APPENDIX A

DATA COLLECTION FOR IMPACT ANALYSES

This appendix describes the collection of data used in the impact analysis. The data come from four sources, which we discuss in turn:

1. The Study Tracking System (STS)
2. The 15-month follow-up survey
3. The long-term follow-up survey
4. Unemployment Insurance (UI) wage records data

1. STUDY TRACKING SYSTEM

We used the STS to collect data on all 7,920 customers who were randomly assigned. Information gathered included customers' characteristics, receipt of services, and outcomes related to the receipt of ITAs.

Customers and counselors recorded data on forms, and a clerk in each site entered the data from the forms into the STS. The three primary paper forms were:

1. ***The Baseline Information Form.*** Completed by all customers before they were randomly assigned, this short form collected (1) identifying and locating information, such as name, address, telephone number, social security number (SSN), and email address; (2) information on demographic and socioeconomic characteristics, such as race/ethnicity, education, employment, and family structure; and (3) information on receipt of public assistance.
2. ***Weekly Service Tracking Report.*** Completed by counselors, this form was used to record meetings and other interactions (such as phone calls) the counselors had with the customers.
3. ***Request for Training Funds Form.*** Customers completed this form once they had chosen the training program they wanted to be funded by an ITA. The form collected information about the training provider, the expected start date of the program, program costs, and other sources of funding for the program. The data were entered into the STS once the counselors approved the choice of training program.

The analysis in this report used an extract of data taken from the STS of July 2004. As the last person was randomly assigned in March 2004, the extract included at least three months of data on every customer. Nine months of STS data are available for 95 percent of the customers in the sample.

2. TWO FOLLOW-UP SURVEYS

Two follow-up surveys were designed to collect information on customers' experiences with obtaining an ITA, their training activities, and their employment outcomes. The 15-month follow-up survey was conducted between November 2003 and July 2005, and was given about 15 months after customers were found eligible for an ITA and randomly assigned to one of the three approaches. The long-term follow-up survey was conducted

between August 2009 and May 2010. On average, the survey took place 7 years after customers were found eligible for an ITA and randomly assigned to one of the three approaches, with a range of 5.5 to 8.5 years. Information collected from both surveys was used in the analysis; however, the final survey sample consists of those who responded to the long-term follow-up survey,z regardless of whether they responded to the 15-month follow-up survey. Including people who responded only to the 15-month survey would not allow us to analyze impacts on any long-term outcomes. At the same time, for those responding to only the long-term follow-up, the survey was adjusted to capture customers' entire training and employment histories from random assignment.

a. Sample Selection

We randomly selected 4,800 customers to survey from all 7,920 customers who were randomly assigned to one of the approaches. This selected sample was used for both the 15-month and the long-term follow-up samples. Because we needed to draw the survey sample and begin interviewing at 15 months, before study enrollment was completed, the sampling occurred in two stages. In the first stage, 4,040 customers were randomly selected from among customers who had been randomly assigned before July 2003 (Appendix Table A.1). In the second stage, an additional 760 customers were randomly selected from among customers who had been randomly assigned in July 2003 or later. We used a stochastic allocation procedure to ensure that the sampling rate was about the same across all sites. A total of 62.9 percent of customers were selected for the survey in the first stage, 50.8 percent in the second stage. The sampling rate was lower in the second stage because more people than expected were found eligible for an ITA after July 2003.

Appendix Table A.1. Survey Sample

	Enrollment Before July 2003		Enrollment During and After July 2003		Total Enrollment	
Site	Total	Survey Sample	Total	Survey Sample	Total	Survey Sample
Phoenix	474	296	172	88	646	384
Maricopa County	441	277	232	119	673	396
Bridgeport	627	394	406	206	1,033	600
Jacksonville	671	424	108	55	779	479
Atlanta	1,408	886	0	0	1,408	886
Northeast Region	171	106	0	0	171	106
North Cook County	1,538	967	271	137	1,809	1,104
Charlotte	1,096	690	307	155	1,403	845
Total	**6,426**	**4,040**	**1,496**	**760**	**7,922**[a]	**4,800**

[a]After the survey sample was selected, two duplicates were discovered in the sampling frame (two customers in Approach 2 in Charlotte). Thus, the actual number of unique customers in the population was 7,920.

b. Data Collection Mode

Both surveys were conducted primarily by telephone using computer-assisted telephone interviewing (CATI). Field staff attempted to locate in person the sample members who could not be located by telephone. Once located in person, sample members were handed a cell phone and asked to call the telephone survey center so that the interview could be conducted with CATI. In 31 cases for the 15-month survey and 12 cases for the long-term follow-up survey, the interview was conducted in person with a hard-copy instrument. Because the survey analysis sample consisted of those who responded to the long-term follow-up survey, the final long-term survey disposition for this sample is presented in Appendix Table A.2.

Appendix Table A.2. Final Survey Disposition of Long-Term Study Sample

	Count	Percentage
Total	**4,800**	
Completed	**3,264**	**68.0**
Complete phone	3,060	63.8
Complete field cell phone	192	4.0
Complete field hard copy	12	0.3
Duplicate	**2**	**0.0**
Located Noncomplete	**688**	**14.3**
Refusal by sample member	112	2.3
Refusal by household	4	0.7
Refusal sent to field	113	2.4
Language barrier	12	0.3
Illness/impaired	18	0.4
Away/unavailable	5	0.1
Deceased	79	1.7
Effort ended	315	6.6
Unlocated	**846**	**17.6**

c. Response Rates

Of the 4,800 sample members selected for the survey, 3,264 completed an interview (Appendix Table A.3)—for a response rate of 68 percent for the full sample.[1] The response rates for each approach were within 1 percentage point of each other. By site, the response rate varied from a low of 61 percent in Phoenix to a high of 74 percent in Atlanta.

[1] After the survey sample was selected, two duplicates were discovered in the sampling frame. Thus, the actual number of unique customers in the population was 7,920, of whom 4,798 were selected for the survey. We show response rates using the full sample of 4,800 customers.

Appendix Table A.3. Response Rates, by Approach and Site

Site	Sampled	Response Rate
Total	**4,800**	**68.0**
Approach 1	1,612	68.6
Approach 2	1,598	67.7
Approach 3	1,590	67.8
Phoenix	384	60.9
Maricopa County	396	68.9
Bridgeport	600	61.7
Jacksonville	479	68.9
Atlanta	886	73.8
Northeast Georgia	106	70.8
North Cook County	1,104	70.2
Charlotte	845	65.4

3. UNEMPLOYMENT INSURANCE WAGE DATA

Data on employment and earnings were obtained from the state UI agencies for all 7,920 customers who were randomly assigned.

a. Data Collection Strategy

UI wage records were collected from the state UI agencies in the six states included in the ITA experiment—Arizona, Connecticut, Florida, Georgia, Illinois, and North Carolina.[2]

We collected the data for a given state by sending a list of the SSNs for all customers in the experiment to the state UI agency, which matched UI wage records to each SSN and sent back a dataset containing UI wage records for each successful match. If a customer's SSN did not match records on databases at the state UI agency, we assumed that the person did not have UI-covered earnings during the period of the evaluation.

The data collected for the ITA evaluation covered a period of nine years. The record matching was performed by each state agency at two points in time. As many states archive wage records data every two to three years, we collected the data during four rounds—two for each record-matching process—to prevent the loss of data from early time periods. Based on the first set of matches, the first and second rounds of data collection included

[2] We obtained the Illinois data from the Administrative Data and Research Evaluation, an alliance of nine state partners. Each partner has negotiated data-sharing agreements with state agency owners of administrative data. These agreements permit controlled access to administrative data sources for authorized research and evaluation purposes that do not disclose the identity of individuals or business entities.

data from 2000 through 2003 and 2003 through 2005, respectively. Based on the second set of matches, the third and fourth rounds of data collection included data through the second quarter of 2008 and through the final quarter of 2009, respectively.

b. Measures Included in UI Earnings Records

Employers in most states are required to maintain and submit earnings records to the state's UI system for workers in jobs covered by UI. These records, which are maintained in machine-readable format, are used to determine workers' eligibility for UI if they are laid off.

The UI wage records include most but not all earnings; they consist of total quarterly earnings reported by employers to state UI agencies for each employee. By law, most employers are subject to a state UI tax and must report what is paid to each employee, including regular earnings, overtime, and tips and bonuses. In most states, the Federal Unemployment Tax Act (FUTA) applies to employers who (1) paid wages of $1,500 or more during any calendar quarter in the current or preceding calendar year, or (2) employed at least one worker for at least one day in each of the 20 weeks during the current or preceding calendar year.

Most workers are covered under FUTA, but there are some excluded categories. In particular, UI wage records do not cover federal workers, military staff, or self-employed people. Other workers excluded from coverage under the FUTA provisions include railroad employees, workers in service for relatives, most agricultural labor (except workers on large farms), domestic service workers whose employers paid less than $1,000 in wages in any calendar quarter, part-time employees of nonprofit institutions, some students employed by their schools, insurance and real estate agents on commission, and workers performing "casual labor" not in the course of the employer's business (U.S. Department of Labor 2004).

The UI wage records may not accurately reflect all earnings in UI-covered jobs. First, we collected UI data for each customer only from the state in which the person enrolled in the experiment. Thus, the earnings measured based on the UI wage records could underestimate customers' earnings if they worked outside their home states or moved during the follow-up period. Second, state UI agencies do not verify reported SSNs. Thus, the UI wage records could miss earnings from people with SSNs that were incorrectly reported by employers or sample members. Third, employers have financial incentives to underreport earnings to state UI programs, because earnings reported to UI agencies provide the basis for assessing the payroll tax that finances UI benefit payments.

The UI data received from each state contain quarterly earnings data for each reported job that customers held from approximately the first quarter of 2000 to the fourth quarter of 2009. For each state and calendar quarter available, we constructed total quarterly earnings for each sample member by summing reported earnings across each of the customer's employers.

For the analysis, we needed a measure of earnings for quarters measured in relation to random assignment rather than calendar quarters. To do this, we defined the first quarter after random assignment as the calendar quarter during which the customer was randomly assigned if the person was randomly assigned in the first half of the calendar quarter, and as

the calendar quarter after the customer was randomly assigned if the person was randomly assigned in the second half of the calendar quarter. For example, if a customer was randomly assigned on November 14, 2003, the fourth quarter in 2003 was designated as the first quarter after random assignment; if the customer was randomly assigned on November 16, 2003, the first quarter of 2004 was designated as the first quarter after random assignment.

APPENDIX B

WEIGHTING FOR SAMPLING AND NONRESPONSE AND TREATMENT OF MISSING VALUES AND OUTLIERS

This appendix discusses how we adjust for nonresponse and outliers. We begin by describing how we dealt with survey nonresponse to the long-term follow-up survey (Section 1). We then discuss how we impute missing values when a data item is missing because a respondent did not answer a particular question—item nonresponse—in either the 15-month or the long-term follow-up survey (Section 2). We end by examining our treatment of outliers (Section 3).

1. WEIGHTS FOR SAMPLING AND SURVEY NONRESPONSE

Nonresponse occurs when sample members cannot be located, as well as when they refuse to respond to the survey. Although the completion rate was high for the long-term follow-up—68 percent—survey nonresponse can still lead to biased impact estimates if respondents differed from nonrespondents in characteristics correlated with the outcomes of interest. Importantly, how the characteristics of nonrespondents are correlated with the outcomes of interest may differ between our ability to locate sample members and the located sample members' refusal to participate. To adjust for any differences in observed characteristics between long-term follow-up respondents and nonrespondents, we created weights for every survey respondent.

In addition to adjusting for nonresponse, we constructed the weights developed for each survey respondent to account for the sampling process. First, we constructed the weights to "undo" the impacts of the different sampling rates before and after July 2003 (Appendix A), so that customers are represented equally irrespective of *when* they were randomly assigned. Second, we rescaled the weights so that the weighted total number of survey respondents equals the total number of customers in the experiment. Hence, the weight assigned to each respondent was made up of four parts: an adjustment (1) for variation in sampling rates, (2) for unlocated customers, (3) for survey nonresponse among the located customers, and (4) to ensure that the weighted number of respondents equals 7,920—the total number of customers randomly assigned. We discuss each part of the weight construction process next.

Adjustment for Variation in Sampling Rates. To adjust for the differential sampling rate in the first and second stages of the selection of the survey sample, we assigned a base sampling weight of:

$$W_{sampling} = \frac{\text{Population Counts in Sampling Stage}}{\text{Count of Sampled Cases in Sampling Stage}}.$$

Thus, for customers who were selected for the survey sample in the first stage, the base sample weight was $W_{sampling} = \frac{6,426}{4,040} = 1.59$, while customers selected in the second stage had a base sample weight $W_{sampling} = \frac{1,496}{760} = 1.97$. Because of the stochastic allocation procedure used to select customers, the probability of selection is the same for all customers within each stage.

Survey Nonresponse Weights. The 4,800 sampled customers were first classified into two categories: located and unlocated. All located customers were further classified into two groups: respondents and nonrespondents. We classified groups separately based on their locate status and their response status because the customers who were more likely to be located did not always share the same characteristics as the customers who were more likely to respond to the survey. Therefore, we created nonresponse weights to adjust for customer characteristics that were related to survey completion at each step.[3]

Using the baseline information form completed by all customers, as well as from UI earnings records in five sites, we compared the characteristics of located customers to those of unlocated customers (Table B.1), as well as characteristics of survey respondents to those of nonrespondents among the located customers (Table B.2). We found that a larger number of customer characteristics were associated with the likelihood of being located when compared to the number associated with responding. Within some of the sites, the following characteristics were associated with the likelihood of being located: age, gender, race, marital status, having a phone, having an email address, number of persons living in the household, receiving public assistance, months of employment in previous year, having a driver's license, education level, dislocated worker/adult, and self-reported earnings in the past year (Table B.1). Within some of the sites, the following characteristics were associated with the likelihood of response: age, race, dislocated worker/adult, marital status, having an email address, receiving public assistance, household size, and education (Table B.2).

The base sampling weights were first adjusted to compensate for the sample members who could not be located. A common method for computing this weight adjustment is to form weighting cells of sample members with similar characteristics and to use the inverse of the cell response rate as the adjustment factor for sampled members in that cell. The weighting cells are formed to ensure sufficient counts in each class to make the adjustment more stable (that is, to have a smaller variance). The weighting cells were defined by the following variables:

- ***Phoenix:*** dislocated worker/adult, self-reported earnings in the past year, past two months of earnings from the UI administrative data, and marital status
- ***Maricopa County:*** education, race, and gender
- ***Bridgeport:*** has ***driver*** license, dislocated worker/adult, race, and age
- ***Jacksonville:*** dislocated worker/adult, race, and age
- ***Atlanta: marital*** status, has email address, self-reported earnings in the past year, race, and age
- ***Northeast Georgia:*** dislocated worker/adult

[3] For the 15-month follow-up survey, 5 percent of the customers were unlocated, and 13 percent of the located customers did not respond. Because this is considered a high rate of locating customers, we did not calculate unlocated adjustment factors separately when analyzing the 15-month follow-up sample. However, for the long-term follow-up survey, 18 percent of the customers were unlocated, and 17 percent of those located did not respond. Given the nearly matching rates of locating customers and survey response of located customers, a two-stage adjustment was preferred.

- ***North Cook County:*** marital status, dislocated worker/adult, earnings in past three months from the UI administrative data, and age

- ***Charlotte: dislocated*** worker/adult, education, gender, and age

For each weighting cell, the unlocated adjustment was calculated by dividing the sum of the number of located and unlocated customers in each cell by the number located in the cell.

$$Adj_{Unlocated,cell} = \frac{\text{Number of Located and Unlocated}_{\text{Cell}}}{\text{Number of Located}_{\text{Cell}}}$$

Among the located customers, we constructed the nonresponse weights to adjust for differences in characteristics between respondents and nonrespondents in a process similar to that for the locate status weights. Again, the construction of these weights involved grouping survey respondents and nonrespondents into cells based on variables that were related to the probability of responding in that site and the number of people who shared certain characteristics within those sites. These cells were defined by the following variables:

- ***Phoenix: sampling*** stage and earnings in the past two months from the UI administrative data

- ***Maricopa County:*** dislocated worker/adult, and earnings in the past two months from the UI administrative data

- ***Bridgeport:*** gender, months worked last year, and household size

- ***Jacksonville:*** gender, age, and education

- ***Atlanta: education***, gender, age, marital status, public assistance receipt in past year

- ***Northeast Georgia:*** marital status

- ***North Cook County:*** age, education, gender, and household size

- ***Charlotte:*** gender, age, and race

- For each cell, we calculated the nonresponse adjustment by dividing the sum of the number of respondents and nonrespondents in each cell by the number of respondents in the cell.

- $$Adj_{Nonresp,cell} = \frac{\text{Number of Respondents and Nonrespondents}_{\text{Cell}}}{\text{Number of Respondents}_{\text{Cell}}}.$$

Appendix Table B.1. Percentage of Located and Unlocated Sampled Members, by Site and Other Characteristics

	City of Phoenix (384)		Maricopa County (396)		Bridgeport (600)		Jacksonville (479)		Atlanta (886)		Northeast Georgia (106)		Northern Cook County (1,104)		Charlotte (843)	
	L	UL	L	UL	L	UL	L	UL	L	UL	L	UL	L	UL	L	UL
All (Counts)	309	75	329	67	434	166	394	85	761	125	88	18	962	142	675	168
Age	**				***		***		***				***		***	
Under 35	24.92	38.67	13.98	17.91	44.24	63.86	31.47	40.00	29.57	40.80	37.50	38.89	18.40	30.28	26.67	43.45
35 to 45	26.54	29.33	28.27	34.33	27.42	23.49	29.70	32.94	31.27	38.40	28.41	33.33	32.74	29.58	37.48	33.93
45 to 55	32.69	25.33	39.82	32.84	20.51	10.24	26.40	25.88	28.52	17.60	23.86	27.78	34.62	33.80	27.41	19.05
Over 55	15.86	6.67	17.93	14.93	7.83	2.41	12.44	1.18	10.64	3.20	10.23	0.00	14.24	6.34	8.44	3.57
Gender	*															
Male	45.63	58.67	51.67	52.24	35.25	28.31	38.58	45.88	43.36	38.40	67.05	66.67	56.55	50.00	44.74	46.43
Female	54.37	41.33	48.33	47.76	64.75	71.69	61.42	54.12	56.64	61.60	32.95	33.33	43.45	50.00	55.26	53.57
Ethnicity			**		***											
Non-Hispanic	70.87	73.33	85.71	73.13	79.95	65.66	93.91	92.94	97.11	96.80	98.86	100.00	92.41	92.96	96.89	98.21
Hispanic	29.13	26.67	14.29	26.87	20.05	34.34	6.09	7.06	2.89	3.20	1.14	0.00	7.59	7.04	3.11	1.79
Race			**		***		***		***				***			
Black	27.18	37.33	10.64	10.45	47.00	52.41	30.96	47.06	56.77	72.00	47.73	55.56	11.23	21.83	66.07	67.86
Native American, Asian, or other	18.45	20.00	7.60	17.91	17.05	27.11	8.38	16.47	5.12	8.80	0.00	5.56	20.48	21.83	5.19	6.55
White	54.37	42.67	81.76	71.64	35.94	20.48	60.66	36.47	38.11	19.20	52.27	38.89	68.30	56.34	28.74	25.60
Marital Status	***				***		*		***				***			
Married or living together	44.66	22.67	55.62	55.22	26.96	15.66	44.67	37.65	53.75	36.00	40.91	44.44	57.69	36.62	40.44	32.14
Separated, divorced, or widowed	30.42	32.00	26.44	28.36	22.12	21.08	34.77	30.59	23.13	31.20	30.68	38.89	20.79	28.87	28.15	27.38
Never married	24.92	45.33	17.93	16.42	50.92	63.25	20.56	31.76	23.13	32.80	28.41	16.67	21.52	34.51	31.41	40.48
Has Phone	***				*				**				**			
No	1.29	10.67	0.30	0.00	3.46	7.23	1.52	1.18	0.39	2.40	2.27	0.00	0.62	2.82	2.37	2.98
Yes	98.71	89.33	99.70	100.00	96.54	92.77	98.48	98.82	99.61	97.60	97.73	100.00	99.38	97.18	97.63	97.02
Has Email	***												**		**	
No	49.51	69.33	24.01	28.36	59.45	66.87	40.61	40.00	25.76	40.00	56.82	61.11	24.64	33.80	37.19	48.21
Yes	50.49	30.67	75.99	71.64	40.55	33.13	59.39	60.00	74.24	60.00	43.18	38.89	75.36	66.20	62.81	51.79
Has Driver's License	***				***											
No	7.44	24.00	0.91	1.49	20.05	39.16	1.02	3.53	1.18	1.60	2.27	0.00	4.26	2.82	4.74	7.74
Yes	92.56	76.00	99.09	98.51	79.95	60.84	98.98	96.47	98.82	98.40	97.73	100.00	95.74	97.18	95.26	92.26
People in Household	*										**		*			
1	20.39	33.33	20.97	25.37	17.97	15.06	21.07	22.35	18.13	24.00	13.64	38.89	23.39	32.39	23.85	25.00
2	23.30	14.67	32.22	23.88	24.88	27.11	26.40	22.35	26.41	24.80	27.27	5.56	24.53	16.20	25.93	23.81
3	22.98	16.00	18.84	14.93	22.81	23.49	25.13	23.53	20.24	20.80	26.14	22.22	22.35	23.94	21.04	22.62
4	19.09	17.33	14.89	16.42	20.05	19.88	15.48	12.94	22.08	18.40	15.91	27.78	18.61	19.01	17.63	16.67
5 or more	14.24	18.67	13.07	19.40	14.29	14.46	11.93	18.82	13.14	12.00	17.05	5.56	11.12	8.45	11.56	11.90
Education			*		***										***	
GED or less	30.74	33.33	10.03	7.46	21.20	31.33	17.26	21.18	7.88	8.80	34.09	33.33	4.16	5.63	6.67	9.52
High school	32.36	34.67	35.26	41.79	41.71	40.36	34.52	41.18	33.25	39.20	37.50	38.89	21.41	26.06	36.74	52.38
Vocational	13.27	17.33	11.85	20.90	18.66	16.87	16.50	15.29	13.27	15.20	13.64	16.67	8.32	11.97	12.44	10.71
Business or professional	13.27	12.00	19.15	17.91	9.22	9.64	17.01	15.29	15.64	16.00	3.41	5.56	14.35	12.68	17.78	16.67
Bachelor's, master's, or doctorate	10.36	2.67	23.71	11.94	9.22	1.81	14.72	7.06	29.96	20.80	11.36	5.56	51.77	43.66	26.37	10.71

	City of Phoenix (384)		Maricopa County (396)		Bridgeport (600)		Jacksonville (479)		Atlanta (886)		Northeast Georgia (106)		Northern Cook County (1,104)		Charlotte (843)	
	L	UL	L	UL	L	UL	L	UL	L	UL	L	UL	L	UL	L	UL
Earnings	***				*		*		**		**					
None	12.94	12.00	10.64	13.43	17.74	19.88	5.33	7.06	8.15	11.20	4.55	27.78	14.55	17.61	11.56	11.31
Under $4,000	12.62	30.67	5.78	8.96	19.59	27.71	10.91	17.65	10.25	20.80	19.32	5.56	7.90	9.86	14.37	17.86
$4,000-$15,000	27.18	37.33	25.53	28.36	36.41	34.34	29.44	37.65	22.08	20.00	29.55	16.67	15.18	17.61	30.07	33.33
$15,000-$30,000	26.86	16.00	30.70	23.88	18.89	14.46	31.73	22.35	25.10	23.20	34.09	38.89	25.05	27.46	25.04	24.40
$30,000-$50,000	13.92	4.00	17.93	16.42	6.68	2.41	15.74	12.94	22.34	18.40	11.36	11.11	18.92	16.20	12.30	5.36
More than $50,000	6.47	0.00	9.42	8.96	0.69	1.20	6.85	2.35	12.09	6.40	1.14	0.00	18.40	11.27	6.67	7.74
Work Limitation					*											
No	91.91	96.00	94.53	94.03	96.31	99.40	90.61	94.12	97.24	96.80	96.59	88.89	96.05	98.59	95.70	95.24
Yes	8.09	4.00	5.47	5.97	3.69	0.60	9.39	5.88	2.76	3.20	3.41	11.11	3.95	1.41	4.30	4.76
Assistance	***				***										***	
No	77.02	61.33	94.22	88.06	63.59	48.80	91.37	92.94	86.99	81.60	84.09	88.89	96.47	93.66	80.15	68.45
Yes	22.98	38.67	5.78	11.94	36.41	51.20	8.63	7.06	13.01	18.40	15.91	11.11	3.53	6.34	19.85	31.55
Working Now							*						*			
No	91.26	90.67	98.78	98.51	75.58	69.88	81.98	74.12	92.64	91.20	98.86	100.00	99.48	97.89	90.22	86.90
Yes	8.74	9.33	1.22	1.49	24.42	30.12	18.02	25.88	7.36	8.80	1.14	0.00	0.52	2.11	9.78	13.10
Months Worked Last Year	**															
None	12.30	10.67	11.55	13.43	17.97	19.88	5.33	5.88	8.41	10.40	5.68	27.78	11.75	15.49	12.15	13.69
0-3	12.30	20.00	11.85	17.91	17.05	18.67	9.14	9.41	13.40	12.00	18.18	11.11	14.55	14.79	16.89	17.26
3-6	19.09	30.67	23.71	16.42	20.51	25.90	17.26	16.47	21.81	28.00	23.86	16.67	20.79	24.65	23.70	23.81
6-9	26.21	22.67	26.44	22.39	22.35	17.47	27.16	29.41	28.65	22.40	19.32	16.67	23.49	16.20	25.63	25.00
9-12	30.10	16.00	26.44	29.85	22.12	18.07	41.12	38.82	27.73	27.20	32.95	27.78	29.42	28.87	21.63	20.24
Treatment																
Treatment 1	35.28	25.33	33.13	35.82	33.41	31.93	35.28	30.59	33.90	32.80	32.95	38.89	33.26	33.80	33.33	35.71
Treatment 2	32.36	38.67	36.17	23.88	32.26	36.75	32.74	32.94	33.11	32.80	31.82	33.33	33.68	31.69	32.30	35.71
Treatment 3	32.36	36.00	30.70	40.30	34.33	31.33	31.98	36.47	32.98	34.40	35.23	27.78	33.06	34.51	34.37	28.57
Dislocated Worker	***				***		**						***		***	
No	33.33	66.67	27.96	29.85	55.53	75.90	37.82	50.59	19.32	25.60	61.36	44.44	12.58	25.35	26.07	38.69
Yes	66.67	33.33	72.04	70.15	44.47	24.10	62.18	49.41	80.68	74.40	38.64	55.56	87.42	74.65	73.93	61.31
Sampling Stage	*						**									
First	75.08	85.33	69.00	74.63	67.51	60.84	87.06	95.29	100.00	100.00	100.00	100.00	88.25	83.10	82.52	78.57
Second	24.92	14.67	31.00	25.37	32.49	39.16	12.94	4.71	n.a.	n.a.	n.a.	n.a.	11.75	16.90	17.48	21.43

Note: */**/***: The *p*-value for Fisher's Exact test was significant at the .10/.05/.01 level.
L = located; n.a. = not applicable; UL = unlocated.

Appendix Table B.2. Percentage of Respondents and Nonrespondents Among Located Sample Members, by Site and Other Characteristics

	City of Phoenix (309)		Maricopa County (329)		Bridgeport (434)		Jacksonville (394)		Atlanta (761)		Northeast Georgia (88)		Northern Cook County (962)		Charlotte (675)	
	R	NR	R	NR	R	NR	R	NR	R	NR	R	NR	R	NR	R	NR
All (Counts)	234	75	273	56	370	64	330	64	654	107	75	13	775	187	553	122
Age	*												***		**	
Under 35	23.08	30.67	13.19	17.86	44.86	40.63	32.12	28.13	28.59	35.51	37.33	38.46	16.77	25.13	25.68	31.15
35 to 45	23.93	34.67	29.30	23.21	28.65	20.31	29.39	31.25	31.35	30.84	29.33	23.08	31.74	36.90	35.80	45.08
45 to 55	35.90	22.67	39.19	42.86	18.92	29.69	27.58	20.31	29.20	24.30	21.33	38.46	35.87	29.41	29.66	17.21
Over 55	17.09	12.00	18.32	16.07	7.57	9.38	10.91	20.31	10.86	9.35	12.00	0.00	15.61	8.56	8.86	6.56
Gender					***										**	
Male	43.59	52.00	51.65	51.79	32.43	51.56	36.97	46.88	42.35	49.53	68.00	61.54	57.03	54.55	42.50	54.92
Female	56.41	48.00	48.35	48.21	67.57	48.44	63.03	53.13	57.65	50.47	32.00	38.46	42.97	45.45	57.50	45.08
Ethnicity																
Non-Hispanic	71.37	69.33	84.62	91.07	79.73	81.25	93.64	95.31	97.09	97.20	100.00	92.31	92.65	91.44	96.93	96.72
Hispanic	28.63	30.67	15.38	8.93	20.27	18.75	6.36	4.69	2.91	2.80	0.00	7.69	7.35	8.56	3.07	3.28
Race	*														**	
Black	23.93	37.33	10.62	10.71	48.11	40.63	29.39	39.06	55.96	61.68	46.67	53.85	10.58	13.90	67.09	61.48
Native American, Asian, or other	20.09	13.33	7.69	7.14	16.22	21.88	8.18	9.38	5.20	4.67	0.00	0.00	20.26	21.39	4.16	9.84
White	55.98	49.33	81.68	82.14	35.68	37.50	62.42	51.56	38.84	33.64	53.33	46.15	69.16	64.71	28.75	28.69
Marital Status									*		**					
Married or living together	46.58	38.67	57.88	44.64	26.76	28.13	45.15	42.19	55.05	45.79	45.33	15.38	57.55	58.29	39.78	43.44
Separated, divorced, or widowed	29.91	32.00	25.64	30.36	22.70	18.75	33.94	39.06	23.24	22.43	25.33	61.54	20.26	22.99	28.75	25.41
Never married	23.50	29.33	16.48	25.00	50.54	53.13	20.91	18.75	21.71	31.78	29.33	23.08	22.19	18.72	31.46	31.15
Has Phone																
No	0.85	2.67	0.00	1.79	3.24	4.69	1.21	3.13	0.46	0.00	1.33	7.69	0.39	1.60	2.17	3.28
Yes	99.15	97.33	100.00	98.21	96.76	95.31	98.79	96.88	99.54	100.00	98.67	92.31	99.61	98.40	97.83	96.72
Has Email									*				**			
No	47.01	57.33	24.54	21.43	60.00	56.25	41.21	37.50	24.62	32.71	56.00	61.54	22.97	31.55	36.53	40.16
Yes	52.99	42.67	75.46	78.57	40.00	43.75	58.79	62.50	75.38	67.29	44.00	38.46	77.03	68.45	63.47	59.84
Has Driver's License																
No	5.98	12.00	0.73	1.79	21.35	12.50	1.21	0.00	0.92	2.80	2.67	0.00	4.39	3.74	5.06	3.28
Yes	94.02	88.00	99.27	98.21	78.65	87.50	98.79	100.00	99.08	97.20	97.33	100.00	95.61	96.26	94.94	96.72
People in Household					**								**			
1	20.09	21.33	19.05	30.36	15.68	31.25	22.12	15.63	18.04	18.69	12.00	23.08	23.87	21.39	24.59	20.49
2	24.36	20.00	32.97	28.57	24.86	25.00	26.36	26.56	26.15	28.04	26.67	30.77	23.10	30.48	24.95	30.33
3	21.79	26.67	19.05	17.86	24.05	15.63	23.33	34.38	19.72	23.36	25.33	30.77	24.00	15.51	22.42	14.75
4	19.23	18.67	15.38	12.50	21.08	14.06	15.76	14.06	22.32	20.56	17.33	7.69	18.71	18.18	17.18	19.67
5 or more	14.53	13.33	13.55	10.71	14.32	14.06	12.42	9.38	13.76	9.35	18.67	7.69	10.32	14.44	10.85	14.75
Education							*		*				***			
GED or less	29.06	36.00	9.52	12.50	21.35	20.31	15.45	26.56	7.03	13.08	30.67	53.85	4.13	4.28	7.23	4.10
High school	35.04	24.00	35.90	32.14	41.62	42.19	36.97	21.88	32.57	37.38	37.33	38.46	19.87	27.81	36.17	39.34
Vocational	11.11	20.00	12.45	8.93	18.38	20.31	16.36	17.19	12.84	15.89	14.67	7.69	7.23	12.83	12.48	12.30
Business or professional	14.10	10.67	18.32	23.21	9.46	7.81	16.97	17.19	16.21	12.15	4.00	0.00	14.45	13.90	18.44	14.75
Bachelor's, master's, or doctorate	10.68	9.33	23.81	23.21	9.19	9.38	14.24	17.19	31.35	21.50	13.33	0.00	54.32	41.18	25.68	29.51

	City of Phoenix (309)		Maricopa County (329)		Bridgeport (434)		Jacksonville (394)		Atlanta (761)		Northeast Georgia (88)		Northern Cook County (962)		Charlotte (675)	
	R	NR	R	NR	R	NR	R	NR	R	NR	R	NR	R	NR	R	NR
Earnings											**					
None	13.25	12.00	9.52	16.07	17.03	21.88	5.45	4.69	8.41	6.54	2.67	15.38	13.94	17.11	11.21	13.11
Under $4,000	12.39	13.33	5.49	7.14	18.38	26.56	11.52	7.81	10.40	9.35	14.67	46.15	7.23	10.70	14.47	13.93
$4,000-$15,000	25.64	32.00	25.27	26.79	36.76	34.38	30.00	26.56	22.02	22.43	33.33	7.69	14.97	16.04	31.65	22.95
$15,000-$30,000	27.35	25.33	32.60	21.43	19.46	15.63	31.52	32.81	24.31	29.91	36.00	23.08	24.65	26.74	24.23	28.69
$30,000-$50,000	14.96	10.67	17.58	19.64	7.57	1.56	14.85	20.31	21.87	25.23	12.00	7.69	19.61	16.04	11.75	14.75
Over $50,000	6.41	6.67	9.52	8.93	0.81	0.00	6.67	7.81	13.00	6.54	1.33	0.00	19.61	13.37	6.69	6.56
Work Limitation																
No	93.16	88.00	94.14	96.43	96.22	96.88	90.00	93.75	97.25	97.20	97.33	92.31	96.00	96.26	95.48	96.72
Yes	6.84	12.00	5.86	3.57	3.78	3.13	10.00	6.25	2.75	2.80	2.67	7.69	4.00	3.74	4.52	3.28
Assistance									*							
No	76.50	78.67	94.87	91.07	64.05	60.94	91.52	90.63	87.92	81.31	86.67	69.23	96.26	97.33	79.57	82.79
Yes	23.50	21.33	5.13	8.93	35.95	39.06	8.48	9.38	12.08	18.69	13.33	30.77	3.74	2.67	20.43	17.21
Working Now																
No	90.60	93.33	98.90	98.21	75.41	76.56	82.73	78.13	92.05	96.26	98.67	100.00	99.35	100.00	90.24	90.16
Yes	9.40	6.67	1.10	1.79	24.59	23.44	17.27	21.88	7.95	3.74	1.33	0.00	0.65	0.00	9.76	9.84
Months Worked Last Year					***											
None	12.39	12.00	10.26	17.86	17.03	23.44	5.45	4.69	9.02	4.67	4.00	15.38	11.35	13.37	11.57	14.75
0-3	13.68	8.00	10.99	16.07	14.59	31.25	9.39	7.81	13.61	12.15	17.33	23.08	14.58	14.44	17.54	13.93
3-6	17.09	25.33	22.71	28.57	21.62	14.06	17.58	15.63	21.56	23.36	22.67	30.77	20.39	22.46	23.87	22.95
6-9	24.79	30.67	28.57	16.07	23.78	14.06	26.36	31.25	29.20	25.23	22.67	0.00	23.61	22.99	25.68	25.41
9-12	32.05	24.00	27.47	21.43	22.97	17.19	41.21	40.63	26.61	34.58	33.33	30.77	30.06	26.74	21.34	22.95
Treatment			*													
Treatment 1	35.47	34.67	32.23	37.50	32.43	39.06	36.06	31.25	34.10	32.71	34.67	23.08	33.03	34.22	34.36	28.69
Treatment 2	32.91	30.67	34.43	44.64	32.70	29.69	31.82	37.50	34.25	26.17	30.67	38.46	34.19	31.55	31.10	37.70
Treatment 3	31.62	34.67	33.33	17.86	34.86	31.25	32.12	31.25	31.65	41.12	34.67	38.46	32.77	34.22	34.54	33.61
Dislocated Worker			**													
No	31.20	40.00	25.64	39.29	54.86	59.38	36.97	42.19	18.35	25.23	58.67	76.92	12.65	12.30	27.12	21.31
Yes	68.80	60.00	74.36	60.71	45.14	40.63	63.03	57.81	81.65	74.77	41.33	23.08	87.35	87.70	72.88	78.69
Sampling Stage	**												***			
First	72.22	84.00	68.13	73.21	68.38	62.50	86.97	87.50	100.00	100.00	100.00	100.00	86.84	94.12	83.54	77.87
Second	27.78	16.00	31.87	26.79	31.62	37.50	13.03	12.50	n.a.	n.a	n.a	n.a.	13.16	5.88	16.46	22.13

Note: */**/***: The *p*-values for Fisher's Exact test was significant at the .10/.05/.01 level.

n.a. = not applicable; NR = nonrespondents; R = respondents.

Ensuring that the Weights Sum to the Population Total. To compute final survey weights, we ratio-adjusted the preliminary weights to ensure that, within strata defined by site, approach, and dislocated/adult worker status, the final weights added up to the population total. The following poststratification adjustment was thus made to each customer's weight:

$$Adj_{Poststratification,cell} = \frac{\text{Population Count}_{\text{Post}}}{\text{Weighted Number of Respondents}_{\text{Post}}}.$$

The final weight, a combination of the base sampling weight, the unlocated adjustment, the nonresponse adjustment, and the poststratification adjustment, was thus calculated as:

$$W_{Final,i,cell,post} = W_{sampling} * Adj_{Unlocated,cell} * Adj_{Nonresponse,cell} * Adj_{Poststratification,post}.$$

2. IMPUTING VALUES FOR ITEM NONRESPONSE

This section describes how we dealt with *item* nonresponse—nonresponses to particular survey questions in each of the two follow-up surveys.

a. Overview of Imputation Strategy

There are very few missing data as a whole. When data are missing, however, our strategy to include people with missing items in the analysis depended upon the type of data item that was missing. When the item was a reported outcome that was independent of other reported items and item nonresponse was small, we simply excluded that person from the analysis. When the item was a covariate or a single component of a constructed outcome of interest, or could simply be replaced with a reasonable substitute, we imputed the data. For example, one simple imputation procedure we used was to assign the 15th as the day of the month when the day was the only information missing from a reported date.[4]

For covariates, it was important that we include all data for each sample member—otherwise, we would need to drop that sample member from all analyses. Hence, for most covariates, we imputed the value based on the mean of the observed data (for continuous covariates) or the most common value (for categorical variables). For the race/ethnicity variables, we included nonresponders in the "other" category.

For missing employment, earnings, and training outcomes that were constructed from multiple data items, however, we used a hot-deck procedure (described below) to impute missing data for the episode instead of omitting the episode with missing data from the

[4] The exceptions to this were if it would cause a conflict with observed data. If a job or training episode was reported to have started and ended in the same month, but the days were not reported, we imputed values by assigning the 10th of the month for the missing start day and the 20th for the missing end day. Another example is if a customer reported starting a job on March 23, 2003, which would make an end date of the 15th impossible. In such cases we imputed the end day as being halfway between the reported start day and the end of the month.

sample member's outcomes.[5] The first reason is that when performing analysis on constructs that use multiple data items, omitting from a construct sample members with any items missing would be equivalent to assuming that the value of the missing construct is equal to the overall mean of the observed data.[6] However, we often know, based on other nonmissing construct components, that sample members' values are not equal to the overall mean, so omitting them would produce biased results. For example, when analyzing quarterly earnings, excluding a sample member who has missing hours worked per week but who reports being employed and specifies an hourly wage would produce biased estimates on earnings, since employed workers have higher earnings than nonemployed workers. A second reason is that there is a relatively high rate of missing data for constructed outcome variables because they are created using many survey "building-block" data items. For example, quarterly earning constructs require job start date, job end date, hourly wages, and hours worked per week. If we were to exclude earnings constructs with any missing information, a missing month would lead to missing employment and earnings data for that job.

We chose to impute the building-block data items rather than the composite outcome variables—such as earnings by quarter—because this made use of all the information we had, and so we imputed the minimum amount of information necessary to construct the outcome variables. For example, suppose a customer worked two jobs after random assignment. All relevant information is available for the first job, but for the second job, we know only that the end date was September 5, 2004, and that the start date was sometime in 2004, but the start month and day were not given. Our procedure will impute the start month as sometime between January and August. The full information on the first job can then be used, along with the reported earnings and end date for the second job and the imputed start date, in constructing the earnings and employment history for that customer. A procedure that imputed only the constructed earnings and employment outcome variables would not easily allow this full use of all available reported information.

We present in two different ways the percentage of missing values that were hot-decked. Table B.3 reports the number of job or training episodes with item nonresponse that required hot-deck imputation for each specific item. Since a person can have multiple jobs and trainings, one individual can contribute multiple episodes, but people with no episodes are excluded from the table. The table provides an overall sense of the amount of imputations that were needed for the item constructs. The relatively high percentage of hot-decking required for job end dates was due mostly to the matching of jobs across the two surveys. For jobs that were ongoing at the time of the 15-month follow-up survey, the end date had to be imputed if a match with a reported job in the long-term follow-up could not be made.

[5] There are a few exceptions. The timing of job episodes was critical for the definition of our outcomes. We excluded from the analysis 11 people who reported one or more employment episodes that had no beginning or end date, since their jobs could not be reasonably attributed to any time period. Also, training episodes that had timing information completely missing (no reported information on start or end of training) were assumed to have taken place during the first three years of follow-up. However, outcomes involving the timing and duration of these trainings were excluded from the analysis.

[6] Excluding data also affects the estimated standard errors, which would be smaller if the individual were included at the mean value.

Appendix Table B.3. Episodes with Variables Imputed Using the Hot-Deck Procedure for Item Nonresponse

Variable	Percentage of Item Nonresponse[a]			
	Structured Choice	Guided Choice	Maximum Choice	Overall
Job Start Month	3.0	3.5	3.1	3.2
Job Start Year	0.8	0.7	0.6	0.7
Job End Month[b]	17.4	18.6	18.7	18.2
Job End Year[b]	16.3	17.0	17.2	16.9
Hours Worked per Week at Job	1.5	1.2	1.4	1.4
Earnings at Job	6.9	7.7	6.8	7.1
Union Status at Job	0.4	0.2	0.3	0.3
Health Insurance Received at Job	0.9	1.0	0.6	0.8
Paid Time Off at Job	0.8	0.7	0.8	0.7
Retirement Benefits at Job	2.0	1.4	2.1	1.8
Training Program Start Month[c]	5.5	5.5	3.8	4.9
Training Program Start Year[c]	1.8	1.1	0.6	1.2
Training Program End Month[c]	4.1	4.3	3.2	3.9
Training Program End Year[c]	1.6	1.3	0.6	1.1
Training Program Cost[d]	22.0	22.7	27.2	23.9

[a]Indicates the percentage of employment/training episodes in the full sample with missing data on the item. We exclude people with no episodes. There are a total of 9,768 job episodes from the two surveys combined, 2,462 training episodes from the 15-month follow-up survey, and 2,423 from the long-term follow-up survey.

[b]All end date values are influenced by jobs that were ongoing at the time of the 15-month follow-up that could not be matched to jobs reported in the long-term follow-up survey.

[c]Training period dates were imputed only for those reported in the 15-month follow-up. The reported percentages are based on training episodes from that survey.

[d]All item nonresponse for training costs is from the long-term follow-up survey, since it is the only one that asked customers to report training costs. The reported percentages are based on training episodes from that survey.

Table B.4 reports the percentage of people with any hot-decked value in the creation of one of their constructs. Here we count people with no job or training episodes as not requiring hot-decking, because constructed outcomes for these people are still well defined. Comparing the two tables clarifies the importance of imputation for constructed variables. For example, only 17 to 18 percent of all job episodes required hot-decking, but nearly 50 percent of people had a job episode that required hot-decking for a job end date. Large gaps in these people's employment histories would have occurred had the various components of the job characteristics not been imputed. Importantly, the rates of item nonresponse are similar across the three approaches. Hence, the imputation procedures are unlikely to create bias in the impact estimates.

Appendix Table B.4. Individuals with Variables Imputed Using the Hot-Deck Procedure for Item Nonresponse

Variable	Percentage of Item Nonresponse[a]			
	Structured Choice	Guided Choice	Maximum Choice	Overall
Job Start Month	7.2	7.9	6.7	7.3
Job Start Year	2.3	1.8	1.7	1.9
Job End Month[b]	46.5	46.6	49.3	47.5
Job End Year[b]	45.1	45.1	47.4	45.9
Hours Worked per Week at Job	3.8	3.3	4.1	3.8
Earnings at Job	14.9	15.5	13.8	14.7
Union Status at Job	1.1	0.6	0.9	0.9
Health Insurance Received at Job	2.4	2.6	1.9	2.3
Paid Time Off at Job	2.1	1.8	2.0	1.9
Retirement Benefits at Job	4.6	3.9	5.7	4.7
Training Program Start Month[c]	3.9	3.5	2.6	3.3
Training Program Start Year[c]	1.4	0.7	0.4	0.8
Training Program End Month[c]	2.7	2.8	2.1	2.5
Training Program End Year[c]	1.1	0.8	0.4	0.8
Training Program Cost[d]	11.5	11.8	13.1	12.1

[a]Indicates the percentage of people in the full sample with any missing data on the item. People with no job/training episodes are included as not missing the variable, since their outcomes are still well defined. A total of 3,253 people had defined job outcomes, and 3,264 had defined training outcomes.

[b]All end date values are influenced by jobs that were ongoing at the time of the 15-month follow-up and could not be matched to jobs reported in the long-term follow-up survey.

[c]Training period dates were imputed only for those reported in the 15-month follow-up. The reported percentages are based on training episodes from that survey, though the population considered is the full analysis sample.

[d]All item nonresponse for training costs is from the long-term follow-up survey, since this is the only one that asked customers to report training costs.

b. Hot-Deck Imputation Procedure

We chose a hot-deck procedure for this analysis because it accommodates the imputation of plausible values given a set of constraints. This is important when imputing dates; we can ensure, for example, that the end date for a job must be after the start date. These constraints would be difficult to implement using other imputation approaches, such as a model-based or mean-imputation procedure (Little and Rubin 2002).

The hot-deck procedure is implemented separately for each variable. The procedure randomly selects an individual with a nonmissing value for the variable (the "donor") and matches the person to an individual with a missing value for the variable (the "recipient") based on a set of additional variables for which the donor and recipient have similar values. The donor's observed value on the variable of interest is then imputed for the missing value for the recipient. A sequential nearest-neighbor hot-deck procedure was implemented using a SAS macro described in Carlson et al. (1995).

The hot-deck procedure first groups survey respondents into mutually exclusive groups of people who all share the same values for a set of categorical matching variables. Within these groups, individuals are ranked according to a different set of sorting variables, some of which may be continuous.[7] Based on these rankings, a donor is chosen for each recipient. Because all matching variables considered in this application are categorical, the procedure will essentially choose as a donor a random individual who has the same values on all matching variables and similar values on all sorting variables as the recipient.

The different timing of the employment episodes reported in the 15-month and the long-term follow-up survey necessitated two slightly different procedures when imputing employment variables. The differences are based on the categorical variables that were used for matching as well as the variables that were used for sorting. The long-term follow-up survey had many more reported job episodes, which allowed greater flexibility when determining the mutually exclusive groupings, and the potential length of the job episodes in the long-term survey required more precise groupings for when jobs could take place, to disallow for censoring of jobs with potentially long durations. For both surveys, the matching variables were tailored based on what was known about the episode. For example, some imputations used start month or year, others used end month or year, others a combination of start and end characteristics (if only months were missing). In some cases, imputations were based on durations rather than specific dates.

For the 15-month follow-up survey, where missing dates for training episodes were imputed (as well as for jobs), potential donors all had the same values as the recipient on the following variables, with priority in the following order[8]:

1. Approach

[7] Sorting variables are distinct from categorical variables in that they are sorted according to level of importance and may also be measured as continuous variables. When a chosen categorical variable creates groupings of a small number of people, that variable can be used as a sorting variable to increase cell size.

[8] For some imputations, this list was modified if there were an insufficient number of donors available given the full set of matching variables.

2. Dislocated/adult worker status
3. State or grantee
4. Nonmissing month/year of start/end date for episode with missing information (if possible)
5. Two-digit job or training occupation code (for hours, earnings, and training dates)

For the long-term follow-up survey, potential donors all had the same values as the recipient on the following variables, with priority in the following order[9]:

1. Full-/part-time job (except for imputation of job hours)
2. Approach
3. Nonmissing month/year of start/end date for job with missing information (if possible)

While sorting, variables were determined by:

1. Dislocated/adult worker status
2. State or grantee
3. Two-digit job occupation code

For new training programs reported in the long-term follow-up, we assigned training only to certain starting periods instead of imputing start and end dates. This was because we were more interested in analyzing when training occurred and less interested in the duration of the training. However, the reported training costs in the long-term follow-up were needed in order to perform the benefit-cost analysis. For these cost variables, potential donors all had the same values as the recipient on the following variables:

1. Approach
2. Type of training provider (private vendor, community college, etc.)
3. Training duration (which we used as a sorting variable)

These matching and sorting variables were chosen because they are believed to be strongly associated with the job and training program characteristics of interest. For the 15-month follow-up survey, job and training episodes were imputed separately depending on the order in which they were reported. However, given the large pool of job episodes in the long-term follow-up, all job episodes were combined and imputed together.

Maintaining consistency is complicated when imputing dates. For this reason, for job episodes imputed from the 15-month follow-up, we did the imputations of dates in the order of days, years, and months. That still resulted in a few inconsistencies between the months and years, such as imputed start dates after imputed end dates, which we corrected either (1) by redoing individual imputations (by constraining the imputation to ensure that start dates came before end dates), or (2) by imposing another correction (such as adjusting an imputed end date that fell after the interview date).

[9] In some instances, these matching variables were tailored depending on what was known about the episode. For example, for some imputations, start date was used, and for others, end date was used.

For the long-term follow-up, in many circumstances the imputation procedure took the approach of imputing employment durations and then calculating the date the job would end or begin, given the imputed duration. This was done using various constraints on the job to be imputed, such as not allowing a full-time job to extend beyond a date when another full-time job was reported to have started.

To ensure that the imputations were reasonable, we implemented a series of checks that involved examining the individual imputations of the building-block variables as well as examining the outcome variables constructed from the building blocks. These checks included:

- Examining the implied quarterly earnings for each individual with imputed data, to ensure that the imputations did not result in extreme outliers, or imposing our top-coding (discussed below) to those outliers

- Examining whether people in different ITA approaches required varying levels of hot-decking

- Examining the length of time from random assignment until the time the surveys were taken across the ITA approaches (because matching variables were sensitive to the timing of the reported job episode since random assignment)

- Comparing the distributions of quarterly earnings for people with imputed data and people with complete data and confirming that any differences observed were reasonable and not due to inappropriate imputations

- Comparing the distributions of duration in training for people with imputed data and people with complete data and confirming that any differences observed were reasonable and not due to inappropriate imputations for the 15-month follow-up survey

Discrepancies found as a result of these checks resulted in fine-tuning of the imputations to ensure their consistency and appropriateness.

3. TREATMENT OF OUTLIERS

Sometimes reported values did not seem reasonable. The survey-based variables that appeared to have some outliers were household income, total number of hours worked each week, earnings reported, and training costs reported. Based on its distribution, we top-coded household income at $125,000 in 2002 dollars, which was above the 95th percentile. When a person reported working more than 99 hours per week across all jobs, we capped the hours at 99. Although this was an extremely high number of hours per week, people who reported these values typically did not sustain such high weekly hours for very long. Across all reported jobs, when taking the maximum reported hours over a person's employment history, 99 hours per week represented the 97th percentile. When hours worked per week exceeded 99, in order to include earnings from all jobs, we adjusted earnings downward across all jobs to reflect a 99-hour work week.

There were also situations when reported wages were too high or too low. When someone not self-employed reported a job that paid $0, we excluded this job episode from the person's work history.[10] We did not exclude job episodes that reported a $0 wage if the person was self-employed. All wages were then bottom-coded at $2.50 in 2002 dollars. We also set a top code on hourly wages of $55 in 2002 dollars, which was at the 99th percentile of reported hourly wages and is about three standard deviations above the mean.

In the long-term follow-up, there were instances of reported training costs that did not seem reasonable based on the type of provider. Based on the distribution of training costs reported by provider type, we applied the following top codes for total training costs by the following providers:

1. Private vendor = $12,936
2. Community college = $22,000
3. Vocational training = $19,297
4. Four-year college = $56,760
5. Other = $17,180

[10] Some examples of these "jobs" were volunteer positions that some people held.

Appendix C

Estimation of impacts, Standard Errors, and net benefits

This appendix describes how we estimated the relative impacts of the three ITA approaches. Because customers were randomly assigned to the three approaches, a simple difference in the mean outcome measures for customers in two approaches provides an unbiased estimate of the impact of one approach versus another. However, we estimated the impacts using a regression model, both to increase precision and to adjust for chance differences in the characteristics of customers in the three approaches.[11] The model used is described in detail below.

1. REGRESSION MODEL FOR ESTIMATING OVERALL IMPACTS OF THE THREE APPROACHES

a. Form of the Model

Our estimates of the relative impacts of the three approaches are based on a comparison of customers randomly assigned to one of the three approaches with customers randomly assigned to another approach. In presenting the model, we refer to Structured Choice, Guided Choice, and Maximum Choice as Approach 1, Approach 2, and Approach 3, respectively. To compute the relative impacts of each approach, we estimated a statistical model that predicts the outcome of interest as a function of approach, site, and a set of background characteristics, detailed below. The basic form of the model is:

$$y_i = \sum_{s=1}^{8} \beta_s S_{si} + \sum_{s=1}^{8} \beta_{1s} S_{si} A_{1i} + \sum_{s=1}^{8} \beta_{3s} S_{si} A_{3i} + \delta X_i + \varepsilon_i, \quad \text{(C.1)}$$

where

y_i is the outcome of interest

S_{si} equals 1 if customer i was in site s and 0 if not

A_{1i} equals 1 if customer i was in Structured Choice (Approach 1) and 0 if not

A_{3i} equals 1 if customer i was in Maximum Choice (Approach 3) and 0 if not

X_i is a vector of baseline characteristics of customer i

ε_i is a random error term that captures the impacts of unobserved factors that influence the outcome. It is assumed to have a mean of zero conditional on {A}, {X}, and {S}

The β and δ terms are parameters or vectors of parameters to be estimated

[11] Appendix D presents results from a sensitivity analysis that estimates impacts using differences-in-means rather than using regression models. The results do not differ much.

The regression models are estimated using weights to account for the sampling design and unit survey nonresponse (see Appendix A).

b. Estimation of Impacts

The parameters of greatest interest are β_{1s} and β_{3s} because they show the impact on customers of being in Approach 1 (or 3) in site *s*, relative to being in Approach 2. These parameters can thus be interpreted as the causal impact of being assigned to Approach 1 (or 3) rather than being assigned to Approach 2, in site *s*. The β_{1s} and β_{3s} terms provide the estimates of the relative impacts of Approach 1 (or 3) versus Approach 2 within each site. The relative impact of Approach 1 versus Approach 3 in site s is obtained by computing $\tau_{13s} = \beta_{1s} - \beta_{3s}$. Thus, within each site (s=1 to 8) we obtain three impact estimates:

$$\tau_{12s} = \beta_{1s}$$
$$\tau_{32s} = \beta_{3s}$$
$$\tau_{13s} = \beta_{1s} - \beta_{3s}$$

To obtain the average impact across all sites, we computed a weighted average of the impacts in each site, where the weight is denoted by W_s:

$$\tau_{12} = \sum_{s=1}^{8} W_s \beta_{1s}$$
$$\tau_{32} = \sum_{s=1}^{8} W_s \beta_{3s}$$
$$\tau_{13} = \sum_{s=1}^{8} W_s (\beta_{1s} - \beta_{3s})$$

The site weights used in the above formulas are the proportion of customers in each site. This is equivalent to pooling all customers across sites and weighting each customer equally, regardless of site of origin. Our rationale for pooling across sites is based on three factors: (1) all sites were asked to implement the same three approaches; (2) the implementation of the three ITA approaches was similar across our study sites; (3) while the contextual factors do vary across the sites, we saw them as having had a limited influence on the outcomes of ITA study participants by approach. Appendices E through G present the results separately by site, and Appendix D presents results obtained when sites are weighted equally.

c. Choice of Linear Regression

For all outcomes we estimate the parameters in Equation C.1 using ordinary least squares, which models the outcome as a linear function of the predictors. An alternative would have been to use logistic regression for binary outcomes such as employment status. Logistic regression models the "log odds of success" as a linear function of the predictors:

$$g(\pi_i) = \log\left(\frac{\pi_i}{1-\pi_i}\right) = \beta X_i + e_i, \text{ where } \pi_i = E(y_i).$$

We chose to use linear regression rather than a logistic regression for all binary outcomes for a few reasons. The first was simplicity of both analysis and presentation. There is not a standard way of estimating or presenting standard error estimates for impacts estimated using logistic regression, whereas the calculation and presentation is very straightforward using linear regression. Second, during the first analysis of binary outcomes using the 15-month follow-up survey, a series of sensitivity analyses concluded that linear and logistic regressions led to nearly identical estimates and statistical inference for most binary outcomes and no meaningful differences (McConnell et al. 2006).

d. Regression Predictors

The predictors included in the regression model (the X variables in Equation C.1) were demographic characteristics (age, sex, race/ethnicity), marital status, presence of children, education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline and earnings in 12 months prior to baseline). These were selected using preliminary investigation of variables predictive of outcomes using a stepwise variable selection procedure (Neter et al. 1996), as well as substantive knowledge.

e. Estimating Subgroup Impacts

A slight simplification to the model was used when estimating impacts for subgroups of customers, such as dislocated workers or adult workers. In particular, to allow efficient estimation of the parameters of key interest for subgroups—the overall impact across all sites for each subgroup—we do not include site indicators in the model when estimating subgroup impacts. Including the site indicators and interactions with the subgroup indicator would greatly increase the number of parameters in the model and may result in less precise estimation of the overall subgroup impacts. The model used for subgroups is thus:

$$y_i = \beta_1 A_{1i} + \beta_2 A_{2i} + \beta_3 A_{3i} + \gamma_0 G_i + \gamma_1 G_i A_{1i} + \gamma_3 G_i A_{3i} + \delta X_i + \varepsilon_i, \quad \text{(C.2)}$$

where the variables are defined as above, and $G_i = 1$ if customer *i* is in group *G* and equals 0 otherwise. The relative impacts for subgroup *G* are calculated as:

$$\tau_{G=1,12} = (\beta_1 - \beta_2) + \gamma_1$$
$$\tau_{G=1,32} = (\beta_3 - \beta_2) + \gamma_3$$
$$\tau_{G=1,13} = (\beta_1 - \beta_3) + \gamma_1 - \gamma_3$$

Similarly, the impacts for customers not in subgroup *G* (G=0) are:

$$\tau_{G=0,12} = (\beta_1 - \beta_2)$$
$$\tau_{G=0,32} = (\beta_3 - \beta_2)$$
$$\tau_{G=0,13} = (\beta_1 - \beta_3)$$

Tests of whether the impacts differ for customers who are and are not in subgroup *G* were conducted by taking the difference of the above impacts for those in subgroup *G* minus the impacts for those not in subgroup *G*.

The subgroups for which we estimate the relative impacts of the three approaches (results in Appendices E-J) are based on:

4. Dislocated workers versus adult workers
5. Education: customers with at most versus more than a high school degree
6. Customers with versus without a vocational certification at the time of random assignment
7. Age: customers over versus under age 40
8. Sex: female versus male customers
9. Race/ethnicity: nonminority customers (white non-Hispanic) versus minority (black, Hispanic, Asian, other) customers

2. CALCULATING STANDARD ERRORS

To determine whether impact estimates are statistically significant, we computed standard errors that account for the study's sample design and, in particular, for the clustering of customers within sites. For outcomes from the survey, we use regression procedures for complex survey data that calculate correct standard errors given the sampling and nonresponse weights (described in Appendix A) and the clustering of customers in sites (Brogan 1998).[12] For outcomes based on the full population of customers—such as from the UI wage records or the STS—we used the same procedure but did not use the individual weights, since we did not need to account for survey sampling or survey nonresponse.

The calculation of standard errors reflects the fact that the ITA sites were chosen purposively, not randomly. Sites had to be willing and had to apply to participate in the experiment, and so are not nationally representative. The results thus generalize only to the set of sites in this study, and not to a broader population.

3. CALCULATING NET BENEFITS

The estimation of net benefits requires the addition of costs at the time of program implementation with benefits that accrue over time. For each benefit type, impacts had to be summed under the assumptions discussed in Chapter VIII. To include impacts over time, we used the following formula to add impacts over time for each benefit type:

$$\sum_{q=1}^{22} \frac{\beta_q}{(1+r)^{\text{floor}\left(\frac{q-1}{4}\right)}} + \beta^* \sum_{q=23}^{4(R-42)} \frac{1}{(1+r)^{\text{floor}\left(\frac{q-1}{4}\right)}},$$

Where

- β represents impacts on a given benefit in quarter q,
- β^* represents impacts in each quarter from Quarter 23 until the time of retirement,

[12] Specifically, we used the "svy" command in Stata 10 to estimate the model, and the "lincom" command to perform significance tests of linear combinations of the coefficients, such as to calculate the overall impact across all sites, or the relative impact of Approach 1 versus Approach 3.

- *r* is the selected discount rate, and
- *R* is the selected age of retirement, and 42 is subtracted from this value because it represents that benefits are set up to capture the customer with the median age at program entry.

APPENDIX D

SENSITIVITY ANALYSES

To assess the robustness of our impact estimates to different estimation procedures and assumptions, we conducted a series of sensitivity analyses. We first identified primary outcomes of interest and then estimated the impacts of switching from Guided Choice to one of the other approaches under different assumptions. Appendix Table D.1 summarizes the findings by presenting the benchmark impact estimates in the "Benchmark" column along with the following sensitivity analyses:

10. Conducting an unweighted analyses
11. Estimating impacts without using regression adjustment
12. Estimating impacts with sites weighted equally

1. UNWEIGHTED ANALYSES

For all outcomes constructed using the survey data, the main impacts presented in the text are estimated using weights that adjust for the survey sampling probabilities and survey nonresponse as described in Appendix B. To assess the effect of this weighting, we also estimated impacts for the survey-based outcomes without any weights. Those results are presented for key outcomes in the "Unweighted" column of Appendix Table D.1.

The results are similar to those in the main analyses that use weights; the magnitudes and significance levels change only slightly. For example, when switching from Guided Choice to Maximum Choice, all the impacts with significant differences maintain significant differences, but two of the training outcomes move from one significance level to the next. The estimated impacts themselves are very similar across the weighted and unweighted analyses.

2. WITHOUT REGRESSION ADJUSTMENT

We also estimated impacts without any covariates in the regression models. This is equivalent to calculating simple differences in means of the outcomes between the approaches, with no adjustments for covariates. The results from this analysis are presented for key outcomes in the column "No Regression Adjustment" in Appendix Table D.1. The results again are very similar to those in the main analyses, which indicates that the regression adjustment did not substantially affect the estimates.

Appendix Table D.1 Sensitivity of Impacts on Primary Outcomes of Switching from Guided Choice to Another Approach

	Switch to Structured Choice				Switch to Maximum Choice			
	Benchmark	Unweighted	No Regression Adjustment	Sites Weighted Equally	Benchmark	Unweighted	No Regression Adjustment	Sites Weighted Equally
Training Outcomes[a]								
Attended Training Program (%)	2	2	2	1	5***	4**	5***	6**
Weeks in Training Program	2	2	2	1	2	1	2	1
Completed a Training Program (%)	4**	4**	5**	5*	6***	5**	6***	8***
Labor Market Outcomes During Final Two Years of Follow-up[b]								
Percentage of Quarters Employed	1	0	1	-1	1	1	1	2
Average Quarterly Earnings	522**	470**	597**	602**	254	313	237	381
Employed in Occupation of Training[a]	5***	5***	6***	6**	2	2	2	5*
Well-Being and Self-Sufficiency Outcomes During Final Year of Follow-up[c]								
Household Income ($)	1,019	570	1,653	1,393	-796	-1,170	-876	-1,001
Household Income Below the Poverty Line	0	1	0	1	0	1	0	1
Received Unemployment Insurance[d]	0	-1	-1	0	-1	-1	-1	-3
Received Food Stamps or Cash Assistance[d]	-1	0	-1	0	1	2	1	2

Sources: 15-month follow-up and long-term follow-up survey.

- Notes: The impacts are based on a comparison of means which were regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.
- [a]Training outcome measures are defined for training that started within the first three years of follow-up.
- [b]The final two years of follow-up are defined as eight 13-week quarters immediately preceding the interview date of the second follow-up survey. The second follow-up survey was collected from August 6, 2009, through May 26, 2010.
- [c]The final year of follow-up is defined as the 12 months immediately preceding the date of the second follow-up survey.
- [d]Receipt of unemployment insurance, food stamps, or cash assistance is based on reports of receipt by anyone in the household.

*** / ** / * Statistically significant positive impact at the .01/.05/.10 level.

3. WEIGHTING SITES EQUALLY

The final sensitivity analysis we conducted was to weight sites equally in computing the overall impacts, rather than weight by the number of customers in each site. Appendix Table D.2 presents the weights used to calculate overall impacts from the impacts by site in the main analysis and when each site is weighted equally. The results from these analyses are presented for key outcomes in the column "Sites Weighted Equally" in Appendix Table D.1.

Appendix Table D.2 Site Weights (Percentages)

	Sites Weighted by Size (Main Analysis)	Sites Weighted Equally (Sensitivity Analysis)
Phoenix	8.2	12.5
Maricopa County	8.5	12.5
Bridgeport	13.0	12.5
Jacksonville	9.8	12.5
Atlanta	17.8	12.5
Northeast Georgia	2.2	12.5
North Cook County	22.8	12.5
Charlotte	17.7	12.5
Total	**100**	**100**

Most findings are not sensitive to how the sites are weighted. There are mostly only small changes in magnitude and levels of statistical significance, but there is one difference where an outcome that was not statistically significant in our main analysis was marginally significant at the 10 percent level in the sensitivity check: for those switching from Guided Choice to Maximum Choice, customers were more likely to be employed and trained in the same occupation when sites are weighted equally.

APPENDIX E

SUPPLEMENTAL TABLES ON TRAINING OUTCOMES (CHAPTER V)

Appendix Table E.1. Impacts on Participation in Training

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Attended Training Program (percentages)						
Within 3 years of RA	73	71	77	2	5***	-3*
Starting at least 3 years after RA	23	24	23	-1	-0	-1
Weeks in Training						
Within 3 years of RA	31	29	30	2	2	1
After 3 years since RA	19	19	19	0	-0	0
Sample Size	**1,105**	**1,081**	**1,078**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

Because the outcomes presented could not be defined for some customers, the sample size ranges are
A1: 1,092 to 1,105
A2: 1,064 to 1,081
A3: 1,056 to 1,078

RA = random assignment.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table E.2. Differences in Timing and Length of Training Among Those Who Trained

	Means			Conditional Differences		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Weeks Until First Training Entry[a]:	20	18	17	2	-1	3*
Number of Weeks in Training						
Within 3 years of RA	31	29	30	2	2	1
After 3 years since RA	19	19	19	0	-0	0
Sample Size	**1,092**	**1,064**	**1,056**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Means were computed using only people who participated in any training. Because these are nonrandom samples of the full groups, differences in means across approaches cannot be interpreted as the impact of one approach as compared with another.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

Because the outcomes presented could not be defined for some customers, the sample size ranges are
A1: 799 to 1,092
A2: 757 to 1,064
A3: 798 to 1,056

[a]If a person was in training at the time of random assignment, weeks until first program entry is 0.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

RA = random assignment.

Appendix Table E.3. Impacts on Reasons for Not Participating in Training

	Means			Impacts		
Reason[a]	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Got a Job or Looking for a Job	7	9	6	-2*	-3***	1
Financial Reasons/ Insufficient Funding	4	4	4	-0	0	-1
Not Interested in Training	3	1	1	1**	-0	2***
Personal Reasons	2	1	1	0	0	0
No Available Programs	1	1	2	-1	0	-1
Other	1	1	1	-0	0	-0
Problems with Counseling	1	1	1	0	-0	1
Unaware of Program	1	1	1	-0	-0	0
Did Not Get into a Program	1	1	1	-0	0	-0
No Suitable Program	0	1	0	-0	-0	0
Timing Too Late/Too Long	0	1	0	-0	-0	0
Decided Training Not Worthwhile	0	0	0	0	0	0
Sample Size	**1,105**	**1,081**	**1,078**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Note: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

[a]People who participate in training are assigned values of 0.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table E.4. Impacts on Sources of Funding for Training

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Participated in ITA-Funded Training[a]	59	56	62	2	6***	-4*
Other Funding Sources[a]						
Personal savings	21	25	25	-4**	-0	-4**
Student loan	8	10	10	-2*	-1	-2
Need-based financial aid	11	11	13	0	2	-2
Other	14	15	14	-1	-1	-0
Sources Other than an ITA Paid for All Training[a]	15	15	14	-0	-1	1
Sample Size	**1,105**	**1,081**	**1,078**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Sources of funding for training within the first 3 years of a person's follow-up period that are reported for any of their training episodes are included.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

[a]People who did not participate in training are assigned values of 0.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table E.5. Differences in Sources of Funding for Training Among ITA Customers Who Participated in Training

	Means			Conditional Differences		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Participated in ITA-Funded Training	79	78	80	1	3	-2
Other Funding Sources						
Personal savings	28	34	32	-7***	-2	-4*
Student loan	11	14	12	-3*	-1	-1
Need-based financial aid	16	15	17	1	2	-2
Other	19	21	18	-2	-3	1
Sample Size	**812**	**773**	**819**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Sources of funding are included if a customer reported funding from the source for any of their training episodes. Means were computed using only people who participated in any training within the first 3 years of follow-up. Because these are nonrandom samples of the full groups, differences in means across approaches cannot be interpreted as the impact of one approach as compared with another.
Sources of funding for training within the first 3 years of a person's follow-up period are included.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table E.6. Impacts on Characteristics of Training Programs Attended

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Number of Training Programs Attended	1.1	1.0	1.1	0.1	0.1***	-0.0
Attended Training Provided by:						
Private	37	30	34	7***	4**	3
Community college	19	22	24	-3*	2	-5***
Vocational training center	13	14	13	-1	-1	0
4-year college or university	8	8	8	-0	0	-0
Other	12	12	11	-0	-1	1
Attended Training for:						
General education	14	14	16	0	2	-1
Occupation or specific skill	66	65	68	1	3*	-2
Attended Training Intended to:						
Prepare for new occupation	45	47	49	-1	2	-4
Improve skills in current occupation	30	27	28	3*	0	3
Sample Size	**1,105**	**1,081**	**1,078**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Characteristics are included across all a participant's reported training episodes. Participation in training is counted for those who trained during the first 3 years of the follow-up period.

The approach means and conditional differences are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table E.7. Characteristics of Training Programs Attended by ITA Customers Who Trained

	Means			Conditional Differences		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Number of Training Programs Attended	1.4	1.4	1.4	0.0	0.0	-0.0
Attended Training Provided by:						
Private	49	42	44	7***	2	5**
Community college	26	30	32	-5**	2	-6***
Vocational training center	19	20	17	-1	-2	1
4-year college or university	10	11	10	-1	-1	0
Other	16	17	14	-1	-3	2
Attended Training for:						
General education	20	20	21	-0	1	-1
Occupation or specific skill	90	91	89	-1	-2	1
Attended Training Intended to:						
Prepare for new occupation	62	65	64	-3	-1	-1
Improve skills in current occupation	40	38	36	2	-2	4*
Sample Size	**812**	**773**	**819**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Characteristics are included across all a participant's reported training episodes. Means were computed using only people who participated in any training within the first 3 years of follow-up. Because these are nonrandom samples of the full groups, differences in means across approaches cannot be interpreted as the impact of one approach as compared with another.

The approach means and conditional differences are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table E.8. Impacts on Completion of Training Programs

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Completed a Training Program						
Starting within 3 years of RA	62	58	64	4**	6***	-2
Starting at least 3 years after RA	16	17	16	-1	-1	0
Earned a Certificate or Degree from a Training Program						
Starting within 3 years of RA	57	53	59	4*	6***	-2
Starting at least 3 years after RA	14	15	13	-1	-2	1
Sample Size	**1,105**	**1,081**	**1,078**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Note: People who did not participate in training are assigned values of 0 for all training-related variables.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

RA = random assignment.

Appendix Table E.9. Differences in Completion of Training Programs Among ITA Customers Who Participated in Training

	Means			Conditional Differences		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Completed a Training Program						
Starting within 3 years of RA	85	81	83	4*	2	1
Earned a Certificate or Degree from a Training Program						
Starting within 3 years of RA	78	74	77	4*	3	1
Sample Size	**812**	**773**	**819**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Means were computed using only people who participated in any training within the first 3 years of follow-up. Because these are nonrandom samples of the full groups, differences in means across approaches cannot be interpreted as the impact of one approach as compared with another.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

RA = random assignment.

Appendix Table E.10. Impacts on Program Completion, by Provider Type

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Private	31	26	30	5**	4**	1
Community College	12	14	17	-2	3*	-5***
Vocational Training Center	11	10	11	1	1	0
Four-Year College or University	6	6	6	-0	-0	0
Other	10	10	9	0	-1	1
Sample Size	**1,105**	**1,081**	**1,078**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Completion of programs is based on completion of a training program within the first 3 years of follow-up for any of the customers' reported training programs.
People with no training have a 0 for completion in training programs of all types.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table E.11. Differences in Program Completion Among ITA Customers Who Participated in Training, by Provider Type

	Means			Conditional Differences		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Training Starting Within Three Years of RA						
Private	41	36	39	5**	3	2
Community college	17	19	22	-2	3	-5***
Vocational training center	16	14	14	1	-0	1
Four-year college or university	8	9	7	-1	-1	1
Other	14	14	11	-0	-3*	3
Sample Size	**812**	**773**	**819**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Completion of program is counted across all a person's reported training episodes. Means were computed using only people who participated in any training within the first 3 years of follow-up. Because these are nonrandom samples of the full groups, differences in means across approaches cannot be interpreted as the impact of one approach as compared with another.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

RA = random assignment.

APPENDIX F

SUPPLEMENTAL TABLES ON EMPLOYMENT OUTCOMES (CHAPTER VI)

Appendix Table F.1. Impacts on Employment, by Quarter (Survey Data)

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Employed						
Quarter 1	32	31	29	1	-2	3
Quarter 2	47	45	44	2	-1	3
Quarter 3	59	58	56	1	-1	3
Quarter 4	69	66	68	2	2	0
Quarter 5	74	72	77	3	5***	-3
Quarter 6	77	76	80	1	4**	-3
Quarter 7	77	78	79	-1	1	-2
Quarter 8	78	79	80	-1	1	-2
Quarter 9	78	78	78	-0	-0	0
Quarter 10	80	78	80	2	2	-0
Quarter 11	81	78	80	3*	2	1
Quarter 12	82	79	81	4**	2	1
Quarter 13	83	81	82	3*	1	2
Quarter 14	83	82	83	1	1	-0
Quarter 15	84	82	84	2	2	0
Quarter 16	84	82	84	2	2	-0
Quarter 17	84	83	84	1	1	0
Quarter 18	84	83	84	1	1	0
Quarter 19	84	83	83	1	0	1
Quarter 20	83	83	83	1	0	1
Quarter 21	82	82	82	1	-0	1
Quarter 22	82	81	81	0	0	0
Average Employment						
Quarters 1–22	76	74	76	1	1	0
Sample Size	**1,097**	**1,080**	**1,076**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Employed is defined as having worked at least one day in the time period. Quarters are defined as 13-week intervals counting backwards from the long-term follow-up survey, and Quarter 1 is the first of these complete 13-week quarters after random assignment.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table F.2. Impacts on Employment Outcomes (Survey Data)

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Full Follow-Up Period						
Percentage of quarters employed	77	76	77	1	1	-0
Hours worked per quarter	407	395	405	12	10	2
Earlier Follow-Up Period						
Percentage of quarters employed	76	75	76	1	1	-0
Hours worked per quarter	397	386	396	11	10	1
Final Two Years of Follow-Up Period						
Labor force participant at time of followup	88	89	90	-2	0	-2
Percentage of quarters employed	80	79	80	1	1	0
Hours worked per quarter	430	418	428	13	10	2
Sample Size	**1,104**	**1,081**	**1,076**			

Source: Long-term follow-up survey.

Notes: Employed in a quarter is defined as having worked at least one day in that quarter. Earnings include totals for all jobs worked in the time period. Dollars are in 2002 dollars.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

Because the outcomes presented could not be defined for some customers, the sample size ranges are:
A1: 1,097 to 1,104
A2: 1,080 to 1,081
A3: 1,076 for all outcomes

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table F.3. Percentage of Customers Who Became Employed in an Occupation in Which They Received Training

	Percentage			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Trained in Occupation Within Three Years Since Random Assignment and Was:						
Ever Employed in Same Occupation	42	38	42	4**	4*	0
Ever Employed in Same Occupation in Early Follow-Up Period	41	37	40	4*	4*	0
Ever Employed in Same Occupation in Final Two Years of Follow-Up Period	32	27	29	5***	2	3

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Percentage of all respondents who were employed in a given two-digit SOC occupation and were trained in the same two-digit SOC occupation.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics. Sample sizes vary by row.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table F.4. Impacts on Earnings, by Quarter (Survey Data)

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Quarterly Earnings						
Quarter 1	1,406	1,304	1,219	102	-84	186
Quarter 2	2,784	2,648	2,474	136	-175	311*
Quarter 3	3,794	3,646	3,546	148	-100	247
Quarter 4	4,581	4,402	4,467	180	65	115
Quarter 5	5,231	4,951	5,256	280	306	-26
Quarter 6	5,685	5,522	5,681	163	159	4
Quarter 7	6,165	5,939	6,144	226	204	22
Quarter 8	6,473	6,243	6,312	231	70	161
Quarter 9	6,780	6,279	6,468	501**	189	312
Quarter 10	7,018	6,405	6,581	613***	176	437*
Quarter 11	7,157	6,502	6,773	656***	272	384
Quarter 12	7,344	6,653	6,923	691***	270	421*
Quarter 13	7,509	6,823	7,012	687***	189	498**
Quarter 14	7,545	6,903	7,060	642***	157	485**
Quarter 15	7,563	6,931	7,229	632***	298	333
Quarter 16	7,557	7,008	7,277	549**	269	280
Quarter 17	7,576	7,076	7,247	500**	171	329
Quarter 18	7,622	7,167	7,223	454*	56	399*
Quarter 19	7,674	7,155	7,262	519**	107	411*
Quarter 20	7,604	7,074	7,167	530**	93	437*
Quarter 21	7,525	7,070	7,170	456*	101	355
Quarter 22	7,437	6,878	7,224	559**	346	213
Average Earnings						
Quarters 1–22	6,365	5,935	6,078	430**	143	287
Sample Size	**1,097**	**1,080**	**1,076**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Quarters are defined as 13-week intervals counting backwards from the long-term follow-up survey, and Quarter 1 is the first of these complete 13-week quarters after random assignment. Dollars are in 2002 dollars.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table F.5. Impacts on Quarterly Earnings (Survey Data)

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Full Follow-Up Period						
Quarterly earnings	6,592	6,152	6,329	440**	176	263
Earlier Follow-Up Period						
Quarterly earnings	6,327	5,934	6,083	392**	149	244
Final Two Years of Follow-Up Period						
Quarterly earnings	7,186	6,665	6,918	522**	254	268
Sample Size	**1,105**	**1,081**	**1,078**			

Source: Long-term follow-up survey.

Notes: Employed in a quarter is defined as having worked at least one day in that quarter. Earnings include totals for all jobs worked in the time period. Dollars are in 2002 dollars.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

Because the outcomes presented could not be defined for some customers, the sample size ranges are:
A1: 986 to 1,105
A2: 948 to 1,081
A3: 957 to 1,078

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table F.6. Impacts on Employment, by Quarter (Administrative Data)

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Employed						
Quarter 1	46	45	43	1	-2	3**
Quarter 2	52	53	51	-2	-2	0
Quarter 3	58	58	58	0	-0	0
Quarter 4	63	64	62	-1	-2	1
Quarter 5	65	66	65	-1	-1	0
Quarter 6	67	68	67	-1	-1	0
Quarter 7	68	68	68	-0	-0	0
Quarter 8	67	67	68	0	1	-0
Quarter 9	68	67	67	1	0	1
Quarter 10	68	68	67	0	-1	2
Quarter 11	68	67	66	1	-0	1
Quarter 12	68	67	66	1	-2	3**
Quarter 13	67	66	65	1	-1	2*
Quarter 14	67	67	65	0	-2	2
Quarter 15	67	67	65	-0	-1	1
Quarter 16	67	66	65	1	-1	2*
Quarter 17	66	66	65	0	-1	1
Quarter 18	65	66	64	-0	-1	1
Quarter 19	65	64	63	1	-1	2
Quarter 20	65	64	63	1	-1	2
Quarter 21	64	63	62	1	-1	2
Quarter 22	63	62	61	1	-1	2*
Average Employment						
Quarters 1–22	64	64	63	0	-1	1
Sample Size	**2,646**	**2,647**	**2,627**			

Source: State Unemployment Insurance wage records.

Notes: Quarters are defined by the first calendar quarter after random assignment.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table F.7. Impacts on Earnings, by Quarter (Administrative Data)

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Quarterly Earnings						
Quarter 1	1,850	1,874	1,604	-24	-270***	247***
Quarter 2	2,391	2,552	2,222	-161	-330***	170
Quarter 3	2,982	3,163	2,906	-181	-257**	76
Quarter 4	3,446	3,589	3,414	-143	-175	31
Quarter 5	3,801	4,015	3,778	-214*	-237*	23
Quarter 6	4,128	4,270	4,068	-141	-202	61
Quarter 7	4,291	4,387	4,314	-97	-73	-24
Quarter 8	4,401	4,506	4,419	-105	-87	-18
Quarter 9	4,623	4,578	4,443	45	-135	180
Quarter 10	4,774	4,673	4,645	102	-27	129
Quarter 11	4,825	4,714	4,745	111	31	80
Quarter 12	5,012	4,818	4,741	194	-78	272**
Quarter 13	5,023	4,897	4,798	126	-99	224
Quarter 14	5,100	4,918	4,829	182	-89	271*
Quarter 15	5,127	5,047	4,849	80	-198	278**
Quarter 16	5,106	5,033	4,875	73	-158	231
Quarter 17	5,145	5,031	4,952	114	-79	193
Quarter 18	5,063	5,034	4,956	30	-78	108
Quarter 19	5,111	5,040	4,965	71	-75	146
Quarter 20	5,130	5,000	4,977	129	-23	152
Quarter 21	5,073	4,953	4,978	120	25	95
Quarter 22	5,037	4,940	4,912	97	-28	125
Average Earnings						
Quarters 1–22	4,429	4,411	4,290	19	-120	139
Sample Size	**2,646**	**2,647**	**2,627**			

Source: State Unemployment Insurance wage records.

Notes: Quarters are defined by the first calendar quarter after random assignment. Dollars are in 2002 dollars.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table F.8. Impacts on Employment and Earnings (Administrative Data)

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Percent of Quarters Employed						
Full Follow-Up (2002-2009)	63	63	62	0	-1	1
Earlier Follow-Up (2002-2007)	65	64	64	0	-1	1
Final Two Years of Follow-Up (2008-2009)	59	59	58	0	0	1
Average Quarterly Earnings						
Full Follow-Up (2002-2009)	4,565	4,540	4,462	25	-78	103
Earlier Follow-Up (2002-2007)	4,480	4,481	4,372	-2	-110	109
Final Two Years of Follow-Up (2008-2009)	4,818	4,713	4,734	105	21	84
Sample Size	**2,646**	**2,647**	**2,627**			

Source: State Unemployment Insurance wage records.

Notes: Quarters are defined by calendar quarters. Dollars are in 2002 dollars.

The approach means and impacts are regression adjusted. The regression predictors include: demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table F.9. Impacts on Employment Quality in Full Follow-Up Period

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Percentage of Quarters Employed in Job with the Following Characteristics						
High-Wage Job[a]	21	18	18	3**	-0	3**
Full-Time Job[b]	68	66	67	2	1	1
Stable Job[c]	75	73	75	1	1	-0
Union	5	5	6	-0	1	-1*
Percentage of Quarters Employed in Job with the Following Benefits						
Health Insurance	57	56	57	1	1	0
Paid Leave	58	57	58	1	2	-1
Retirement Benefits	52	51	51	1	1	0
Sample Size	**1,097**	**1,080**	**1,076**			

Source: Long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

[a]A high-wage job pays $20 or more an hour in 2002 dollars.

[b]A high-wage job with benefits pays $20 or more an hour in 2002 dollars and has health insurance, paid leave, or retirement benefits.

[c]A stable job is one in which the customer is employed continuously for at least six months.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table F.10. Impacts on Employment Quality in Early Follow-Up Period

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Percentage of Quarters Employed in Job with the Following Characteristics						
High-Wage Job[a]	19	17	17	2*	-0	3**
Full-Time Job[b]	67	65	66	2	1	1
Stable Job[c]	73	72	73	1	2	-0
Union	4	5	6	-0	1	-1
Percentage of Quarters Employed in Job with the Following Benefits						
Health Insurance	55	54	55	1	1	-0
Paid Leave	55	54	56	1	2	-1
Retirement Benefits	49	49	49	1	1	0
Sample Size	**1,097**	**1,080**	**1,076**			

Source: Long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

[a]A high-wage job pays $20 or more an hour in 2002 dollars.

[b]A high-wage job with benefits pays $20 or more an hour in 2002 dollars and has health insurance, paid leave, or retirement benefits.

[c]A stable job is one in which the customer is employed continuously for at least six months.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

APPENDIX G

SUPPLEMENTAL TABLES FOR BENEFIT-COST ANALYSIS (CHAPTER VIII)

Appendix Table G.1. Fringe Benefits as a Percentage of Earnings

Fringe Benefit	Total Wages and Salaries ($)	Total Cost of Benefit ($)	Fringe Benefit as % of Wages and Salaries	Civilian Participation Rate (%)	Estimated % of Wage Cost for Those Receiving Benefit
Health Benefits	20.69	2.49	12.0	55	21.8
Paid Leave[a]	20.69	2.05	9.9	74	13.3
Retirement	20.69	1.34	6.5	55	11.8
Legally Required	20.69	2.32	11.2	100	11.2

Sources: Table 1, *Employer Costs for Employee Compensation*, Department of Labor, 2010; Table 1, Table 2, Table 6, *Employee Benefits in the United States – March 2010*, Department of Labor, 2010.

Notes: Costs are based on reports for civilian workers. Fringe benefit costs reported by employers include average costs for all employees, even those not receiving benefits. The final estimate of fringe benefit value is based on the benefit value as a percent of wages and salaries over all civilian workers divided by the participation rate of civilian workers.

[a]Paid leave benefits from the *Employee Benefits Survey* are broken down by paid sick leave, paid vacation, and paid personal leave. The survey simply asked for paid time off, so the estimate for paid vacation was used since it had the highest participation rate.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table G.2. Impacts on Earnings (Survey Data)

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Earnings						
Quarter 1	1,406	1,304	1,219	102	-84	186
Quarter 2	2,784	2,648	2,474	136	-175	311*
Quarter 3	3,794	3,646	3,546	148	-100	247
Quarter 4	4,581	4,402	4,467	180	65	115
Quarter 5	5,231	4,951	5,256	280	306	-26
Quarter 6	5,685	5,522	5,681	163	159	4
Quarter 7	6,165	5,939	6,144	226	204	22
Quarter 8	6,473	6,243	6,312	231	70	161
Quarter 9	6,780	6,279	6,468	501**	189	312
Quarter 10	7,018	6,405	6,581	613***	176	437*
Quarter 11	7,157	6,502	6,773	656***	272	384
Quarter 12	7,344	6,653	6,923	691***	270	421*
Quarter 13	7,509	6,823	7,012	687***	189	498**
Quarter 14	7,545	6,903	7,060	642***	157	485**
Quarter 15	7,563	6,931	7,229	632***	298	333
Quarter 16	7,557	7,008	7,277	549**	269	280
Quarter 17	7,576	7,076	7,247	500**	171	329
Quarter 18	7,622	7,167	7,223	454*	56	399*
Quarter 19	7,674	7,155	7,262	519**	107	411*
Quarter 20	7,604	7,074	7,167	530**	93	437*
Quarter 21	7,525	7,070	7,170	456*	101	355
Quarter 22	7,437	6,878	7,224	559**	346	213
Average Quarterly Earnings in Final Year of Follow-up	6,880	6,343	6,589	537**	246	291
Sample Size	**1,097**	**1,080**	**1,076**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Quarters are defined as 13-week intervals counting backwards from the long-term follow-up survey, and Quarter 1 is the first of these complete 13-week quarters after RA. Dollars are in 2002 dollars.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table G.3. Impacts on Employee Fringe Benefit: Health Insurance Receipt (Survey Data)

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Health Insurance						
Quarter 1	18	19	16	-1	-3*	2
Quarter 2	29	29	27	-1	-2	1
Quarter 3	36	38	36	-2	-3	1
Quarter 4	44	44	45	-1	1	-1
Quarter 5	48	50	51	-2	1	-3
Quarter 6	52	53	55	-1	2	-3
Quarter 7	53	56	57	-3	0	-4*
Quarter 8	55	57	57	-2	-0	-2
Quarter 9	56	56	56	-0	0	-0
Quarter 10	59	56	58	3	2	1
Quarter 11	60	58	59	3	2	1
Quarter 12	62	58	61	4*	2	1
Quarter 13	63	60	61	4*	2	2
Quarter 14	63	61	62	2	1	1
Quarter 15	63	62	62	1	1	1
Quarter 16	64	62	63	1	1	1
Quarter 17	64	64	63	-0	-1	1
Quarter 18	65	64	64	1	1	0
Quarter 19	65	64	65	1	0	1
Quarter 20	65	64	64	1	-0	1
Quarter 21	64	63	64	1	0	1
Quarter 22	63	62	64	1	2	-0
Average Income When Received Health Insurance in Final Four Quarters of Follow-up[a]	5,816	5,387	5,519	428*	132	297
Sample Size	**1,105**	**1,081**	**1,078**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Quarters are defined as 13-week intervals counting backwards from the long-term follow-up survey, and Quarter 1 is the first of these complete 13-week quarters after RA. Dollars are in 2002 dollars.
The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

[a] This value is needed in order to properly include the value of health benefits in the final four quarters of follow-up.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table G.4. Impacts on Employee Fringe Benefit: Paid Time Off (Survey Data)

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Paid Time Off						
Quarter 1	18	19	16	-1	-3*	2
Quarter 2	27	28	26	-1	-2	1
Quarter 3	36	37	35	-1	-3	1
Quarter 4	44	43	45	1	2	-1
Quarter 5	48	49	50	-1	2	-2
Quarter 6	52	52	55	-0	3	-3
Quarter 7	53	55	57	-2	2	-4*
Quarter 8	56	56	58	-0	2	-2
Quarter 9	57	55	58	1	2	-1
Quarter 10	59	55	60	4*	4**	-0
Quarter 11	61	57	61	4*	4*	0
Quarter 12	63	58	63	5**	5**	-0
Quarter 13	63	60	64	4*	4*	-0
Quarter 14	63	62	64	1	2	-1
Quarter 15	64	63	65	1	2	-1
Quarter 16	65	64	65	1	2	-0
Quarter 17	65	65	66	-0	0	-0
Quarter 18	66	65	67	1	2	-1
Quarter 19	67	65	67	1	1	-0
Quarter 20	66	66	66	1	1	0
Quarter 21	65	65	66	1	2	-1
Quarter 22	65	64	66	1	2	-1
Average Income When Received Paid Time Off in Final Four Quarters of Follow-up[a]	5,879	5,340	5,511	538**	171	367
Sample Size	**1,105**	**1,081**	**1,078**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Quarters are defined as 13-week intervals counting backwards from the long-term follow-up survey, and Quarter 1 is the first of these complete 13-week quarters after RA. Dollars are in 2002 dollars.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

[a]This value is needed in order to properly include the value of retirement benefits in the final four quarters of follow-up.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table G.5. Impacts on Employee Fringe Benefit: Retirement Benefits (Survey Data)

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Retirement Benefits						
Quarter 1	15	18	15	-3*	-3**	0
Quarter 2	24	26	24	-2	-3	0
Quarter 3	32	33	30	-2	-3	1
Quarter 4	38	38	39	0	1	-0
Quarter 5	42	43	44	-1	0	-1
Quarter 6	46	47	47	-1	0	-2
Quarter 7	47	50	49	-3	-1	-2
Quarter 8	49	50	50	-1	-0	-1
Quarter 9	51	50	50	1	-0	1
Quarter 10	53	50	51	3	1	2
Quarter 11	54	52	53	2	1	1
Quarter 12	55	53	55	3	2	0
Quarter 13	57	54	57	3	3	0
Quarter 14	57	55	57	2	2	-0
Quarter 15	58	55	58	2	2	0
Quarter 16	59	55	58	3	3	1
Quarter 17	59	58	58	1	0	1
Quarter 18	59	58	59	2	2	0
Quarter 19	60	58	60	2	1	0
Quarter 20	60	58	59	2	1	1
Quarter 21	59	58	58	1	1	1
Quarter 22	58	57	59	2	2	-1
Average Income When Received Retirement Benefits in Final Four Quarters of Follow-up[a]	5,398	5,053	5,052	345	-2	347
Sample Size	**1,105**	**1,081**	**1,078**			

Sources: 15-month follow-up survey and long-term follow-up survey.

Notes: Quarters are defined as 13-week intervals counting backwards from the long-term follow-up survey, and Quarter 1 is the first of these complete 13-week quarters after RA. Dollars are in 2002 dollars.

The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

[a]This value is needed in order to properly include the value of retirement benefits in the final four quarters of follow-up.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table G.6. Benefits of Switching from Guided Choice for Customers, Government, and Society, in 2002 Dollars if Customers Retire at Age 65

	Structured Choice vs. Guided Choice			Maximum Choice vs. Guided Choice		
	Customers	Government	Society	Customers	Government	Society
Earnings (survey)	35,802***	0	35,802***	15,265	0	15,265
Fringe Benefits (survey)						
Health benefits	6,231*	0	6,231*	1,423	0	1,423
Paid leave	4,606**	0	4,606**	1,380	0	1,380
Retirement	2,744	0	2,744	-116	0	-116
Legally required	4,010***	0	4,010***	1,710	0	1,710
Taxes	-6,086***	6,086***	0	-2,595	2,595	0
Unemployment Insurance						
Benefits	194	-194	0	1,682	-1,682	0
Administrative costs	0	-17	-17	0	-151	-151
Public Assistance Receipt						
Food stamp benefits	-255	255	0	-624	624	0
Food stamp administrative costs	0	61	61	0	150	150
Other cash assistance benefits	-1,040	1,040	0	1,783	-1783	0
Other cash assistance administrative costs	0	104	104	0	-178	-178
Total Benefits	**46,207****	**7,335**	**53,541****	**19,907**	**-425**	**19,482**

Sources: Table VII.1, Table VII.2, Appendix Table G.1, Appendix Table G.2, Appendix Table G.3, Appendix Table G.4, and Appendix Table G.5

Note: Calculations are based on the median customer as described in the chapter and detailed in Appendix C. The following assumptions are applied: (1) discount rate is 2.5 percent; (2) retirement age is 65; (3) future unobserved impacts are the same as impacts in the final year of follow-up. Total benefits amount is based on adding impacts over different benefits, but significance levels are based on significance level of individual-level total benefits regressions as described in the chapter.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table G.7. Benefits of Switching from Guided Choice for Customers, Government, and Society, in 2002 Dollars over the Observable Follow-up Period

	Structured Choice vs. Guided Choice			Maximum Choice vs. Guided Choice		
	Customers	Government	Society	Customers	Government	Society
Earnings (survey)	11,169**	0	11,169**	3,992	0	3,992
Fringe Benefits (survey)						
Health benefits	1,946*	0	1,946*	107	0	107
Paid leave	1,322**	0	1,322**	337	0	337
Retirement	876	0	876	-107	0	-107
Legally required	1,251**	0	1,251**	447	0	447
Taxes	-1,899**	1,899**	0	-679	679	0
Unemployment Insurance						
Benefits	69	-69	0	596	-596	0
Administrative costs	0	-6	-6	0	53	53
Public Assistance Receipt						
Food stamp benefits	-90	90	0	-221	221	0
Food stamp administrative costs	0	22	22	0	53	53
Other cash assistance benefits	-369	369	0	632	-632	0
Other cash assistance administrative costs	0	37	37	0	-63	-63
Total Benefits	**14,275****	**2,341**	**16,616****	**5,103**	**-391**	**4,712**

Sources: Table VII.1, Table VII.2, Appendix Table G.1, Appendix Table G.2, Appendix Table G.3, Appendix Table G.4, and Appendix Table G.5.

Note: Calculations are based on the median customer as described in the chapter and detailed in Appendix C. The following assumptions are applied: (1) discount rate is 2.5 percent; (2) retirement age is 62; (3) future unobserved impacts do not exist beyond follow-up. Total benefits amount is based on adding impacts over different benefits, but significance levels are based on significance level of individual-level total benefits regressions as described in the chapter.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table G.8. Benefits of Switching from Guided Choice for Customers, Government, and Society, in 2002 Dollars with a Discount Rate of 10 Percent

	Structured Choice vs. Guided Choice			Maximum Choice vs. Guided Choice		
	Customers	Government	Society	Customers	Government	Society
Earnings (survey)	17,790**	0	17,790	7,183	0	7,183
Fringe Benefits (survey)						
Health benefits	3,070*	0	3,070	545	0	545
Paid leave	2,228**	0	2,228	637	0	637
Retirement	1,359	0	1,359	-102	0	-102
Legally required	1,992**	0	1,992	804	0	804
Taxes	-3,024**	3,024	0	-1,221	1,221	0
Unemployment Insurance						
Benefits	102	-102	0	887	-887	0
Administrative costs	0	-9	-9	0	-80	-80
Public Assistance Receipt						
Food stamp benefits	-134	134	0	-329	329	0
Food stamp administrative costs	0	32	32	0	79	79
Other cash assistance benefits	-548	548	0	940	-940	0
Other cash assistance administrative costs	0	55	55	0	-94	-94
Total Benefits	**22,835****	**3,682**	**26,517****	**9,344**	**-371**	**8,973**

Sources: Table VII.1, Table VII.2, Appendix Table G.1, Appendix Table G.2, Appendix Table G.3, Appendix Table G.4, and Appendix Table G.5.

Note: Calculations are based on the median customer as described in the chapter and detailed in Appendix C. The following assumptions are applied: (1) discount rate is 10 percent; (2) retirement age is 62; (3) future unobserved impacts are the same as impacts in the final year of follow-up. Total benefits amount is based on adding impacts over different benefits, but significance levels are based on significance level of individual-level total benefits regressions as described in the chapter.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

Appendix Table G.9. Benefits of Switching from Guided Choice for Customers, Government, and Society, in 2002 Dollars Using Administrative Data

	Structured Choice vs. Guided Choice			Maximum Choice vs. Guided Choice		
	Customers	Government	Society	Customers	Government	Society
Earnings (administrative)	-6,105	0	-6,105	2,386	0	2,386
Fringe Benefits (survey)						
Health benefits	-2,982	0	-2,982	45	0	45
Paid leave	-1,110	0	-1,110	587	0	587
Retirement	-765	0	-765	129	0	129
Legally required	-684	0	-684	267	0	267
Taxes	1,038	-1038	0	-406	406	0
Unemployment Insurance						
Benefits	175	-175	0	1,513	-1,513	0
Administrative costs	0	-16	-16	0	-136	-136
Public Assistance Receipt						
Food stamp benefits	-936	936	0	1,604	-1,604	0
Food stamp administrative costs	0	55	55	-	135	135
Other cash assistance benefits	-936	936	0	1,604	-1,604	0
Other cash assistance administrative costs	0	94	94	0	-160	-160
Total Benefits	**-11,598**	**85**	**-11,513**	**5,563**	**-2,311**	**3,252**

Sources: Table VII.1, Table G.7, Table VII.2, Appendix Table G.2, Appendix Table G.3, Appendix Table G.4, and Appendix Table G.5.

Note: Calculations are based on the median customer as described in the chapter and detailed in Appendix C. The following assumptions are applied: (1) discount rate is 2.5 percent; (2) retirement age is 62; (3) future unobserved impacts are the same as impacts in the final year of follow-up. Total benefits amount is based on adding impacts over different benefits, but significance levels are based on significance level of individual-level total benefits regressions as described in the chapter.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

APPENDIX H

SUPPLEMENTAL TABLES ON ANALYSIS OF IMPACTS BY SUBGROUPS AND SITES (CHAPTER IX)

Appendix Table H.1. Impacts on Primary Outcomes, by Age Category

	Age 40 or Younger at Baseline						Older than 40 at Baseline					
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Training Outcomes												
Attended Training Program	73	73	79	1	6**	-6**	73	71	74	2	4	-1
Weeks in Training Program	33	31	33	2	2	0	29	27	28	2	1	1
Completed a Training Program	62	57	66	5	9***	-4	62	59	62	3	3	0
Labor Market Outcomes During Final Two Years of Follow-Up[b]												
Percentage of Quarters Employed	76	74	75	2	1	1	83	83	84	0	1	-1
Average Quarterly Earnings	6,830	6,352	6,623	478	271	206	7,517	6,922	7,182	595*	260	335
Ever Employed in an Occupation Matching Training Program[a]	35	27	31	8**	4	4	30	27	27	3	0	3
Income and Self-Sufficiency Outcomes During Final Year of Follow-Up[c]												
Household Income	40,757	38,469	38,585	2,289	116	2,172	40,652	40,609	39,147	43	-1,462	1,504
Household Income Below the Poverty Line	14	14	16	-0	1	-1	19	19	19	0	-0	1
Received Unemployment Insurance[d]	22	18	19	4[†]	1	3	21	26	23	-4*,[†]	-2	-2
Received SNAP or Cash Assistance[d]	31	35	36	-3	2	-5*	22	21	22	1	1	0
Sample Size	**485**	**459**	**484**				**620**	**622**	**594**			

Sources: 15-month follow-up and long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics. Because the outcomes presented could not be defined for some customers, the sample sizes ranges for age 40 and under are A1: 458 to 485, A2: 425 to 459, A3: 458 to 484; and for those over 40 are A1: 552 to 620, A2: 562 to 622, A3: 530 to 594.

[a]Training outcome measures are defined for training that started within the first three years of follow-up.

[b]The final two years of follow-up are defined as the eight 13-week quarters immediately preceding the interview date of the long-term follow-up survey. The long-term follow-up survey was collected from August 6, 2009, through May 26, 2010.

[c]The final year of follow-up is defined as the 12 months immediately preceding the date of the long-term follow-up survey.

[d]Receipt of unemployment insurance, SNAP, or cash assistance is based on reports of receipt by anyone in the household.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.
† Subgroup impacts are significantly different from one another at the 0.05 level.

Appendix Table H.2. Impacts on Primary Outcomes, by Educational Attainment at Baseline

	High School Degree or Less at Baseline						More than High School Degree at Baseline					
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Training Outcomes												
Attended Training Program	73	71	77	2	6**	-4*	74	73	75	1	3	-1
Weeks in Training Program	29	27	29	2	2	-0	35	33	33	2	0	2
Completed a Training Program	61	57	65	4	8***	-4	64	60	62	4	2	2
Labor Market Outcomes During Final Two Years of Follow-Up[b]												
Percentage of Quarters Employed	78	77	79	2	2	-1	83	83	81	-0	-2	2
Average Quarterly Earnings	6,504	5,959	6,551	545**	592**	-47	8,467	7,977	7,597	491	-379	870**
Ever Employed in an Occupation Matching Training Program[a]	33	28	31	5**	3	2	30	25	26	5	0	5
Income and Self-Sufficiency Outcomes During Final Year of Follow-Up[c]												
Household Income	38,135	37,252	37,476	882	224	659	45,484	44,075	41,452	1,409	-2,622	4,032**
Household Income Below the Poverty Line	18	19	20	-1	2	-2	14	13	11	1	-2	3
Received Unemployment Insurance[d]	23	21	21	2	1	1	20	24	21	-5	-3	-2
Received SNAP or Cash Assistance[d]	31	32	33	-1	1	-2	19	18	21	0	2	-2
Sample Size	**677**	**672**	**681**				**428**	**409**	**397**			

Sources: 15-month follow-up and long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics. Because the outcomes presented could not be defined for some customers, the sample sizes ranges for those with a high school degree or less are A1: 617 to 677, A2: 628 to 672, A3: 629 to 681; and for those with more than a high school degree are A1: 393 to 428, A2: 359 to 409, A3: 359 to 397.

[a]Training outcome measures are defined for training that started within the first three years of follow-up.

[b]The final two years of follow-up are defined as the eight 13-week quarters immediately preceding the interview date of the long-term follow-up survey. The long-term follow-up survey was collected from August 6, 2009, through May 26, 2010.

[c]The final year of follow-up is defined as the 12 months immediately preceding the date of the long-term follow-up survey.

[d]Receipt of unemployment insurance, SNAP, or cash assistance is based on reports of receipt by anyone in the household.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.
† Subgroup impacts are significantly different from one another at the 0.05 level.

Appendix Table H.3. Impacts on Primary Outcomes, by Vocational Certificate Status

	Had Vocational Certificate at Baseline						Did Not Have Vocational Certificate at Baseline					
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Training Outcomes												
Attended Training Program	81	69	77	11***,†	7*	4†	71	72	76	-1†	4*	-5**,†
Weeks in Training Program	34	27	31	7**	4	3	30	29	30	1	1	-0
Completed a Training Program	66	58	63	8*	6	2	61	58	64	3	6**	-3
Labor Market Outcomes During Final Two Years of Follow-Up[b]												
Percentage of Quarters Employed	80	78	78	3	1	2	80	79	80	0	1	-0
Average Quarterly Earnings	7,684	6,914	7,254	769*	340	429	7,037	6,580	6,813	457*	234	224
Ever Employed in an Occupation Matching Training Program[a]	36	25	29	11***	4	7*	31	28	29	3	1	2
Income and Self-Sufficiency Outcomes During Final Year of Follow-Up[c]												
Household Income	41,032	41,195	38,534	-163	-2,661	2,498	40,593	39,076	39,002	1,517	-73	1,590
Household Income Below the Poverty Line	18	17	19	1	2	-1	17	17	17	-0	-0	-0
Received Unemployment Insurance[d]	23	22	24	1	1	-1	21	22	21	-1	-1	0
Received SNAP or Cash Assistance[d]	24	25	30	-0	6	-6	27	28	28	-1	0	-1
Sample Size	**248**	**274**	**263**				**857**	**807**	**815**			

Sources: 15-month follow-up and long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics. Because the outcomes presented could not be defined for some customers, the sample sizes ranges for those with vocational certification are A1: 222 to 248, A2: 251 to 274, A3: 241 to 263; and for those with no vocational certification are A1: 788 to 857, A2: 737 to 807, A3: 747 to 815.

[a]Training outcome measures are defined for training that started within the first three years of follow-up.

[b]The final two years of follow-up are defined as the eight 13-week quarters immediately preceding the interview date of the long-term follow-up survey. The long-term follow-up survey was collected from August 6, 2009, through May 26, 2010.

[c]The final year of follow-up is defined as the 12 months immediately preceding the date of the long-term follow-up survey.

[d]Receipt of unemployment insurance, SNAP, or cash assistance is based on reports of receipt by anyone in the household.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.
† Subgroup impacts are significantly different from one another at the 0.05 level.

Appendix Table H.4. Impacts on Primary Outcomes in Phoenix

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Training Outcomes[a]						
Attended Training Program	75	73	81	3	8	-5
Weeks in Training Program	30	34	28	-3	-6	2
Completed a Training Program	61	57	71	4	14*	-10
Labor Market Outcomes During Final Two Years of Follow-Up[b]						
Percentage of Quarters Employed	76	72	80	5	9	-4
Average Quarterly Earnings	6,807	5,557	6,270	1,250**	713	537
Ever Employed in an Occupation Matching Training Program[a]	38	24	30	14*	6	8
Well-Being and Self-Sufficiency Outcomes During Final Year of Follow-Up[c]						
Household Income	37,766	33,296	32,225	4,470	-1,071	5,542*
Household Income Below the Poverty Line	24	20	23	5	3	2
Received Unemployment Insurance[d]	18	17	17	2	1	1
Received Food Stamps or Cash Assistance[d]	32	31	39	1	8	-7
Sample Size	**83**	**77**	**74**			

Sources: 15-month follow-up and long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

Because the outcomes presented could not be defined for some customers, the sample sizes ranges are
A1: 74 to 83
A2: 69 to 77
A3: 67 to 74

[a]Training outcome measures are defined for training that started within the first three years of follow-up.

[b]The final two years of follow-up are defined as the eight 13-week quarters immediately preceding the interview date of the long-term follow-up survey. The long-term follow-up survey was collected from August 6, 2009, through May 26, 2010.

[c]The final year of follow-up is defined as the 12 months immediately preceding the date of the long-term follow-up survey.

[d]Receipt of unemployment insurance, food stamps, or cash assistance is based on reports of receipt by anyone in the household.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.
† Subgroup impacts are significantly different from one another at the 0.05 level.

Appendix Table H.5. Impacts on Primary Outcomes in Maricopa County

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Training Outcomes[a]						
Attended Training Program	80	79	86	1	7	-6
Weeks in Training Program	41	34	36	7	2	5
Completed a Training Program	66	65	69	1	4	-3
Labor Market Outcomes During Final Two Years of Follow-Up[b]						
Percentage of Quarters Employed	85	85	90	-0	5	-5
Average Quarterly Earnings	7,263	6,754	7,953	509	1,199*	-690
Ever Employed in an Occupation Matching Training Program[a]	36	34	44	2	10	-8
Well-Being and Self-Sufficiency Outcomes During Final Year of Follow-Up[c]						
Household Income	41,159	41,456	43,961	-297	2,505	-2,802
Household Income Below the Poverty Line	16	14	7	2	-7*	9**,†
Received Unemployment Insurance[d]	24	24	15	0	-9	9
Received Food Stamps or Cash Assistance[d]	27	27	21	-1	-7	6
Sample Size	**88**	**94**	**91**			

Sources: 15-month follow-up and long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

Because the outcomes presented could not be defined for some customers, the sample sizes ranges are
A1: 79 to 88
A2: 86 to 94
A3: 83 to 91

[a]Training outcome measures are defined for training that started within the first three years of follow-up.

[b]The final two years of follow-up are defined as the eight 13-week quarters immediately preceding the interview date of the long-term follow-up survey. The long-term follow-up survey was collected from August 6, 2009, through May 26, 2010.

[c]The final year of follow-up is defined as the 12 months immediately preceding the date of the long-term follow-up survey.

[d]Receipt of unemployment insurance, food stamps, or cash assistance is based on reports of receipt by anyone in the household.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.
† Subgroup impacts are significantly different from one another at the 0.05 level.

Appendix Table H.6. Impacts on Primary Outcomes in Bridgeport

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Training Outcomes[a]						
Attended Training Program	82	83	91	-1	8*	-9**
Weeks in Training Program	28	21	23	7*	2	5
Completed a Training Program	70	71	77	-1	6	-8
Labor Market Outcomes During Final Two Years of Follow-Up[b]						
Percentage of Quarters Employed	76	75	76	1	1	1
Average Quarterly Earnings	7,383	6,049	6,420	1,334**	371	963*
Ever Employed in an Occupation Matching Training Program[a]	33	28	33	5	5	-0
Well-Being and Self-Sufficiency Outcomes During Final Year of Follow-Up[c]						
Household Income	41,482	34,112	36,887	7,370***,†	2,775	4,595*
Household Income Below the Poverty Line	20	27	28	-7	1	-8
Received Unemployment Insurance[d]	28	24	21	5	-2	7
Received Food Stamps or Cash Assistance[d]	34	35	34	-1	-1	-0
Sample Size	**120**	**121**	**129**			

Sources: 15-month follow-up and long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

Because the outcomes presented could not be defined for some customers, the sample sizes ranges are
A1: 110 to 120
A2: 116 to 121
A3: 120 to 129

[a]Training outcome measures are defined for training that started within the first three years of follow-up.

[b]The final two years of follow-up are defined as the eight 13-week quarters immediately preceding the interview date of the long-term follow-up survey. The long-term follow-up survey was collected from August 6, 2009, through May 26, 2010.

[c]The final year of follow-up is defined as the 12 months immediately preceding the date of the long-term follow-up survey.

[d]Receipt of unemployment insurance, food stamps, or cash assistance is based on reports of receipt by anyone in the household.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.
† Subgroup impacts are significantly different from one another at the 0.05 level.

Appendix Table H.7. Impacts on Primary Outcomes in Jacksonville

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Training Outcomes[a]						
Attended Training Program	80	82	80	-2	-2	-0
Weeks in Training Program	38	43	45	-5	2	-6
Completed a Training Program	66	60	61	6	1	5
Labor Market Outcomes During Final Two Years of Follow-Up[b]						
Percentage of Quarters Employed	77	78	81	-1	3	-3
Average Quarterly Earnings	6,225	5,950	7,308	275	1,358*	-1,083
Ever Employed in an Occupation Matching Training Program[a]	34	32	35	2	3	-1
Well-Being and Self-Sufficiency Outcomes During Final Year of Follow-Up[c]						
Household Income	39,972	39,487	38,025	485	-1,463	1,947
Household Income Below the Poverty Line	21	11	8	9*	-3	12**,†
Received Unemployment Insurance[d]	11	17	14	-6	-3	-3
Received Food Stamps or Cash Assistance[d]	23	18	21	5	3	2
Sample Size	**119**	**105**	**106**			

Sources: 15-month follow-up and long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

Because the outcomes presented could not be defined for some customers, the sample sizes ranges are
A1: 113 to 119
A2: 96 to 105
A3: 96 to 106

[a]Training outcome measures are defined for training that started within the first three years of follow-up.

[b]The final two years of follow-up are defined as the eight 13-week quarters immediately preceding the interview date of the long-term follow-up survey. The long-term follow-up survey was collected from August 6, 2009, through May 26, 2010.

[c]The final year of follow-up is defined as the 12 months immediately preceding the date of the long-term follow-up survey.

[d]Receipt of unemployment insurance, food stamps, or cash assistance is based on reports of receipt by anyone in the household.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.
† Subgroup impacts are significantly different from one another at the 0.05 level.

Appendix Table H.8. Impacts on Primary Outcomes in Atlanta

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Training Outcomes[a]						
Attended Training Program	60	60	65	1	6	-5
Weeks in Training Program	24	24	26	-0	2	-2
Completed a Training Program	51	48	54	3	6	-3
Labor Market Outcomes During Final Two Years of Follow-Up[b]						
Percentage of Quarters Employed	81	80	81	1	1	1
Average Quarterly Earnings	7,299	6,481	6,773	818	291	526
Ever Employed in an Occupation Matching Training Program[a]	27	26	17	0	-9**,†	9**
Well-Being and Self-Sufficiency Outcomes During Final Year of Follow-Up[c]						
Household Income	40,427	40,420	36,890	6	-3,530	3,536
Household Income Below the Poverty Line	16	16	22	-0	7*	-7*
Received Unemployment Insurance[d]	22	16	19	5	3	2
Received Food Stamps or Cash Assistance[d]	26	25	29	1	4	-3
Sample Size	**223**	**224**	**207**			

Sources: 15-month follow-up and long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

Because the outcomes presented could not be defined for some customers, the sample sizes ranges are
A1: 207 to 223
A2: 202 to 224
A3: 193 to 207

[a]Training outcome measures are defined for training that started within the first three years of follow-up.

[b]The final two years of follow-up are defined as the eight 13-week quarters immediately preceding the interview date of the long-term follow-up survey. The long-term follow-up survey was collected from August 6, 2009, through May 26, 2010.

[c]The final year of follow-up is defined as the 12 months immediately preceding the date of the long-term follow-up survey.

[d]Receipt of unemployment insurance, food stamps, or cash assistance is based on reports of receipt by anyone in the household.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.
† Subgroup impacts are significantly different from one another at the 0.05 level.

Appendix Table H.9. Impacts on Primary Outcomes in Northeast Region

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Training Outcomes[a]						
Attended Training Program	62	65	76	-4	10	-14
Weeks in Training Program	22	22	20	-0	-2	2
Completed a Training Program	53	41	66	13	25*	-12
Labor Market Outcomes During Final Two Years of Follow-Up[b]						
Percentage of Quarters Employed	66	81	82	-15	1	-16
Average Quarterly Earnings	6,877	6,515	6,093	362	-422	784
Ever Employed in an Occupation Matching Training Program[a]	27	16	33	11	17	-6
Well-Being and Self-Sufficiency Outcomes During Final Year of Follow-Up[c]						
Household Income	39,819	39,227	33,867	591	-5,360	5,951
Household Income Below the Poverty Line	15	13	20	2	7	-5
Received Unemployment Insurance[d]	38	30	14	7	-17	24**,†
Received Food Stamps or Cash Assistance[d]	26	30	40	-3	10	-14
Sample Size	**26**	**23**	**26**			

Sources: 15-month follow-up and long-term follow-up survey

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

Because the outcomes presented could not be defined for some customers, the sample sizes ranges are
A1: 26 to 26
A2: 20 to 23
A3: 24 to 26

[a] Training outcome measures are defined for training that started within the first three years of follow-up.

[b] The final two years of follow-up are defined as the eight 13-week quarters immediately preceding the interview date of the long-term follow-up survey. The long-term follow-up survey was collected from August 6, 2009, through May 26, 2010.

[c] The final year of follow-up is defined as the 12 months immediately preceding the date of the long-term follow-up survey.

[d] Receipt of unemployment insurance, food stamps, or cash assistance is based on reports of receipt by anyone in the household.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.
† Subgroup impacts are significantly different from one another at the 0.05 level.

Appendix Table H.10. Impacts on Primary Outcomes in North Cook County

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Training Outcomes[a]						
Attended Training Program	77	71	74	6	3	3
Weeks in Training Program	32	26	32	6*	6*	0
Completed a Training Program	69	62	64	7*	2	5
Labor Market Outcomes During Final Two Years of Follow-Up[b]						
Percentage of Quarters Employed	77	80	77	-4	-4	-0
Average Quarterly Earnings	7,125	7,609	6,650	-484[†]	-959*[,†]	475
Ever Employed in an Occupation Matching Training Program[a]	34	27	26	7*	-1	8**
Well-Being and Self-Sufficiency Outcomes During Final Year of Follow-Up[c]						
Household Income	41,087	42,637	41,345	-1,550	-1,292	-258
Household Income Below the Poverty Line	16	18	14	-2	-4	2
Received Unemployment Insurance[d]	24	21	29	3	8**[,†]	-5
Received Food Stamps or Cash Assistance[d]	24	24	29	-0	5	-5
Sample Size	**256**	**265**	**254**			

Sources: 15-month follow-up and long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

Because the outcomes presented could not be defined for some customers, the sample sizes ranges are
A1: 232 to 256
A2: 241 to 265
A3: 232 to 254

[a]Training outcome measures are defined for training that started within the first three years of follow-up.

[b]The final two years of follow-up are defined as the eight 13-week quarters immediately preceding the interview date of the long-term follow-up survey. The long-term follow-up survey was collected from August 6, 2009, through May 26, 2010.

[c]The final year of follow-up is defined as the 12 months immediately preceding the date of the long-term follow-up survey.

[d]Receipt of unemployment insurance, food stamps, or cash assistance is based on reports of receipt by anyone in the household.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.
[†] Subgroup impacts are significantly different from one another at the 0.05 level.

Appendix Table H.11. Impacts on Primary Outcomes in Charlotte

	Means			Impacts		
	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice	Between A1 & A2	Between A3 & A2	Between A1 & A3
Training Outcomes[a]						
Attended Training Program	69	66	72	3	6	-3
Weeks in Training Program	31	31	30	0	-1	1
Completed a Training Program	57	52	60	5	8	-3
Labor Market Outcomes During Final Two Years of Follow-Up[b]						
Percentage of Quarters Employed	87	79	79	8**,†	0	8**,†
Average Quarterly Earnings	7,717	6,966	7,464	751	498	253
Ever Employed in an Occupation Matching Training Program[a]	29	23	29	6	7	-0
Well-Being and Self-Sufficiency Outcomes During Final Year of Follow-Up[c]						
Household Income	41,398	41,333	40,761	65	-572	638
Household Income Below the Poverty Line	13	13	16	-0	3	-4
Received Unemployment Insurance[d]	17	30	24	-13***,†	-7	-6
Received Food Stamps or Cash Assistance[d]	24	31	26	-6	-4	-2
Sample Size	**190**	**172**	**191**			

Sources: 15-month follow-up and long-term follow-up survey.

Notes: The approach means and impacts are regression adjusted. The regression predictors include demographics (age, sex, race/ethnicity), marital status, has children (yes or no), education level (associate's degree, bachelor's degree or higher), vocational certification, primary language (English or not), type of worker (dislocated or adult), and baseline employment characteristics (employed at baseline, earnings in 12 months prior to baseline). Estimates were obtained using weights to adjust for differences between respondents and nonrespondents in baseline characteristics.

Because the outcomes presented could not be defined for some customers, the sample sizes ranges are
A1: 169 to 190
A2: 157 to 172
A3: 173 to 191

[a]Training outcome measures are defined for training that started within the first three years of follow-up.

[b]The final two years of follow-up are defined as the eight 13-week quarters immediately preceding the interview date of the long-term follow-up survey. The long-term follow-up survey was collected from August 6, 2009, through May 26, 2010.

[c]The final year of follow-up is defined as the 12 months immediately preceding the date of the long-term follow-up survey.

[d]Receipt of unemployment insurance, food stamps, or cash assistance is based on reports of receipt by anyone in the household.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.
† Subgroup impacts are significantly different from one another at the 0.05 level.

APPENDIX I

DIFFERENCES IN MEASURES OF EMPLOYMENT AND EARNINGS BASED ON SURVEY AND ADMINISTRATIVE DATA

The employment rates and earnings levels based on state UI wage records are substantially lower than those based on survey responses, as discussed in Chapter VI. In addition, the impact on average quarterly earnings for Structured Choice customers relative to Guided Choice customers is much smaller in UI data than survey data. This appendix explores a set of potential explanations for discrepancies between survey- and UI-based measures. We begin by summarizing differences in survey- and UI-measures and providing a framework for assessing factors associated with the survey-UI earnings gap. We then explore factors potentially related to differences in survey- and UI-based employment rate, such as low levels of UI coverage in certain employment sectors. Next, we examine factors potentially associated with differences in survey and UI earnings levels among workers, such as reporting patterns in the components that make up the survey-based earnings measure. Finally, we summarize our findings from this analysis.

A. DISCREPANCIES IN SURVEY- AND UI-BASED EARNINGS MEASURES

There are several possible explanations for the higher reported earnings levels in the survey data than in administrative records data. First, informal and some formal jobs are not covered by the administrative records data but may be captured in the survey data. Second, some survey respondents may have over-reported their earnings and employment levels due to recall error or other reasons. Third, some employers may have inaccurately reported (or not reported) sample members' earnings to the government. Finally, the administrative records data may have missed earnings from sample members with SSNs (or other identifying information) that were incorrectly reported by employers or sample members.

To examine reasons for the reporting differences, we use available job information from the long-term follow-up interview. These survey data contain some information on jobs that sample members held during the follow-up period. However, the survey was not structured to gather sufficiently detailed information to determine whether jobs were or were not likely to have been reported to the government. Thus, our analysis is somewhat limited by data constraints. Still, it provides important insights into the reasons that earnings levels are so much higher in the survey than administrative data.

In order to compare individual-level differences in survey- and UI-based earnings, we conducted our analyses including only customers who completed the long-term follow-up interview. Our analysis focuses on employment and earnings in quarter 22 after random assignment. This quarter was selected because it is the last quarter for which long-term follow-up data is available for all these customers—it is the minimum time between random assignment and the long-term survey interview. Focusing on the most recent quarter available reduces recall error associated with survey measures.

1. Differences in Reported Employment and Earnings

Appendix Table I.1 displays summary statistics related to individual employment and earnings as reported in the survey and administrative data. These statistics are presented for the full sample and for those employed according to both data sources, separately for customers by ITA approach. Because the goal of this descriptive analysis is to examine reporting differences at the individual level, sample weights were *not* used in the analysis.

Appendix Table I.1. Survey- and UI-Based Employment and Earnings and the Distribution of Survey-UI Earnings Differences, by ITA Approach

	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice
Full Sample			
Employed based on:			
Survey Data	82	82	83
UI Wage Records	65	67	66
Both	60	63	63
Earnings based on:			
Survey Data	$7,611	$7,140	$7,457
UI Wage Records	$5,399	$5,488	$5,711
Survey-UI Earnings Difference	$2,213	$1,652	$1,746
Percentile of Survey-UI Earnings Difference Distribution			
10	-$2,589	-$2,651	-$2,635
25	-$452	-$540	-$418
50	$409	$134	$242
75	$3,881	$2,902	$3,081
90	$9,681	$7,768	$9,167
Employed in Both Data Sources			
Earnings based on:			
Survey Data	$9,275	$8,730	$8,848
UI Wage Records	$8,535	$8,440	$8,747
Survey-UI Earnings Difference	$740	$289	$102
Percentile of Survey-UI Earnings Difference Distribution			
10	-$2,801	-$3,274	-$3,175
25	-$866	-$1,050	-$960
50	$350	$88	$181
75	$1,915	$1,616	$1,510
90	$4,634	$4,503	$4,038
Sample Size	**1,097**	**1,080**	**1,076**

Source: Long-term follow-up survey and State UI wage records

Notes: All figures unweighted.

As discussed, the employment rate in quarter 22 after random assignment is substantially higher according to the survey than UI data (Table I.1). For all three ITA approaches, the employment rate is about 82 percent using the survey data, compared to about 66 percent using the UI data. Slightly more than 60 percent of customers in all three approaches were reported as employed in both data sources. About 93 percent of workers in the UI wage records were also employed according to the survey data. Thus, there is considerable overlap in employment status using the survey and UI data *among* those identified as workers in the UI wage records.

For the full sample, earnings levels are substantially higher using the survey data than the UI data on average and for most customers (Table I.1). Median differences are smaller than mean differences, because reporting differences are large and positive for a substantial fraction of sample members. (That is, the distribution of differences is skewed to the right.) However, we find that survey-based earnings per job are larger than administrative-based earnings for about 75 percent of workers. This result is similar for customers in all three ITA approaches. Thus, differences in reported earnings are common and the large mean survey-UI earnings differences are not due solely to a small number of people who reported much higher earnings in the survey.

For the sample that is reported as employed in both data sources, survey-UI earnings differences are still common but are much smaller in magnitude, particularly in the upper percentiles of the survey-UI earnings difference distribution. This pattern suggests that the omission of certain jobs from the UI wage records may be more important in explaining the overall gap between survey- and UI-based earnings than is over-reporting of earnings among those who are employed. In the next section, we develop a framework for formally assessing this hypothesis.

2. Decomposing UI and Survey Earnings Differences into Their Component Parts

Differences in quarter 22 earnings based on the survey and UI data can be decomposed into differences due to (1) employment levels and (2) earnings among those who are employed. To calculate the relative contribution of these components, we express the overall mean difference in survey-based and UI-based earnings as follows:

$$(1)\quad (\overline{E}_S - \overline{E}_{UI}) = \frac{\overline{E}_S}{\overline{W}_S}\overline{W}_S - \frac{\overline{E}_{UI}}{\overline{W}_{UI}}\overline{W}_{UI}$$

where E_S is mean earnings using the survey data, E_{UI} is mean earnings using the UI data, the W_i ($i = S,UI$) represent employment rates according to the survey and UI data. After adding and subtracting relevant terms, the gap between survey-based and UI-based earnings can be expressed as a weighted sum of the ratios in the right-hand side of equation (1) as follows:

$$(2)\quad (\overline{E}_S - \overline{E}_{UI}) = \left(\overline{W}_S - \overline{W}_{UI}\right)\frac{\overline{E}_S}{\overline{W}_S} + \left(\frac{\overline{E}_S}{\overline{W}_S} - \frac{\overline{E}_{UI}}{\overline{W}_{UI}}\right)\overline{W}_{UI}$$

Equation (2) can then be used to decompose overall mean earnings differences into its component parts. The first term represents the portion of the gap that is due to differences in employment rates. This term is most strongly related to fact that UI records do not include certain types of workers and is therefore missing some employment. The second term represents the portion of the gap that is due to differences in earnings reports for those who are employed. This term is likely related to earnings reporting error in both the survey and UI data.

For customers in all three approaches, we find that differences in employment rates contribute much more to the overall gap between survey- and UI-based earnings than do differences in earnings among the employed (Table I.2). However, differences in earnings among the employed are relatively more important in explaining the survey-UI earnings gap for Structured Choice customers. For Guided Choice and Maximum Choice customers, about 85 percent of the survey-UI earnings gap is due to differences in employment rates, with the remaining 15 percent being due to differences in earnings among the employed. For Structured Choice customers, the portion of the gap due to differences in employment rates is 70 percent, with the remaining 30 percent being related to differences in earnings among the employed.

The implication of these decomposition results is that in seeking explanations for differences in levels of earnings between the survey and UI data, we should focus primarily on factors that may lead to differences in employment rates in the two data sources, such as the fact that UI data do not cover certain employment sectors. However, in seeking explanations for the larger Structured Choice impacts on earnings in the survey data compared to the UI data, we should focus on factors that may lead to differences in earnings reports in the two data sources. We explore potential explanations for discrepancies in survey- and UI-based employment rates and earnings reports in the following two sections.

Appendix Table I.2. Decomposition of Differences in Mean Quarter 22 Earnings Estimates Based on Survey and UI Wage Records Data

	Difference In Survey- and UI-Based Mean Earnings	Difference in Survey- and UI-Based Mean Earnings Attributable to:			
		Differences in Employment Rates		Differences in Earnings Among the Employed	
		Dollars	Percentage	Dollars	Percentage
Structured Choice	$2,252	$1,583	70	$669	30
Guided Choice	$2,214	$1,850	84	$364	16
Maximum Choice	$1,755	$1,486	85	$269	15

Source: Long-term follow-up survey and State UI wage records

Notes: All figures were calculated using sample weights to adjust for the sample and survey designs.

B. EXPLANATIONS FOR EMPLOYMENT RATE DISCREPANCIES

In order to explore possible explanations for discrepancies in survey- and administrative-based employment rates, we compared the characteristics of employment reported in *both* the survey and UI data with the characteristics of employment reported in the survey data *only*. We expect that the survey-only employment was less likely to have been covered by UI than employment reported in both data sources. We examined three types of characteristics likely to be associated with UI coverage: (1) inter-state mobility, (2) employment in job types with low UI coverage, such as self-employment and employment in certain occupations, and (3) employment in jobs with characteristics associated with formal employment, such as full-time status and provision of fringe benefits.

Throughout this analysis, employment characteristics were obtained from the survey data, and pertain to employment during quarter 22 after random assignment. Because the UI wage records provide little information about job characteristics (and in some cases provide no employer-level data), it is not possible to "match" specific jobs from the survey data to employment in the UI wage data. Therefore, employment characteristics apply to any employment held during quarter 22 rather than to a particular job.

As a complement to the analysis of employment characteristics by survey-only employment status, we also compare the survey- and UI-based employment rates for customers with each employment characteristic examined above. Groups with higher UI coverage should have survey- and UI-based employment rates that are more similar, while those with lower UI coverage should have employment rates that are more different. We use these rates in combination with the decomposition framework developed in the previous section to simulate the change in the survey-UI earnings gap that would be expected if the UI records for low UI coverage groups more closely matched their survey reports. Specifically, we simulated the change in the gap that would result if the UI-to-survey employment rate ratio for the low coverage group was the same as the survey-to-UI employment rate ratio for the high coverage group. This simulation helps us translate the observed differences in survey-UI employment rate agreement for customers with different employment characteristics into an estimated contribution toward explaining the discrepancy in UI-based and survey-based earnings.

As an example of this simulation, consider customers who moved to a different state at some point during the follow-up period. These customers represent a low UI coverage group since out of state wages are not included in state UI records. Therefore, we should find that

$$(3)\quad \frac{\overline{W}_{UI}^{Low}}{\overline{W}_{S}^{Low}} < \frac{\overline{W}_{UI}^{High}}{\overline{W}_{S}^{High}}$$

where $\overline{W}_{i}^{High}$ ($i = S,UI$) represents employment rates according to the survey and UI data for the high coverage group (in this example, those who remained in the same state) and $\overline{W}_{UI}^{Low}$ represents analogous employment rates for the low coverage group UI (in this example, those who did not remain in the same state). We simulate the UI-based employment rate of the low coverage group by assuming that the UI coverage of survey-based employment for

the low coverage group is as the same as that for the high UI coverage group. This is done by multiplying the survey-based employment rate of the low coverage group by the UI-to-survey employment ratio for the high coverage group:

$$(4)\ \overline{W}_{UI}^{Low,Sim} = \overline{W}_{S}^{Low} * \frac{\overline{W}_{UI}^{High}}{\overline{W}_{S}^{High}}$$

Next, we estimate a simulated overall employment rate based on the actual employment rate of the high coverage group and the simulated employment rate of the low coverage group:

$$(5)\ \overline{W}_{UI}^{Overall,Sim} = p^{Low} * \overline{W}_{UI}^{Low,Sim} + p^{High} * \overline{W}_{UI}^{High}$$

where p^{High} is the proportion of the sample in the high coverage group and p^{Low} is the proportion in the low coverage group. Finally, we combine equations (5) and (2) to simulate the change in the survey-UI earnings gap that would be associated with higher simulated UI coverage rates for the low coverage group (while assuming no additional change due to differences across sources in earnings among the employed):

$$(6)\left(\overline{W}_{UI} - \overline{W}_{UI}^{Overall,Sim}\right)\frac{\overline{E}_{S}}{\overline{W}_{S}}$$

For context, based on this formula and the values in the data, the gap between UI- and survey-based mean earnings will decrease by about $90 (or 4 percent) for every percentage point that the UI-based employment rate increases. Thus, the formula tells us that relatively small changes in UI-based employment rates lead to relatively large changes in the gap between UI- and survey-based mean earnings. This relationship is consistent with expectations since the omission of each job from UI wage records may represent the omission of a substantial portion of an individual's earnings.

Inter-state mobility. State UI wage records exclude earnings from customers' out-of-state jobs, as well as earnings from customers who moved to a different state at some point during the follow-up period. Therefore, it is much more likely that the employment of customers who remain in the same state throughout the study will be represented in the UI data. Indeed, we find that nearly all customers with quarter 22 employment reported both data sources were located in the same state at baseline and in the two follow-up interviews (Table I.2). The rate of same state location is significantly lower among those with survey-only employment, at just under 75 percent. This pattern is similar for customers in all three approaches.

Inter-state mobility is fairly uncommon in our sample. Across the three ITA approaches, about 9 percent of survey respondents are located in a different state at either of the two follow-up interviews (Table I.3). This is almost certainly an underestimate of inter-state mobility in the sample since we do not know if customers were located in different states between follow-up interviews. In addition, this figure does not account for employment in out-of-state jobs held while living in the original state.

Appendix Table I.3. Characteristics of Customers with Reported Quarter 22 Survey-Based Employment, by Agreement of Survey- and Administrative-Based Employment Status and ITA Approach

	A1: Structured Choice		A2: Guided Choice		A3: Maximum Choice	
	Employed in Both Survey and UI Data	Employed in Survey Data Only	Employed in Both Survey and UI Data	Employed in Survey Data Only	Employed in Both Survey and UI Data	Employed in Survey Data Only
Inter-State Mobility						
Same State at Baseline and Both Follow-Ups	98***	72	98***	70	99***	74
Job Types with Low UI Coverage						
Self-Employed or Employed in Low UI Coverage Occupation	3***	14	3***	11	3***	16
Job Characteristics Associated with Formal Employment						
Full-Time Job	92**	87	89	85	91*	86
Job Offering Hourly Wage of at Least $20	29**	36	31	25	25	31
Job Offering:						
Health Insurance	84***	62	81***	62	85***	62
Paid Leave	86***	65	83***	63	87***	65
Retirement Benefits	78***	55	74***	57	79***	55
Sample Size	**655**	**240**	**682**	**202**	**677**	**213**

Source: Long-term follow-up survey

Notes: All figures unweighted.

* / ** / *** Both-data-source estimate significantly different from survey-only estimate at the 0.10 / 0.05 / 0.01 level.

Despite its infrequency and underestimation, inter-state mobility has very important implications for the discrepancy in survey- and administrative-based employment and earnings reports. This is because there is a very large difference in the survey- and UI-based employment rates for those who moved away from their original state. Across all three approaches, this group of customers has a survey-based employment rate of about 80 percent and a UI-based employment rate of only about 16 percent (Table I.3). By contrast, those who remain in the same state have a survey-based employment rate of about 80 percent and a UI-based employment rate of about 70 percent. This pattern suggests that a large majority of the employment of those who move to a different state is not included in the UI records. The employment simulation analysis indicates that if the UI coverage of employment for customers who moved to a different state were the same as those who remained in the same state, the UI-based employment rate would increase from about 66 percent to about 70 percent, translating to more than a 20 percent decline in the gap between survey-based and UI-based average earnings (Table I.3). In other words, one-fifth of the gap between survey-based and UI-based average earnings can be explained by the omission of out-of-state wages from UI wage records.

Employment in job types with low UI coverage. UI wage records do not cover workers in some formal jobs. These workers include self-employed people, federal workers, military staff, agricultural labor (except workers on large farms), and domestic service workers.[13] We anticipate that survey-only employment is more likely to be in these "low-coverage" sectors. Therefore, we examined the prevalence of self-employment and employment in military or agricultural work among those with employment in both data sources and those with survey-only employment. Workers in low coverage sectors were identified using survey information on reported job occupations (which were open-ended responses coded into three-digit SOC codes). The survey did not collect information on type of employer, so we cannot identify other categories of workers unlikely to be covered by UI, such as federal workers. Therefore, our estimates of employment in low coverage sectors likely understate its true prevalence.

About five percent of workers across ITA approaches reported in the survey that they worked in these low coverage sectors during quarter 22. About four percent were self-employed and less than one percent worked in the military or in agricultural occupations. We expect that some sample members in these low-coverage jobs were actually covered by the UI program. UI wage records cover about 94 percent of workers nationally (U.S. General Accounting Office 2002), but U.S. workers in the low coverage sectors described above comprise more than 6 percent of all U.S. workers.[14] Thus, some of these low coverage U.S. workers must have actually been covered by the UI program. For example, some farmers and domestic workers are covered by the UI program, although it is not possible to

[13] Federal workers and military staff are eligible to receive UI benefits. Their earnings are not reported to state UI agencies, however, and so are not in the UI wage records.

[14] In 1999, 2.1 percent of all workers nationally reported working for the federal government, 7 percent were self-employed, 3.5 percent worked in agricultural-related occupations, and 1 percent worked in private household occupations (Statistical Abstract of the United States 2000).

determine from published statistics (or our survey data) the number of such workers. Furthermore, there is often ambiguity about reported self-employment status.

Consistent with our expectations, we find that employment in low coverage sectors is much less common among customers with employment in both data sources than among those with survey-only employment (Table I.3). Across all three approaches, three percent of those with employment in both data sources were employed in "low coverage" sectors, compared to about 14 percent of those with survey-only employment.

Despite the confirmation of our expectations about the relative prevalence of employment in low coverage sectors among customers with survey-only employment, the contribution of this factor to the overall discrepancy between survey- and UI-record-based employment measures is smaller than that of inter-state mobility. This is partially because employment in low coverage sectors is less common than inter-state mobility. In addition, the difference between the survey- and UI-based employment rates for those in low coverage sectors is large, but not as large as the analogous difference for those who moved to a different state. Across all three approaches, customers employed in low coverage sectors had a survey-based employment rate of 100 percent and a UI-based employment rate of only about 40 percent.

The employment simulation analysis indicates that if the UI coverage of employment for customers in low-coverage sectors were the same that of other customers, the UI-based employment rate would increase from about 66 percent to about 68 percent, translating to about a 10 percent decline in the gap between survey-based and UI-based average earnings (Table I.4). Thus, one-tenth of the gap between survey-based and UI-based average earnings can be explained by the omission of employment in low coverage sector from UI wage records. This estimate would likely be higher if we were better able to identify which customers were employed in additional low coverage sectors, such as federal employment.

Employment in jobs with characteristics associated with formal employment. Another possible explanation for the lower employment levels in the UI data is that earnings from informal (casual or cash-only) jobs are covered in the survey data but not in the UI data. We expect that the survey-only jobs were more likely to have been informal jobs than those reported in both data sources. Thus, we anticipate that the survey-only workers had less employment in (1) full-time jobs, (2) high wage jobs, and (3) jobs offering fringe benefits.

The hypotheses related to full-time and high wage jobs are not supported by the data (Table I.3). Employment in full-time jobs is similar for customers with employment in both surveys and for those in the survey-only group. Counter to expectations, employment in high-wage jobs is actually lower for Structured Choice and Maximum Choice customers with employment in both surveys than for their survey-only group counterparts, although this difference is only statistically significant for Structured Choice customers.

The hypothesis related to employment in jobs offering fringe benefits is supported by the data (Table I.3). Customers employed in both data sources were significantly more likely to have each of the three types of fringe benefits examined than customers with survey-only employment. These differences are significant at the one-percent level for all three benefit types for all three ITA approaches.

Appendix Table I.4. Simulation of Change in Gap Between UI- and Survey-Based Earnings Based on Alternative UI-based Employment Rates, by Customer Characteristic and ITA Approach

	A1: Structured Choice						A2: Guided Choice						A3: Maximum Choice					
					Simulated:						Simulated:						Simulated	
	%. With Char.	W_S	W_{UI}	$\frac{W_s}{W_{UI}}$	W_{UI}	% Change in Gap	%. With Char.	W_S	W_{UI}	$\frac{W_s}{W_{UI}}$	W_{UI}	% Change in Gap	%. With Char.	W_S	W_{UI}	$\frac{W_s}{W_{UI}}$	W_{UI}	% Change in Gap
Inter-State Mobility																		
Same State at Baseline and Both Follow-Ups																		
Yes	91	82	70	85	--	--	92	82	71	87	--	--	92	83	71	85	--	--
No	9	82	15	18	70	--	8	82	17	20	71	--	8	78	13	17	66	--
Either	100	82	65	79	70	-21	100	82	67	81	71	-24	100	83	66	80	70	-22
Job Types with Low UI Coverage																		
Self-Employed or Employed in Low UI Coverage Occupation																		
Yes	5	100	39	39	82	--	4	100	51	51	83	--	5	100	37	37	83	--
No	95	81	66	82	--	--	96	81	67	83	--	--	95	82	68	83	--	--
Either	100	82	65	79	67	-11	100	82	67	81	68	-7	100	83	66	80	69	-12

	A1: Structured Choice						A2: Guided Choice						A3: Maximum Choice					
					Simulated:						Simulated:						Simulated	
	%. With Char.	W_S	W_{UI}	$\frac{W_s}{W_{UI}}$	W_{UI}	% Change in Gap	%. With Char.	W_S	W_{UI}	$\frac{W_s}{W_{UI}}$	W_{UI}	% Change in Gap	%. With Char.	W_S	W_{UI}	$\frac{W_s}{W_{UI}}$	W_{UI}	% Change in Gap
Job Characteristics Associated with Formal Employment																		
Employed in Job Offering Health Insurance																		
Yes	64	100	79	79	--	--	63	100	81	81	--	--	66	100	81	81	--	--
No	36	49	41	83	38	--	37	51	41	81	41	--	34	50	38	76	40	--
Either	100	82	65	79	64	1	100	82	67	81	67	-1	100	83	66	80	67	-4

Source: Long-term follow-up survey and State UI wage records

Notes: For the group with expected lower UI coverage, simulated UI-based employment rate is the product of the observed survey-based employment rate for the low coverage group and the UI-to-survey employment rate ratio for the high group. The overall simulated UI-based employment rate is a weighted average of the observed UI-based employment rate for the high UI coverage group and the simulated UI-based employment rate for the low UI coverage group. The simulated change in the survey-UI earnings gap is based on the simulated overall UI-based employment rate.

Although these results provide some evidence that the survey-only jobs were more likely than the jobs reported in both data sources to be informal jobs, the contribution of informal employment to the overall survey-administrative employment rate discrepancy is modest. This is because the difference between the survey- and UI-based employment rates is similar for customers employed in jobs with characteristics associated with formal employment and those who are not. As a result, simulated UI-based employment rates are similar to observed UI-based employment rates, leading to simulated UI-survey earnings gap that is similar to the observed UI-survey earnings gap. Table I.4 provides an example of this pattern based on simulation results related to employment in a job offering health insurance. Among those who were not employed in a job offering health insurance benefits during quarter 22, UI-based employment rates were about 40 percent, or four-fifths of the survey-based employment rate of about 50 percent. The ratio of survey-based employment to UI-based employment was very similar among those who were employed in a job offering health insurance during quarter 22. As a result, the simulated UI-based employment rate is nearly identical to the observed UI-based employment rate. Therefore, little to none of the gap between survey-based and UI-based average earnings can be explained by the omission of employment in that does not offer health insurance benefits and is thus more likely to be informal work.

C. EXPLANATIONS FOR EARNINGS DISCREPANCIES

As shown in our decomposition framework, overall mean earnings differences according to the survey and UI data are due not only to differences in quarterly employment rates, but also to differences in earnings reports for those who are working. In this section, we examine which components of the survey earnings measure—weeks worked, hours worked, and hourly wages—are associated with larger survey-to-UI earnings differences and how this may be related to overreporting in the survey. Next, we examine whether the survey-to-UI earnings differences vary according to job quality measures.

1. Survey-to-UI Earnings Ratios by Components of the Survey Earnings Measure

The income that a worker earns in a job over a given period is the product of (1) the number of weeks worked on the job during the period, (2) the usual hours per week worked, and (3) the hourly wage rate. Consequently, differences in worker earnings using the survey and UI data can be attributed to survey-to-UI differences in each of these three components. A critical analysis objective is to ascertain which of these components is most important in explaining the large gap in mean earnings for workers as measured by the two data sources.

Ideally, we would like to compare differences in each of the three earnings components as reported by sample members and their employers. This is not possible, however, because the UI wage records do not contain the components of earnings. Instead, we examined the association between each of the earnings components—as measured by the survey—and the *ratio* of average survey-to-UI earnings. Thus, we assessed the extent to which the survey-to-UI earnings ratios vary by the number of weeks worked, the number of hours per week worked, and the hourly wage rate as measured by the survey. These results provide indirect evidence as to the earnings components that matter most in explaining the large gap in earnings using the survey and UI data.

The sample for this analysis consists of those who were classified as workers according to *both* data sources. In this sample, the ratio of survey-to-UI mean earnings is 1.48 for Structured Choice customers, 1.44 for Guided Choice customers, and 1.43 for Maximum Choice customers (Table I.5). Stated differently, mean quarter 22 earnings for workers are between 43 and 48 percent higher according to the survey than the UI data for customers in each ITA approach. Although the survey-to-UI earnings ratio for Structured Choice customers is not statistically different than the ratios for customers in the other two approaches, the fact that the Structured Choice ratio is higher than the other ratios contributes to the larger earnings impact using the survey than UI data.

To assess the extent to which the survey-to-UI earnings ratios vary by the number of weeks worked, the number of hours per week worked, and the hourly wage rate, we estimated a regression model in which the dependent variable is the survey-to-UI earnings ratio and the independent variables are the components of earnings. We hypothesized that the relationship between survey-to-UI earnings ratios and reported hours worked might be different at different points in the distribution of hours worked. Therefore, we estimated the regression equation using a set of categorical variables indicating whether the worker reported (1) less than 30 hours of work, (2) at least 30 hours of work but less than 40, or (3) at least 50 hours of work (at least 40 hours of work but less than 50 was the omitted category).[15]

This regression approach allows us to assess the relationship between survey-to-UI earnings ratios and the components of earnings while adjusting for the correlation among these measures. Our findings are similar when examining the components of earnings individually.

Our main finding is that the reported hours worked per week has a strong association with higher earnings reported in the survey than UI data, but that the other two components of the survey earning measure do not. In particular, we find that both workers with high reports of hours worked and those with low reports of hours worked have significantly higher survey-to-UI earnings ratios than workers reporting at least 40 but less than 50 hours of work (Table I.6). The implications of these two findings are discussed in turn below.

a. Survey-to-UI earnings ratios for workers with low reported hours of work

For all three approaches, workers with less than 30 hours of work had significantly larger survey-to-UI earnings ratios than workers with between 40 and 50 hours of work. Controlling for reported hourly wages and weeks worked, Structured Choice workers in the low hours category had ratios that were 0.80 higher than their counterparts in the at least 40 but less than 50 hours group, a difference that is statistically significant at the one percent level. In other words, the low hours group had survey-based earnings that were 80

[15] We also estimated the model using a continuous measure of hours worked per week. In this specification, the coefficient on hours worked per week is positive and statistically significant at the one percent level. However, this finding masks important variation in the association of hours worked with survey-to-UI ratio. We also investigated whether there were non-linear effects for the other components of earnings. We found no evidence of a relationship between the survey-to-UI earnings ratio and hourly wage or weeks worked under these alternative specifications.

Appendix Table I.5 Simulation Results from Reducing Hours Worked Per Week in the Survey Data, by ITA Approach

	A1: Structured Choice		A2: Guided Choice		A3: Maximum Choice	
	Mean Hours Worked per Week, Survey Data	Ratio of Survey-to-UI Earnings	Mean Hours Worked per Week, Survey Data	Ratio of Survey-to-UI Earnings	Mean Hours Worked per Week, Survey Data	Ratio of Survey-to-UI Earnings
Cap on Hours Worked per Week (Hours)						
99 (benchmark)	43.5	1.48	42.5	1.44	43.3	1.43
70	42.8	1.47	41.9	1.42	42.8	1.41
60	42.2	1.45	41.2	1.40	42.0	1.39
50	40.7	1.41	39.8	1.36	40.5	1.35
Percentage Reduction in Hours Worked per Week						
10	39.1	1.36	38.2	1.31	38.9	1.31
15	36.9	1.29	36.1	1.24	36.8	1.25
25	32.6	1.16	31.9	1.12	32.5	1.12
35	28.3	1.02	27.6	0.99	28.1	0.99
Sample Size	**655**		**682**		**677**	

Source: Long-term follow-up survey

Notes: The sample includes only customers with reported employment in both the survey and UI wage records.

Appendix Table I.6. Marginal Effects from Regression Model of the Ratio of Survey-to-UI Earnings in Quarter 22 on Earnings Measure Inputs, by ITA Approach

	A1: Structured Choice	A2: Guided Choice	A3: Maximum Choice
Hourly Wage	-0.001	0.009	0.004
Hours Worked Per Week			
Less than 30	0.801***	0.400*	0.410*
At least 30, Less than 40	0.068	0.524***	0.160
At least 40, Less than 50	--	--	--
At least 50	0.817***	0.572***	0.737***
Number of Weeks Worked	0.043	0.022	0.013
Sample Size	**655**	**682**	**677**

Source: Long-term follow-up survey and State UI wage records

Notes: The sample for this analysis includes only customers with reported employment in both the survey and UI wage records. All estimates derived from a single OLS regression model. Marginal effects for Structured Choice workers are simply the coefficient estimates from this model. Marginal effects for the other two groups are calculated as the sum of the base coefficient estimates and coefficients on approach-specific interaction terms.

* / ** / *** Estimate significantly different from zero at the 0.10 / 0.05 / 0.01 level.

percentage points higher than their UI-based earnings when compared to the omitted group. The adjusted difference between the high hours reported category and the omitted category is also large for Guided Choice and Maximum Choice customers (0.400 and 0.410 respectively), although these differences are smaller than the Structured Choice difference and are only marginally significant. The large survey-to-UI earnings ratios for low hours workers in all three approaches are consistent with the higher levels of survey earnings than UI earnings. That the ratio for the low hours reported group is larger for Structured Choice workers than other workers (though not significantly so) contributes toward the larger Structured Choice earnings impacts in the survey data than the UI data.

One possible explanation for the finding that workers with low hours of work have large differences in survey- and UI-based earnings is that these workers over-reported hours worked in their survey responses. This could be because they could not accurately recall their hours and wages if the work hours were irregular or the employment was informal. If the work was irregular and informal, it is also possible that employers under-reported earnings for these workers, which would also contribute to a larger survey-to-UI earnings ratio.

b. Survey-to-UI earnings ratios for workers with high reported hours of work

The pattern of survey-to-UI ratios for workers with high reported hours of work is similar to the pattern described for workers with low reported hours of work. For all three approaches, workers with at least 50 hours of work had substantially larger survey-to-UI earnings ratios than workers with between 40 and 50 reported hours of work. Controlling

for reported hourly wages and weeks worked, Structured Choice workers in the high hours reported category had ratios that were 0.81 higher than their counterparts in the at least 40 but less than 50 hours reported group, a difference that is statistically significant at the one percent level. The adjusted difference between the high hours reported category and the omitted category is also very large and statistically significant for Guided Choice and Maximum Choice customers (0.572 and 0.737 respectively). The large survey-to-UI earnings ratios for high hours workers across all three approaches is consistent with the higher levels of survey earnings than UI earnings. That the ratio for the high hours reported group is larger for Structured Choice workers than Guided Choice workers (though not significantly so) contributes toward the larger Structured Choice earnings impacts in the survey data than the UI data.

One possible explanation for this finding is that some customers are over-reporting their hours worked on the survey. This could happen if the survey questions requesting information on hours worked were unclear or misleading. However, we do not believe that this was the case. For each job, the survey asked each worker the following simple question: (1) "How many hours did you usually work in an average week?" This data item was rarely missing, and there was no evidence that survey respondents had trouble responding to these questions.

Another possible reason for over-reporting hours could be that sample members reported high hours worked in the survey because of recall error. However, recall error would also affect the hourly wage variables and other job-related variables. Furthermore, it is unclear why recall error would systematically lead to overreporting of hours worked.

Still another possibility is that workers reported the number of hours that their employers *advertised* they would work rather than their actual hours. For example, some workers may have been hired as full-time workers but may have only worked part-time when demand for their services was low (for example, in "off-seasons" in retail trade occupations). Similarly, some workers may have actually worked less hours than they were supposed to have worked due to child care issues, transportation problems, or other reasons, but reported the hours they were supposed to have worked.

Of course, it is also possible that the survey data are accurate and that employers did not accurately report earnings from employees' overtime or other hours to the government.

To examine further the extent to which the hours worked component accounts for the gap in earnings per job using the survey and UI data, we simulated the effects of reducing hours worked on survey-based earnings levels, and hence, on the survey-to-UI earnings per job ratios. The simulations were conducted by (1) lowering the cap on hours per week worked from 99 hours to 70, 60, and 50 hours, respectively; and (2) reducing hours worked for all workers by 10, 15, 25 and 35 percent, respectively.

The simulation results show that reducing mean hours worked leads to reductions in the survey-to-UI earnings ratios, although earnings levels are still substantially higher according to the survey than UI data (Table I.6). For example, if hours are reduced by 10 percent for all workers (which assumes that workers overreported earnings by 10 percent in the survey), mean hours worked decrease from about 43 hours to about 38 hours for customers in all three approaches, while the survey-to-UI ratio decreases from about 1.45 to between 1.31

and 1.36. The survey-to-UI ratios reduce to 1.0 for workers in all approaches if hours for all workers were reduced by 35 percent. In this case the mean hours worked per week becomes about 28 hours. We believe that it is unrealistic to assume that hours worked were overreported to this extent and that mean hours worked were this low.

We also simulated reducing the cap on hours worked from 99 to various lower levels. These changes reduce the survey-to-UI earnings ratios, but not substantially enough to remove the survey-UI earnings gap (Table I.6). For example, capping hours at 50 hours per week, which affects about one-third of customers, reduces the earnings ratio from 1.48 to 1.41 for Structured Choice customers, from 1.44 to 1.36 for Guided Choice customers, and from 1.43 to 1.35 for Maximum Choice customers.

In sum, the apparent overreporting of hours worked in the survey data provides a partial explanation for the higher earnings per job levels in the survey than UI data. However, based on our simulations, reported hours would need to be reduced by about a third to close the survey-to-UI earnings gap completely. We believe that it is unlikely that the survey-to-UI differences in reported hours are that large. Thus, residual factors (including discrepancies in reported hourly wages and weeks worked) also account for some of the survey-to-UI earnings differences.

2. Survey-to-UI Earnings Ratios by Job Characteristics

We hypothesize that earnings differences using the survey and UI data would be smaller for sample members who held higher quality jobs than for those who held lower quality ones. Those who held high quality jobs were probably more likely to have worked regular hours than their counterparts and thus may have more accurately recalled their usual hours worked, job start and end dates, and hourly wages. Furthermore, employers may have been more likely to report earnings for workers who held high quality jobs than for those who held irregular, informal ones.

To test this hypothesis, examined survey-to-UI ratios for groups of worker defined based on whether they were employed in (1) a full-time job, (2) a job offering hourly wages of at least $20, and (3) a job offering health insurance, paid leave, or retirement benefits. We expected the ratios to be smaller for workers in full-time jobs, high wage jobs, and jobs offering fringe benefits.

The results by full-time and fringe benefit status strongly support our hypothesis that reporting differences are smaller for those in higher quality jobs than lower quality ones, but the results for high-wage employment do not (Table I.7). Across all three approaches, survey-to-UI earnings ratios are significantly smaller for workers in full-time jobs than for other workers, with ratios ranging from 1.38 to 1.44 for full-time workers and from 1.88 to 1.97 for other workers. This suggests either that part-time workers could not accurately recall their hours and wages (perhaps because work hours were irregular) or that their employers did not accurately report their earnings.

Appendix Table I.7. Ratio of Survey-to-UI Earnings in Quarter 22, by Job Characteristic and ITA Approach

	A1: Structured Choice		A2: Guided Choice		A3: Maximum Choice	
	Percent with Characteristic	Ratio of Survey-to-UI Earnings	Percent with Characteristic	Ratio of Survey-to-UI Mean Earnings	Percent with Characteristic	Ratio of Survey-to-UI Mean Earnings
Overall	100	1.48	100	1.44	100	1.43
Full-Time Job		***		***		***
No	8	1.97	11	1.61	9	1.88
Yes	92	1.44	88	1.41	91	1.38
Job Offering Hourly Wage of at Least $20						*
No	71	1.48	75	1.43	75	1.37
Yes	29	1.51	25	1.42	25	1.62
Job Offering Fringe Benefits		***		***		***
No	10	2.77	11	1.96	9	2.07
Yes	90	1.34	89	1.37	91	1.36
Sample Size	**655**		**682**		**677**	

Source: Long-term follow-up survey and State UI wage records

Notes: The sample includes only customers with reported employment in both the survey and UI wage records

* / ** / *** Estimate for customers with job characteristic significantly different from estimate for those without characteristic at the 0.10 / 0.05 / 0.01 level.

The pattern for workers in jobs offering fringe benefits is similar to the one for full-time workers. Across all three approaches, we find significantly smaller survey-to-UI earnings ratios for workers in jobs offering benefits than for workers in other jobs. This difference is the largest for Structured Choice workers, for whom we estimate a survey-to-UI earnings ratio of 1.34 for those in full-time jobs and 2.77 for those who are not, although the differences are also very large for Guided Choice workers (1.37 versus 1.96) and Maximum Choice workers (1.36 versus 2.07).

Counter to our expectations, we find little evidence that there are smaller survey-to-UI differences in earnings for workers in high-wage jobs (Table I.7). In the Structured Choice and Guided Choice approaches, workers employed in high-wage jobs had similar survey-to-UI earnings ratios to those employed in low-wage jobs. Maximum Choice workers in high wage jobs actually had higher survey-to-UI earnings ratios than low-wage workers, although the difference is only marginally significant.

D. SUMMARY OF FINDINGS

We have explored potential reasons that employment rates and earnings for workers in quarter 22 are higher using the survey than the UI data. Because of data limitations, our analysis could not fully identify all relevant factors explaining these employment and earnings differences, especially for the employment differences. However, we were able to identify some partial explanations and to discard others. Our main findings can be summarized as follows:

- ***Differences in employment rates contribute much more to the overall gap between survey- and UI-based earnings than do differences in earnings among the employed.*** For Guided Choice and Maximum Choice customers, about 85 percent of the survey-UI earnings gap is due to differences in employment rates, with the remaining 15 percent due to differences in earnings among the employed. For Structured Choice customers, the portion of the gap due to employment rates is 70 percent, with the remaining 30 percent related to earnings among the employed.

- ***Inter-state mobility has very important implications for the discrepancy in survey- and administrative-based employment and earnings reports.*** Simulation analysis suggests that one-fifth of the gap between survey-based and UI-based average earnings can be explained by the omission of out-of-state wages from UI wage records. This estimate may understate the true effect of out-of-state employment on the gap since we cannot identify which customers moved to a different state between surveys nor those who remained in the same state but worked in an out-of-state job.

- ***Employment in sectors with low UI coverage, such as self-employment and agricultural or military work, also makes an important contribution to the survey-UI earnings gap.*** Simulation analysis suggests that one-tenth of the gap between survey-based and UI-based average earnings can be explained by the omission of employment in low coverage sector from UI wage records. This estimate would likely be higher if we were better able to identify which customers were employed in additional low coverage sectors, such as federal employment.

- ***The contribution of informal employment to the overall survey-administrative employment rate discrepancy is modest.*** Little to none of the gap between survey-based and UI-based average earnings can be explained by the omission of employment without ***characteristics*** likely to be associated with formal work. This finding may be the result of our limited ability to identify informal employment.

- ***The reported hours worked per week has a strong association with higher earnings reported in the survey than UI data, but that the other two components of the survey earning measure—hourly wage and weeks worked—do not.*** Both workers with high reports of hours worked and those with low reports of hours worked have significantly higher survey-to-UI earnings ratios than workers reporting at least 40 but less than 50 hours of work. This finding may indicate that there is over-reporting of hours ***worked*** in the survey. However, simulation analysis indicates that survey reports of hours worked would have to be

45 percent lower to reduce the survey-to-UI earnings ratio to 1. We believe that it is unlikely that the survey-to-UI differences in reported hours are that large and that residual factors (including discrepancies in reported hourly wages and weeks worked) also account for some of the survey-to-UI earnings differences.

There is some evidence that customers in higher quality jobs have smaller differences between their survey- and UI-based earnings. Workers employed in full-time jobs had much lower survey-to-UI earnings ratios than those employed in other jobs. Similarly, workers employed in jobs offering fringe benefits had much lower survey-to-UI earnings ratios than those employed in other jobs. However, we find no differences in survey-to-UI earnings ratios based on whether workers were employed in high-wage jobs.

APPENDIX J

FORMS AND WORKSHEETS USED TO SUPPORT IMPLEMENTATION OF THE ITA EXPERIMENT

APPENDIX J.1

THE GUIDE TO HIGH-RETURN TRAINING:

A RECIPE FOR SUCCESS FOR INDIVIDUAL TRAINING ACCOUNT CUSTOMERS

FOREWORD

Congratulations! You qualify for our one-stop training services! Our staff recognizes that training can be a big help to you. Training can greatly improve your chances to find work, earn good pay, and improve your career. Now you must decide on the best training for you.

Training decisions are complicated and important. Here are some reasons. Training may be expensive. The fact that we will help pay for training presents a terrific opportunity for you. Although you may still need to use other resources, the support our one-stop center will provide will surely help you. Training also represents an investment of more than just dollars. To succeed in training, you must also invest time and effort. You should consider your training decisions very carefully, to be sure that you get the *best* possible benefits from this effort.

Other factors are also important to consider before you choose a training course. How do you feel about returning to school? What is your learning style? What are your personal circumstances, needs, and pressures? Choosing the right program can mean the difference between successfully completing training or wasting an opportunity to help realize your dreams of a better career and a better life.

We want you to succeed in training! That's why we developed this booklet. The Guide to High Return Training should help you *succeed* in training and get started in a rewarding career.[16] As its title suggests, the guide is designed to help you identify "high return" training, which simply means training that will give you the *best* possible benefits from this important investment.

The Guide to High-Return Training was written to help you make good training decisions. It will help you to identify the benefits of training. It will also help you decide which training options are the most likely to meet your needs, and fit your lifestyle. The guide also explains the results of studies about the benefits of training. The findings from these studies may help you make good decisions.

[16]This guide was developed by Mathematica Policy Research, Inc. with support from the U.S. Department of Labor. It was developed specifically for the Individual Training Account demonstration.

A ROAD MAP
FOR THE GUIDE TO HIGH-RETURN TRAINING

The Guide to High-Return Training outlines five steps that you can follow to make your training decisions:

Step 1: Select an occupation. First, you must decide what job to train for. Some customers train for a whole new occupation, while others build up the skills they already have to lead to better jobs.

Step 2: Identify your training options. Once you choose an occupation, you must decide how to get the best training. Often there are many ways to find the type of training you want. The guide will help you identify the training programs that are most likely to meet your needs.

Step 3: Evaluate your training options. Next, it is important to gather information that will let you to compare your training options. The guide outlines a process that you can use to compare the costs to the benefits of training for each program. This should help you decide which option is best for you.

Step 4: Choose a program. The guide outlines a process you and your counselor can follow to put all the pieces together--benefits, limitations, and preferences--in order to make a confident training selection.

Step 5: ***Plan ahead.*** Once you have selected a program, you must make sure that you can afford to pay for the training. Before you set out for training, it will be important to plan for upcoming household expenses and develop a workable household budget.

The rest of the guide follows this five-step roadmap. Clearly, not everyone who is thinking about training completes these steps as they are presented here. In fact, you may already have a good idea of the occupation for which you would like to train or the program you wish to attend. Regardless of where you are in the process of making your training decisions, the information this guide provides can help you be more *confident* in the choices you make.

STEP 1: SELECT AN OCCUPATION

With thousands of occupations available today, it may be hard to decide on the best career path for you. Following are steps you can follow to identify occupations that may be good for you:

Match your interests and background with occupations. To start, you should consider your interests, skills, education, and work experience. If you have worked before, you may want to explore occupations that are similar to or that build upon that type of work. After all, you know the work and know that it is something you can do. However, training can also represent a terrific opportunity to consider something *new*. You may want to explore a different line of work! If you are not sure about the type of work you would like to do or are ready for a change, your counselor can meet with you to help you identify other possible options.

Explore high-wage demand occupations. As you consider possible occupations, keep in mind differences in how much you could earn at different jobs. Also, consider the availability of jobs in each occupation. Both considerations are important. Unless you are willing and able to move to a different area, your best bet may be training for an occupation with good pay for which jobs are available in your local area. Your counselor can help you identify occupations that offer high wages and are in demand locally.

Consider possible career paths. Consider how much you are likely to get paid immediately after completing training, as well as the possibilities for growth within each of the occupations you are considering. Also consider the types of jobs you could advance to in your career, both with or without additional training.

Research your career options fully. Unless you have worked in the field before, you will probably want to find out important information about the occupations you are considering. Knowing about starting pay, career paths, and the availability of jobs locally is a good start, but is not enough to make a truly informed decision. Often there are aspects of the work you may not have thought of, such as daily activities, stress on the job, or how you will travel to the job. Also consider benefits beyond pay, such as vacation and health insurance, that may sway your decisions. Your counselor can provide tools and point you to one-stop resources that can help you research occupations.

Commit yourself to the occupation. The success of your training experience is based on your commitment to the occupation you choose. Before you decide to train for an occupation, you should make sure that you would be comfortable doing this type of work for some time. This is not to say that you will never change careers in the future, but unless you complete training, find a job, and stay there for a while, you may not get the full benefits from your investment in training.

STEP 2: IDENTIFY YOUR TRAINING OPTIONS

Once you have chosen an occupation, you will need to find out the ways in which you can get training. You are likely to have several training options that may differ in many ways. For example, you may only need to take a few courses to enter the occupation. Then again, it may be easier to find a job if you complete a program that grants a degree. Differences in location, cost, and time needed in training will be fairly easy to figure out. However, other differences between your training choices may be less obvious.

Before trying to choose a program, you should "narrow the field" by finding two or three options that meet your most important needs. Here are some good steps to take:

1. ***Match Your Needs with Training Programs.*** Before looking at any training programs, think about what is important to you in training. This may include things about both the occupation and your personal life. For instance, will you need to get a particular degree to work in the career you want? Do you need to stay in the local area or can you train and find work elsewhere? Can you train full-time or do you need a more flexible schedule, such as attending evening or weekend classes? What is the longest amount of time that you can stay in training? If getting ahead in the career you choose is likely to require more training, will you be able to transfer the credits from the training program that you complete to another program? Your counselor can help you sort out your basic training needs.

2. ***Select Programs to Explore Further.*** Once you have found your most important training needs, choose two or three state-approved training options to review in detail. Your counselor can then lead you through a series of exercises to help decide which of these options would be best for you.

Making sure that your training plans fit well with your life style is clearly important. These concerns, however, must be considered along with the benefits that you expect from training. As noted, training is an investment, and investments usually require some sacrifice. Therefore, before you dismiss whole categories of training programs based only on personal preferences or limitations, you may want to consider the following research findings:[17]

- Research shows that each additional year of college credits results in higher earnings. Furthermore, individuals who complete programs that confer a degree or other widely recognized credential often earn higher wages than those who complete the same amount of course work without receiving a degree (Kane and Rouse 1995).

- A study of displaced workers who attended community college in Washington State shows that individuals who complete technically oriented and/or scientific courses experience larger earnings gains than those who complete less technical courses (Jacobson et al. 2000).

[17] When reviewing research studies, keep in mind that it may not be appropriate to apply some findings to your personal circumstances. Many studies refer only to specific groups of people or areas of the country. Furthermore, the evidence from studies on the effects of training is limited, and some studies may be inconclusive. Therefore, this research should be viewed only as food-for-thought as you make decisions about training.

STEP 3: EVALUATE YOUR TRAINING OPTIONS

Once you have identified several programs that seem likely to meet your most important training needs, you should take a closer look at these options. You will want to gather information about these programs so that you can compare them and make an informed, confident decision. You will want to know details about program requirements, the cost and length of the program, and financial aid options. Your counselor can provide tools and guide you to one-stop resources that can help you learn more about the programs you are considering.

Then, it will be important to look at the total investment each program would require and the benefits that you could expect to get from each. Your counselor can help you put these two pieces of information together--investments and benefits--to help you identify the training option (or options) that would benefit you the most.

Estimating Investments in Training. Investments in training include much more than a program's cost. You must consider the time and effort that you must invest in order to succeed in training. You will need to take into account the earnings you will give up in order to attend training, and your expenses related to training, such as transportation or child care. To figure out the investments that you would have to make, your counselor can help you evaluate the following for each of your training options:

- ***Direct Costs.*** These include costs that are directly related to the program you are considering. They include tuition, fees, and materials required to complete the program, such as books, tools, and other supplies.

- ***Indirect Costs.*** Indirect costs are expenses that are not related directly to the program you are considering but that you would have to pay in order to attend. For instance, you may have to pay for transportation to get to school or pay for child care in order to go to classes or spend time studying.

Estimating Wage Gains From Training. Your counselor can also help you estimate the increase in pay that you can expect to realize from training. You must compare the types of jobs you would be able to get if you did not attend training and those you could get after completing training. Completing training may also allow you to increase your work hours. Your counselor can help you understand how this will affect your earnings.

Evaluating the Net Benefits of Training. Your training options may require very different investments. To compare them, you should look at each program's benefits together with the investments. This way, you will be able to determine which programs would allow you to get back your investments and, more importantly, which would give you the *biggest* benefits.

STEP 4: CHOOSE A PROGRAM

Having figured out the benefits that you can expect from your different training options, you will be in a better position to select a program. Clearly, you will want to choose a program that gives you a *high* return on investment--that is, a program for which benefits are high compared to your investment in training.

When considering training options that seem to offer similar benefits, look at other program characteristics. For instance, one program's schedule or location may be more convenient for you. You may like the teaching style at one program better than the others. A program that costs slightly more may be more attractive because you would be able to complete it quicker, before your Unemployment Insurance benefits or severance payments run out. All of these things are important, since they could influence your chances of completing training.

STEP 5: PLAN AHEAD

After you have thought about all of these things and selected a program, you will want to make sure that you will be able to complete training and get the expected benefits. First, it will be important to figure out a way to cover your full costs of training. Second, you will want to make sure that you will be able to support yourself and your family while you go to training. Your one-stop counselor can help you develop a plan to pay for training and a workable household budget while you are in training.

- ***Determine How to Pay for Training.*** Your counselor can help you determine the total amount of money you will need to pay for training. The Individual Training Account, or ITA, should help you cover these costs. However, you may need additional help. Your counselor can help you apply for Pell grants, state grants, scholarships, or other programs for which you may qualify. If all these sources combined are still not enough to cover your total training costs, your counselor can help you decide if it would make sense to pay some training costs out of your own pocket, get student or personal loans, or consider other training programs.

- ***Develop a Household Budget.*** Before you begin training, it will be important to plan out your household expenses while you attend training. Your counselor will help you examine your household's income and financial responsibilities for the period while you would be attending training in order to develop a smart household budget. The more you plan, the better prepared you and everyone in your family will be for upcoming challenges and unexpected events, and the more likely you will be to work out these challenges successfully.

A FINAL NOTE

We hope that this Guide to High-Return Training will help you not only select the training that is right for you, but also increase your chances of *succeeding* in training and getting a rewarding career. Your one-stop counselor is ready to help you with *any* questions you may have about this guide and your career plans. Good luck!

REFERENCES

Jacobson, Louis, et al. *Participation in Community Schooling and Its Effects on Displaced Workers' Earnings and Employment Prospects: A Study of Displaced Workers from Washington State.* Rockville, MD: Westat, May 2000.

Kane, Thomas, and Cecilia Elena Rouse. "Labor-Market Returns to Two- and Four-Year College." *The American Economic Review,* vol. 85, no. 3, 1995, pp. 600-614.

APPENDIX J.2

PROGRAM RESEARCH

Participant:_____ **Date:**_____

INSTRUCTIONS: This worksheet will help you gather information about the programs that provide training for the occupation you have chosen. To help you decide which program is best for you, be sure to complete a separate form for each of the programs that you are considering. Try to use as many resources as possible when completing this form. Available resources include: (1) resource materials in the one-stop center, (2) visits to the prospective programs, and (3) interviews with current students, graduates, instructors, or administrators.

Vendor: _____

Program: _____

1. How long has the vendor been providing this type of training?_____
2. When does the next set of classes begins? _____
3. What is the application deadline? _____
4. What are the program's entry requirements? _____

   _____
5. What is the program's typical class size? _____
6. What percentage of applicants are typically accepted? _____
7. What is the program's duration? (How long does it take to complete?) _____
8. How is the program structured (for example, number of terms, classes per term, hours per week, timing of classesCday/evening/weekend)? _____

   _____
9. How much does it cost to attend this program? (What are tuition and fees per term? How have program costs changed over recent years?)_____

   _____
10. What other expenses are typically required (such as books, basic supplies, tools, uniforms, etc.)? _____

    _____

J.16 _____

11. What financial aid options are available? _____
12. What are the program's completion requirements? _____

    _____
13. What degrees or certificates do students receive upon program completion? _____

    _____
14. What percentage of students actually graduate (overall and within the past year)? _____
15. What types of jobs do graduates typically get? What types of businesses tend to employ them? _____

    _____

    _____
16. What are the average starting wages of graduates? _____
17. What are the average starting benefits of graduates? _____
18. What types of employment placement assistance is provided to graduates? _____

    _____

    _____
19. What do students tend to like and dislike about the program? _____

    _____

    _____
20. Am I likely to need to change my current child care arrangements if I attend this program? (If yes, describe.) _____
21. How far is the program from my home? Will I have reliable transportation to and from school? Will I need to room near the program? (If yes, describe.) _____

    _____
22. Other important considerations: _____

    _____

    _____

EXHIBIT J.3

TRAINING COSTS

INSTRUCTIONS: This worksheet will help assess how the costs of each training program you are considering compare to the resources you have available to pay for training. In the first column, enter the costs and resources for each term/session. Based on the number of terms/sessions that it will take you to complete the program, in the second column, estimate the total costs of the program and total resources you will have available.

Vendor: _____

Program: _____

Duration (in terms or sessions): _____

A.	**DIRECT TRAINING COSTS**		**Amount Per Term**				
	Tuition and Fees						
	Books						
	Supplies						
	Tools						
	Uniforms				**Number**		
	Other:	+			**of Terms**		**Subtotal**
	Subtotal for Direct Costs			**x**		**=**	
B.	**INDIRECT TRAINING COSTS**						
	Transportation						
	Room and Board						
	Child Care						
	Other:				**Number**		
	Other:	+			**of Terms**		**Subtotal**
	Subtotal for Indirect Costs			**x**		**=**	
C.	**ESTIMATED NON-ITA TRAINING RESOURCES**						
	Pell Grants						
	State Grants						
	Scholarships						
	Personal Savings or Loans				**Number**		
	Other:	+			**of Terms**		**Subtotal**
	Subtotal for Non-ITA Resources			**x**		**=**	
D.	**UNSUBSIDIZED TRAINING COSTS** (Direct Costs + Indirect Costs - Non-ITA Resources)**:**						

EXHIBIT J.4

ITA

TRAINING COSTS AND BENEFITS WORKSHEET

— FOR COUNSELOR USE ONLY —

Participant:_____ **Counselor ID:**_____

Date:

	1	2	3
OCCUPATION			
PROGRAM			
I. INVESTMENT IN TRAINING			
Program Costs			
A. Direct Training Costs (from Training Costs Form).....	$	$	$
B. Indirect Training Costs (from Training Costs Form).....	$	$	$
C. TOTAL PROGRAM COSTS (A + B).....	$	$	$
II. GAINS FROM TRAINING			
Earnings Increase in First Year After Training			
D. Estimated Wages Upon Completion of Training.....	$ per week	$ per week	$ per week
E. Wages If Customer Did Not Attend Training.....	$ per week	$ per week	$ per week
F. Weekly Wage Increase After Training (D - E).....	$ per week	$ per week	$ per week
G. Yearly Work Schedule Upon Completion of Training.....	weeks per year	weeks per year	weeks per year
H. ESTIMATED EARNINGS INCREASE (F * G).....	$ per year	$ per year	$ per year
J. PRESENT VALUE OF EARNINGS GAINS (4 * H).....	$	$	$
III. ESTIMATED NET EARNINGS GAINS [J - C].....	$	$	$

Exhibit J.4 *(continued)*

PROGRAM ENDORSEMENT WORKSHEET
— FOR COUNSELOR USE ONLY —

(Refer to previous page for program descriptions)	1	2	3
1. Is the program expected to have positive (+) net benefits from training?	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**
2. Does the program seem appropriate for the customer? - Do the customer's skills and interests match the occupation/program? - Does the program appear feasible with the ITA and other resources?	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**
3. Does the customer have a reasonable chance of completing training? - Do program attendance requirements seem compatible with the customer's circumstances? - Could the customer reasonably support him/herself and his/her family for the duration of training?	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**
4. Does the customer have a reasonable chance of finding employment in this occupation if s/he completes the program? - Is this a high-wage occupation in demand in the local area? - Do program graduates have a reasonable record of success finding employment? - Is the customer planning or willing to relocate to another area? - Does the customer already have employment lined up?	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**
5. Relative to the other programs being considered, does this program offer the highest estimated net earnings gains (Item III)? **NOTE:** ***Programs within $500 of the highest value should ALL be marked YES.***	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**
6. Are there other factors leading you to endorse this program? **NOTE:** ***If marked yes, counselor MUST provide an explanation.*** - Does this program include features that significantly improve the customer's chances of completing training (e.g., individualized or integrated basic skills instruction)? - Does the vendor have a particularly strong track record which could lead to better employment outcomes for the customer (e.g., higher wages at placement which mean a higher wage replacement rate if the customer is a dislocated worker)? - If the customer is considering different occupations, would this program provide access to jobs that are more appealing for important non-wage reasons (e.g., they match the customer's interests more closely, offer benefits, or give access to a career ladder)? [*If so, recommend the program with the* ***highest*** *estimated net gains among programs being considered of this type.*] - Are there other reasons why this program seems particularly appropriate for this customer (e.g., the customer could transfer credits or complete before UI benefits run out)?	☐ **Yes** ☐ **No** **Explain:**	☐ **Yes** ☐ **No** **Explain:**	☐ **Yes** ☐ **No** **Explain:**
COUNSELOR'S ENDORSEMENT: Is this program recommended? **NOTE:** ***To recommend a program the following conditions must apply:*** **(a) Questions 1, 2, 3, and 4 must ALL be checked YES** **AND** **(b) Either question 5 OR question 6 must ALSO be checked YES.**	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**

EXHIBIT J.5

INCOME AND EXPENSES

Participant: _____ **Date:** _____

For training in [Program/Vendor]: _____

Projected training period: **From:** _____ **To:** _____

INSTRUCTIONS: As you make your final training choice, you can use this worksheet to examine whether you will have enough income to cover your living expenses while you attend training. When completing the form, think about the income and expenses that you have daily and monthly as well as those that occur less frequently, say once or twice a year. You should also consider special circumstances. For instance, if you will need to make a large payment (such as auto insurance) shortly after the training program ends, you should include the amount you will need to save for that payment while you are in training.

TOTAL INCOME WHILE IN TRAINING

	(A) INCOME		**(B)** NUMBER OF TIMES WHILE IN TRAINING		**(Multiply A x B)** INCOME WHILE IN TRAINING
WAGES					
Personal Wages (after tax) - Source 1	$	x		=	$
Personal Wages (after tax) - Source 2	$	x		=	$
Personal Wages (after tax) - Source 3	$	x		=	$
Household Members' Wages (after tax)	$	x		=	$
Household Members' Wages (after tax)	$	x		=	$
Household Members' Wages (after tax)	$	x		=	$
			TOTAL WAGES:		$
OTHER INCOME					
Unemployment Insurance (after taxes)	$	x		=	$
TANF (Cash Assistance)	$	x		=	$
GA (General Assistance)	$	x		=	$
Food Stamps	$	x		=	$
SSI (Supplemental Security Income)	$	x		=	$
Worker's Compensation	$	x		=	$
Child Support (after taxes)	$	x		=	$
Alimony (after taxes)	$	x		=	$
SSA or Survivor's Benefits (after taxes)	$	x		=	$
Pension/Annuities (after taxes)	$	x		=	$
Armed Services (after taxes)	$	x		=	$
Other: _____	$	x		=	$
Other: _____	$	x		=	$
Other: _____	$	x		=	$
Other: _____	$	x		=	$
			TOTAL OTHER INCOME:		$
			TOTAL INCOME (total wages + other income):		$

EXHIBIT J.5 (*continued)*

TOTAL EXPENSES WHILE IN TRAINING

	(A) EXPENSE	(B) NUMBER OF TIMES WHILE IN TRAINING	(Multiply A x B) EXPENSES WHILE IN TRAINING
HOUSEHOLD EXPENSES			
Mortgage/Rent	$	x	= $
Property taxes	$	x	= $
Other taxes	$	x	= $
Food	$	x	= $
Utilities (Gas, Electric, Water)	$	x	= $
Telephone	$	x	= $
Other:	$	x	= $
TOTAL HOUSEHOLD EXPENSES:			$
TRANSPORTATION			
Bus/Train/Subway	$	x	= $
Gasoline	$	x	= $
Vehicle repairs	$	x	= $
Vehicle insurance	$	x	= $
Other:	$	x	= $
TOTAL TRANSPORTATION:			$
FAMILY CARE AND HEALTH			
Child care	$	x	= $
Elderly Care	$	x	= $
Insurance (health, dental, life)	$	x	= $
Medication	$	x	= $
Doctor visit co-pays	$	x	= $
Other:	$	x	= $
TOTAL FAMILY CARE AND HEALTH:			$
CREDITORS			
Credit card debt	$	x	= $
Auto payments	$	x	= $
Loans (student, bank, etc.)	$	x	= $
TOTAL CREDITORS:			$
MISCELLANEOUS			
Clothing	$	x	= $
Child support	$	x	= $
Entertainment	$	x	= $
Other:	$	x	= $
Other:	$	x	= $
TOTAL MISCELLANEOUS:			$
TOTAL EXPENSES: (household expenses + transportation + family care and health + creditors + miscellaneous)			$

NET CASH FLOW WHILE IN TRAINING (+/-):
Total Income minus Total Expenses: $

EXHIBIT J.6

TRAINING BUDGET

Participant: _____ **Date:** _____

For training in [Program/Vendor]: _____

INSTRUCTIONS: As a final step in confirming your training choice, consider whether it is financing feasible for you to complete the training program you have chosen. The calculations in Part I of this worksheet will help clarify how the out-of-pocket portion of training costs will affect your household's cash flow. You should use the Training Costs (TC) worksheet and the Income and Expenses (IE) worksheet to complete Part I. Once you have finished the calculations, you can use the questions in Part II of this form to discuss any cash flow issues with your ITA counselor.

CALCULATION OF NET CASH FLOW WHILE IN TRAINING

A.	Direct Training Costs (see TC)	$ _____
B.	Indirect Training Costs (see TC)	$ _____
C.	Non-ITA Resources for Training (see TC)	$ _____
D.	Estimated ITA award (from counselor)	$ _____
E.	Out-of-Pocket Training Costs *(A + B - C - D)*	$ _____
F.	Net Cash Flow While in Training (see IE)	$ _____
G.	**Net Cash Flow Minus the Cost of Training (*F - E*)**	**$ _____**

II. QUESTIONS FOR DISCUSSION WITH YOUR ITA COUNSELOR

If your net cash flow minus the costs of training (item G above) is expected to be negative (-):

- Are there other sources of income that you forgot to include in your calculations?
- Are there any monthly obligations that will end while you are in training?
- Is it possible to reduce any of your household's monthly expenses?
- If you do not already plan to do so, is it possible to work part-time while you attend training?

If your net cash flow minus the costs of training (item G above) is positive (+):

- Are any of your income sources potentially unstable (for example, will your Unemployment Insurance benefits run out while you are still in training)?
- Have you included all expenses that spike up during the training period (e.g., insurance payments, property taxes, etc.)?
- Do the monthly expenses that you calculated realistically reflect your lifestyle and your family's lifestyle?

EXHIBIT J.7

ITA

TRAINING OPTIONS COMPARISON

Participant:_____ **Date:** _____

INSTRUCTIONS: In order to choose the program that is right for you, you will need to evaluate the merits of each potential program. Presented below are several questions to help you and your counselor discuss your training options. When you talk with your counselor, be sure to bring the Program Research worksheet that you completed for each program.

OCCUPATION **PROGRAM**	_____ _____	_____ _____	_____ _____
1. Does the program provide training for the occupation that you want to pursue? - Do graduates of program tend to find jobs that interest you? - Does the program and occupation closely match your interests? - Do graduates of the program have success finding good jobs that pay well?	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**
2. Will you be able to pay for the full cost of training at this program? - Will the program costs be fully covered by your ITA? - If not, can you access other sources of financial aid, use your personal savings, or take out personal loans to help pay for training?	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**
3. Do you have a reasonable chance of completing this program? - Are you confident that you have the skills needed to complete the program? - Can you support yourself and/or your family while you attend training? - Does the program seem compatible with your lifestyle and family circumstances? (For instance, will you be able to attend all your classes, do homework, and study for tests? Do you have friends or family who can help with some of your other responsibilities?)	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**	☐ **Yes** ☐ **No**
4. Are there reasons, other than cost, that make this program seem more appealing than other programs that you are considering? - Can you complete the program before your UI or severance payments run out? - Is the program much shorter than the others? - Is the location of the program more convenient for you? - Is the course schedule more appealing (part-time vs. full-time, weekend or evening classes)? - Will you receive a degree or credential after completing the program? - Does the teaching style seem more appropriate for you?	☐ **Yes** ☐ **No** Explain:	☐ **Yes** ☐ **No** Explain:	☐ **Yes** ☐ **No** Explain:
RANK THESE PROGRAMS IN THE ORDER OF YOUR PREFERENCE: (Mark the program you like the best as number 1)			