 30 to 39 | 27.3 | 36.4 | -9.1** | 3.8 |
| 40 to 49 | 32.6 | 36.8 | -4.2 | 2.9 |
| 50 to 59 | 40.4 | 47.3 | -7.0** | 2.9 |
| 60 or over | 63.0 | 50.0 | 13.0** | 5.8 |
| Race† | | | | |
| White | 41.4 | 53.3 | -11.9*** | 2.6 |
| Black | 24.7 | 19.1 | 5.6** | 2.8 |
| Hispanic | 19.4 | 18.3 | 1.1 | 3.8 |
| Other | 29.9 | 65.8 | -35.9 | 869.4 |
| Gender | | | | |
| Female | 36.1 | 41.8 | -5.8** | 2.5 |
| Male | 34.8 | 42.1 | -7.4*** | 2.7 |
| Family Composition | | | | |
| Married w/kids | 41.0 | 47.9 | -6.9** | 3.1 |
| Unmarried w/kids | 8.7 | 11.7 | -3.1 | 2.6 |
| Married no kids | 54.1 | 59.5 | -5.4 | 3.3 |
| Unmarried no kids | 24.1 | 32.2 | -8.1*** | 2.8 |
| Education | | | | |
| Less Than High School | 23.3 | 22.6 | 0.7 | 3.2 |
| High School Diploma | 35.0 | 41.5 | -6.4** | 2.6 |
| Some College | 39.3 | 50.5 | -11.2*** | 4.2 |
| Bachelors or More | 61.4 | 70.6 | -9.1 | 8.3 |
| Health† | | | | |
| Good or Excellent | 36.4 | 45.4 | -9.0*** | 2.3 |
| Fair or Poor | 31.7 | 29.9 | 1.8 | 3.9 |

## Table IX-34 (continued)

| | TAA Participants | Comparison Group | Impact | Standard Error |
|---|---|---|---|---|
| Metropolitan Area | | | | |
| Metro | 36.4 | 42.1 | -5.7** | 2.5 |
| Non-Metro | 33.9 | 44.4 | -10.5*** | 2.7 |
| Local Unemployment Rate at UI Claim | | | | |
| Less than 4.4 | 40.9 | 45.0 | -4.1 | 3.1 |
| 4.4 to 5.1 | 39.1 | 43.9 | -4.8 | 3.7 |
| 5.1 to 6.0 | 34.0 | 38.2 | -4.2 | 3.2 |
| Greater than 6.0 | 29.6 | 38.8 | -9.3** | 3.6 |

Source: Mathematica TAA Initial and Follow-Up Surveys.

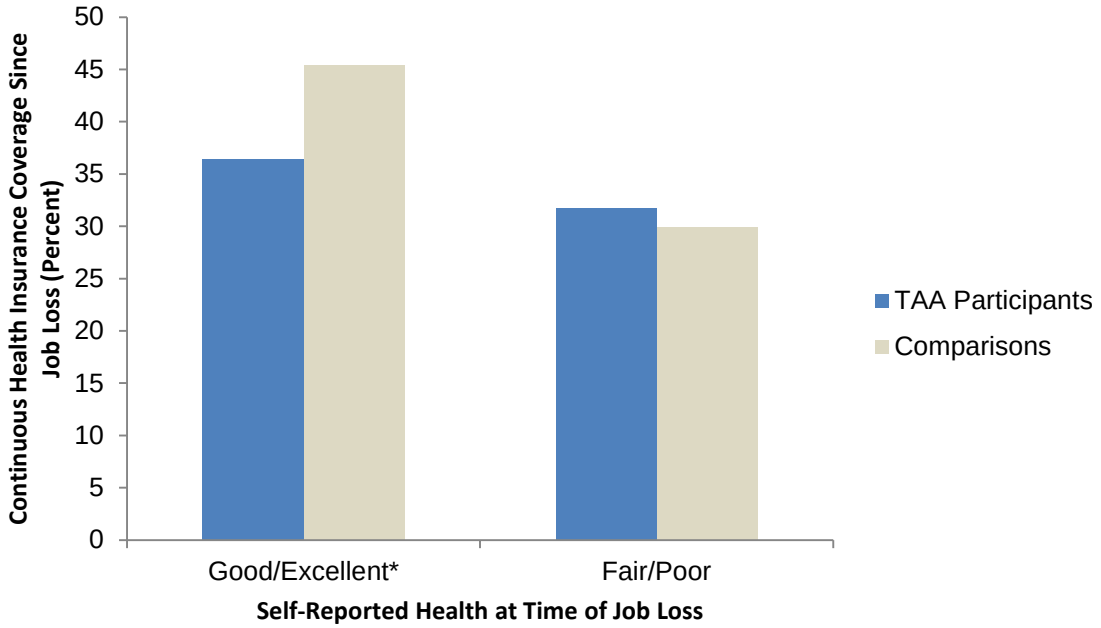
Notes: The outcome for all rows is the percentage of workers who reported being covered by health insurance for the entire time period from job loss to the follow-up interview. Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design. The sample is restricted to individuals who completed the follow-up survey.

[1] Training status may be affected by TAA participation, so the difference between participants and comparisons may not represent an impact.

*/**/*** Impact of TAA is significantly different from zero at the 0.10/0.05/0.01 level, two-tailed test.

† Impact of TAA is significantly different across subgroups at the 0.05 level, two-tailed test.

## Exhibit IX-5: Impact on Health Insurance Coverage by Baseline Health Status




Source: Mathematica TAA Initial and Follow-up Surveys.

Notes: Treatment group weights account for sample design and nonresponse, and comparison group weights are constructed using a kernel matching algorithm. Comparison group means and impacts are regression adjusted. Standard errors account for the two-stage sampling design.

* Impact of TAA is significantly different from zero at the 0.05 level, two-tailed test.

# X. CONCLUSIONS

The TAA program's chief goal is to help manufacturing workers rebound from job separation experienced as a consequence of foreign competition, by providing training, temporary income support, and other services. Using a quasi-experimental comparison-group design, we estimated the impact of TAA in achieving these objectives.

We found that TAA appears to be having significant and strong effects in providing participants with access to desired services. Participants in our sample were much more likely to access reemployment services than their comparisons, including services with the objective of helping workers find new jobs quickly, as well as those focused on longer term career planning. Moreover, participants were significantly more likely to access education and training, spending about 8 times as many weeks in these activities as the average comparison group member (49 versus 6 weeks), and obtaining educational credentials more than twice as often (51 percent of TAA participants compared with 21 percent of comparisons).

Given the substantial duration of their program participation, we would expect TAA to delay participants' onset of reemployment, and indeed that was the case. During the first two years after they lost their jobs—during what was essentially a period in program services for many of them—TAA participants were significantly less likely to be employed than comparisons and they earned substantially less. The hypothesized rebound after services ended did not fully materialize, however. As their participation in training and other TAA services drew to a close, participants began to catch up with their comparisons, but, even four years after job loss, they had not yet closed the gap. These broad patterns of results are remarkably consistent with those reported by Corson and his colleagues in their study of the TAA program nearly two decades ago (Corson et al. 1993).

Beyond these broad stroke findings, impacts for subpopulations of TAA workers suggest important lessons for program improvement. First, there is strong evidence that the program is ineffective for older workers; employment and earnings impacts were large and negative throughout the follow-up period for this group, and did not materially decrease over time. Furthermore, we found that TAA significantly increased older workers' retirement rates. Our results offer suggestive evidence that ATAA would be an appropriate strategy for improving both the short- and longer-term earnings of older participants. However, this program component is little used. Qualitative findings from the broader evaluation suggest reasons why: eligibility guidelines are restrictive (D'Amico et al. 2010) and this program component has not been strongly promoted (D'Amico et al. 2011). Thus, consideration might be given to strategies that would broaden program access and promote take up among older workers. An important focus for future research should be to provide a clearer understanding of ATAA's effects.

Second, there is little evidence that the TAA program has positive economic benefits for those who receive TRA in the absence of training. The TRA-only subgroup fared relatively poorly in contrast to their comparisons, and impacts on their employment and earnings were less favorable than for TAA trainees. Furthermore, the economic gains they realized through receipt of TRA did not wholly offset the income that they lost through their lower earnings. Thus, the program's current provision that TRA in the absence of training should only be allowed under limited waiver conditions seems on target.

Third, and by contrast, earnings effects were more encouraging for trainees, and, although our conclusions on this must be tentative, they seem especially promising for those who find jobs in occupations for which they received training. In light of these findings, ETA's continued efforts to promote training in occupations that are in high demand, coupled with program staff members' diligent efforts to place training completers into jobs that match their skills, should both be further emphasized. Obtaining more rigorous evidence of the link between occupational training and post-program earnings impacts is an important area for future research that could lend further credence to these suggestions.

Although these strategies taken together might improve program performance, it is worth speculating as to why the positive economic returns to the services TAA participants received are not already more fully in evidence. We suggest several reasons. First, about one third of TAA participants receive income support without undertaking training of any type. As we have just noted, impacts were particularly unfavorable for study participants who fell into this group, and there are compelling reasons why this finding might have been expected. Most obviously, it has been shown that the availability of UI can delay the return to employment among the unemployed (see, for example, Katz and Meyer 1990; Card and Levine 2000; Feldstein 2005; Card, Chetty, and Weber 2007; and Elsby, Hobijn, and Sahin 2010), and the same effect can be expected to hold true for TRA.[45] In fact, workers can receive waivers from the training requirement if their retirement is expected, which explicitly acknowledges that some in the TRA-only group are not expected to return to work (about nine percent of those in the TRA-only subgroup were listed as having received waivers from the training requirement due to their being within two years of retirement). Furthermore, case management and job placement assistance targeted to TAA participants not enrolled in training seem weak (Mack 2009). It appears, therefore, that the attraction of TRA benefits increased the duration of unemployment for many TRA-only workers, without an associated increase in their job search activities and eventual ability to obtain better quality jobs.

Even so, impacts on employment and earnings were only somewhat more favorable for the service subgroup that received training—by the fourth year of follow-up there were no positive impacts for them on average weeks worked, and impacts on earnings, although small, remained negative and statistically significant. The earnings of trainees were trending in a direction that indicates they might overtake their comparisons, and it is quite possible that we would have seen positive returns to TAA participation for the trainees if we had a longer follow-up period. But, by the fourth year, these positive impacts were not yet in evidence.

If there are to be eventual positive impacts, why are they taking so long to emerge? In studies of workforce investment programs it is quite common to see the treatment group record much

[45] Although a number of studies (including the ones cited here) report a significant effect of UI on the duration of unemployment, the magnitude of the effect is not clear. Decker (1997) reviews the literature and concludes that a one week increase in the potential duration of UI extends unemployment by between 0.1 and 0.5 weeks

lower earnings than comparisons during an "in-program period," but their earnings then rebound and overtake the comparisons' earnings shortly after the period of program participation ends. For example, in the national evaluation of the Job Training Partnership Act (JTPA), JTPA participants realized positive returns to their program participation by the thirtieth month after random assignment (Bloom et al. 1994), and Job Corps was similarly found to generate positive impacts on earnings for participants by about the beginning of the third year after random assignment (Schochet et al. 2001). Why, then, is it taking so long for TAA trainees to overtake their comparisons?

One possible reason is that the onset of the outcome measurement periods in the JTPA and Job Corps evaluations was the point at which program applicants were requesting services, whereas in our study, it was at the point of job loss, which was often many months before TAA participants began program services. In the Job Corps study, for example, participation rates for the treatment group were at their peak in the first quarter after random assignment (i.e., the beginning of their outcome measurement period). By contrast, we found that peak participation for TAA participants was not reached until the third quarter of their outcome measurement period. This finding reminds us that, after job loss, it takes TAA participants time to be notified about their eligibility for program services, attend orientation sessions to have services explained to them, and make decisions about whether to participate. Moreover, as we have noted, about 27 percent of participants were not yet eligible for TAA at the time of their trigger claims, which is when we began recording their outcomes.

Further, the duration of participation was typically much longer for our sample of TAA participants than it was for the JTPA and Job Corps participant samples. In the Job Corps study, the average duration of participation was eight months. For TAA trainees in our study, by contrast, the duration of participation was almost two years (including their time in training and receiving post-training services). Both the slower onset and longer duration of services, then, can help explain why it has taken longer for TAA participants to catch up to their comparisons than it did for JTPA or Job Corps participants.

Also relevant is a large body of literature that suggests that job loss can have lasting effects on workers' earnings (see, for example, Wachter 2011). Although medium-term estimates of the cost of job loss range somewhat across studies, the literature suggests that workers who lose their jobs typically earn about 20 percent less than similar workers who do not lose their jobs. We found this to be the case for our comparison group, who earned about 88 percent of their pre-UI hourly wage in their most recent jobs in the final follow-up year. Because TAA training substantially increases the time workers spend out of the labor force, it might not be surprising that the effects of job displacement take somewhat longer to resolve for TAA participants than for the typical displaced worker. Further compounding their problem of readjustment, TAA participants were significantly more likely than their comparisons to have switched industries and occupations, and in particular, were less likely to have been employed in the production industries from which they came. Thus, trainees may have been more likely than their comparisons to have started new careers, and therefore we might expect it to take a while for them to begin to show evidence of career advancement.

Finally, TAA trainees completed their training and re-entered the labor market when the nation's economy was mired in its worst economic recession since the Great Depression. As we saw in Chapter IV, about one half of TAA trainees completed their training—and, hence, presumably began their job searches—after the onset of the Great Recession. On the other hand, because they

spent less time in training, comparisons were more likely to have returned to the labor market before economic conditions deteriorated. Trainees may begin to see positive returns to their training investment when the labor market begins to rebound and they can make better use of the new job skills they have acquired. Only additional data covering a longer follow-up period will tell us for sure.

As a final note, the impacts presented in this report do not address the possible benefits of the TAA program in making free trade politically feasible. Historical evidence suggests that free trade agreements are enacted on the condition that the most affected workers are provided access to enhanced benefits and services that give them a transition period to recover from their job losses. We will address this important issue in the upcoming benefit-cost report, where we will present estimates of the value of free trade and discuss assumptions about the extent to which the TAA program makes free trade politically feasible.

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# APPENDIX

## OTHER REPORTS PREPARED TO DATE AS PART OF THE EVALUATION


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