DOES TRADE ADJUSTMENT ASSISTANCE MAKE A DIFFERENCE?

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The U.S. Trade Adjustment Assistance (TAA) program provides workers who have lost their jobs due to increased trade with income support and training, job search, and relocation benefits. This paper uses data collected by the Department of Labor on TAA beneficiaries to provide the most recent econometric evaluation of the effectiveness of the TAA program. Summary statistics suggest that the TAA program successfully targets displaced workers who have a greater difficulty finding new employment. However, using propensity score matching techniques we find that while the required training component of the program improves the employment outcomes of beneficiaries, on average the TAA program has no discernible impact on the employment outcomes of the participants. (*JEL* F16)

I. INTRODUCTION

For over 30 years, the U.S. Trade Adjustment Assistance (TAA) program has provided workers who can show that they have lost their jobs due to increased imports with income support, training, job search, and relocation benefits. The United States spent $855.1 million to assist approximately 150,000 TAA beneficiaries in 2007. This amount is expected to increase dramatically in the future as the program was significantly expanded by the American Recovery and Reinvestment Act of 2009.

The TAA program receives a great deal of support from policy makers; one of the unstated goals of the program is to decrease political resistance to new trade liberalization efforts and Congress has approved additional funding for the TAA program with virtually every new free trade agreement that has been implemented since the program's inception.[1] There is little evidence, however, regarding whether workers have actually been helped by the TAA program. On the contrary, in 2007 the Office of Management and Budget (OMB) rated the program "ineffective" because it failed to demonstrate the cost effectiveness of achieving its goals.

The Department of Labor reports simple statistics that measure, for example, the percentage of beneficiaries who are able to find employment following their participation in the TAA program. Statistics such as these fail to take into account the fact that these participants may have found the same employment absent participation in the TAA program. The more appropriate measure of a program's effectiveness is to what extent the program *changed* the employment outcome of TAA participants.

This paper provides an in-depth statistical evaluation of the effectiveness of the TAA program. Summary statistics suggest that the TAA program successfully targets displaced workers who have greater difficulty finding new employment, but we find no statistical evidence that the TAA program actually improves the

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1. Critics of the TAA program sometimes argue that there is no justification for funding a program that helps workers displaced from their jobs due to increased imports while not funding similar programs for workers displaced because of technological change or industrial restructuring.

ABBREVIATIONS

ATAA: Alternative TAA
ATE: Average Treatment Effect
CIA: Conditional Independence Assumption
CPS: Current Population Survey
GAO: General Accounting Office
NAICS: North American Industry Classification
OMB: Office of Management and Budget
TAA: U.S. Trade Adjustment Assistance
TAPR: Trade Act Participant Reports
TRA: Trade Readjustment Allowance
UI: Unemployment Insurance

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employment outcomes of beneficiaries. We do find strong evidence, however, suggesting that those workers who participate in TAA-funded training opportunities are more likely to obtain reemployment, and at higher wages, when compared to TAA beneficiaries who do not participate in training. The results suggest that policy makers should require that the TAA program undergoes a comprehensive evaluation as to how it can more effectively assist displaced workers.

II. THE TRADE ADJUSTMENT ASSISTANCE PROGRAM

The U.S. TAA program, first developed in 1962 and amended repeatedly since that time, compensates workers harmed by increased import competition.[2] The goals of the program are threefold: encourage the rapid reemployment of participants; provide training and income support to allow participants to achieve reemployment; and assist participants to obtain reemployment in fields where they are "likely to remain employed and earn wages comparable to their prior jobs."[3]

To be eligible for the TAA program, a company affiliate must file a petition with the Department of Labor alleging that workers in the firm lost their jobs (or suffered a reduction in hours and/or wages) as a result of increased imports or shifts in production outside of the United States.[4,5] The Department of Labor must then decide whether the firm is eligible for the program under current guidelines.

Once a firm is certified, any worker from the firm laid off up to 1 year before or 2 years after the petition was filed is eligible for TAA benefits. TAA benefits fall into five categories: training, income support, relocation allowances, job search allowances, and a health coverage tax credit.[6] Specifically, the TAA program will pay for up to 104 weeks of any basic training program, or up to 130 weeks if the worker is in need of remedial education. Participants are eligible for an additional 26 weeks of unemployment insurance, known as the Trade Readjustment Allowance (TRA). Workers must be enrolled in or have completed training to receive TRA benefits, although some workers may be eligible for a waiver of the training requirement.[7] Workers who are participating in a training program may receive 52 weeks of "additional" TRA, and those workers enrolled in remedial education may be eligible for remedial TRA payments.

Other benefits include a job search and relocation allowance; the maximum payment for each allowance is $1,250. Finally, TAA-eligible workers qualify for the Health Coverage Tax Credit, which pays 65% of the premium for qualified health insurance plans.

III. LITERATURE REVIEW

In part because of a lack of data, there have been few empirical evaluations of the TAA program. Early evaluations, including Corson and Nicholson (1981) and the U.S. General Accounting Office (GAO) (1980) found that 70% of TAA support went to workers who eventually returned to work with their previous employers.

More recently, Decker and Corson (1995) used a survey of TAA participants between 1988 and 1989 to evaluate the impact of the 1988 amendments to the program that mandated participation in a training program. The authors find that the 1988 changes in the TAA program increased participation in training programs, reduced the amount of TRA collected by beneficiaries, and led to a decline in the duration of unemployment. The authors conclude, however, that training does not have a substantial positive effect on the earnings of TAA participants.

Marcal (2001) uses the same data as Decker and Corson (1995) to study whether the TAA program increases the earnings of beneficiaries over comparable unemployment insurance (UI) exhaustees. Marcal (2001) also finds little evidence that the TAA program improves the

2. For an excellent history of the TAA program, see Baicker and Marit Rehavi (2004).

3. "Overview of the TAA program," Department of Labor.

4. State labor agencies may also file a petition on behalf of the workers. The program rules described in this section are those in place between 2002 and 2008. The American Recovery and Reinvestment Act of 2009 made a number of important changes, including expanding TRA payments to 130 weeks.

5. Service workers were ineligible for the TAA program prior to May 18, 2009. The firm's shift in production must either be (1) to a country that receives preferential tariff treatment or (2) likely to result in an increase in imports.

6. The Trade Act of 2002 created an additional TAA program for workers over 50 years old which will not be analyzed in this paper: the Alternative TAA (ATAA) program.

7. Workers who show that they are subject to recall, in poor health, near retirement, or already possess marketable skills can obtain a waiver of the training requirement. Waivers are also available to workers who can prove that training is either unavailable or they are unable to enroll in training.

earnings of displaced workers, although TAA beneficiaries that participate in training programs were employed more on average than both UI exhaustees and TAA beneficiaries who did not participate in a training program.

The only recent evaluations of the TAA program have been conducted by federal agencies such as the GAO. In the study most relevant to this paper, GAO (2006b) conducted a survey of workers from five trade-related plant closures. They found that reemployment rates at these plants ranged from 33% to 60%. The study further found that the majority of reemployed workers were earning less than they had previously.

This paper improves upon the most recent evaluations of the TAA program in a number of ways. Perhaps most importantly, rather than relying on summary statistics of the employment outcomes of TAA beneficiaries, we use propensity score matching econometric techniques to estimate the actual impact of the TAA program on the participants.

IV. EMPIRICAL METHODOLOGY AND DATA

As described in Heckman, LaLonde, and Smith (1999), there is an extremely large literature devoted to the evaluation of various labor market programs. To accurately evaluate the impact of a program such as TAA on workers, the researcher must compare the outcome of interest for program participants with the outcome for a "comparable" group of nonparticipants.

Ideally, one would want to compare the outcome of interest (i.e., whether or not reemployed) for displaced workers when they participate in the TAA program to the outcome for these same workers when they do not participate. The problem is that the researcher never observes how the TAA participant would have fared if they chose not to participate in the program.

Alternatively, one could compare the outcome of displaced workers who enrolled in the TAA program with the outcome of those who were ineligible or chose not to enroll in the TAA program. Unfortunately, estimators such as these suffer from selection bias if those who choose to participate in the program are systematically different from those who are ineligible or chose not to participate. Although controlling for differences in observable characteristics can help alleviate this problem, selection bias will still remain if those who participate are systematically different from nonparticipants in their unobservable characteristics.

Selection bias in the case of the TAA program can arise at three separate points. First, an eligible entity (firm, union, etc.) must choose to apply for TAA certification. Next, the Department of Labor must certify the firm as eligible for the TAA program. Finally, the individual worker must choose to apply for TAA benefits. According to the Department of Labor Statistics, only 38.7% of those workers eligible for TAA benefits between 2003 and 2005 chose to participate in the program.[8]

As discussed in Heckman and Navarro-Lozano (2004), a number of econometric methods have been developed to deal with selection bias of this nature, including control function methods such as the Heckman selection model.[9] Control function methods explicitly model the stochastic dependence of the unobservable characteristics in the outcome equation on the observable characteristics. To do this, these methods typically require specification of the functional form of the outcome equation, and thus can be susceptible to misspecification.

Heckman, Ichimura, and Todd (1997, 1998) proposed another solution to the selection bias problem called propensity score matching in which each participant in the labor program is matched with a "control" observation from an alternative dataset using a propensity score; the outcome variable of the participant is then matched with the outcome of the control observation.[10] Specifically, propensity score matching techniques use the estimates from a logistic or probit regression analysis to generate the predicted probability of program participation for each observation based on observed characteristics such as age, gender, and education level. The impact of the program, commonly known as the average treatment effect (ATE), is calculated as the average difference in the outcome variable between the program participants and the control observations that they are matched to based

8. This figure was calculated by dividing the number of new workers enrolling in the TAA by the estimated number of workers covered by new TAA certifications.

9. This was the method used in the Marcal (2001) analysis of the TAA program.

10. Although the method was initially developed in Rosenbaum and Rubin (1983), it became a popular method of evaluating labor market policies following the work of Heckman, Ichimura, and Todd (1997, 1998).

on the similarity of the predicted probability or propensity score.[11]

Propensity score matching models rely on the assumption that conditional on observable characteristics the decision to participate in a program is completely independent of the unobservable characteristics. In other words, it assumes that after conditioning on observables one can assume that the decision to participate in a program is completely random. Although this is a strong assumption, we choose to use this method to analyze the impact of the TAA program because we believe it is preferable to specifying a functional form for the outcome equation. Specifically, we correct for potential selection bias by comparing the employment outcomes of TAA participants to the employment outcomes of displaced workers who did not participate in the TAA program, but have observable characteristics similar to TAA participants as measured by their propensity score.

To conduct this evaluation of the TAA program, we utilize data from the Department of Labor's Trade Act Participant Reports (TAPR). Since 1999, each state has had to submit TAPR reports to the Department of Labor every quarter with data on individuals who exited the TAA program.[12] TAPR reports include data on the TAA participant, including their gender and education level, and the services they received under the TAA program. Outcome variables in the TAPR reports include whether the participant was employed in the first three quarters after exit and the worker's earnings in these quarters.

Unfortunately, the TAPR data have a number of weaknesses. The U.S. GAO (2006a) notes that only half of the states reported that the data they submit in the TAPR include all TAA participants who exit the program. Other states are inaccurately recording some workers' employment status due to limited information technology systems. Nevertheless, the TAPR database is the best data available regarding participants in the TAA program.

Our original TAPR dataset included data on 286,840 individuals who exited the TAA program between the final quarter of 2000 and the first quarter of 2008. In order to observe the industry of the worker, we limit the dataset to those observations which reported a valid TAA Petition Number.

We include only manufacturing sector workers in the final dataset; program rules in place prior to 2009 ensured that virtually all TAA beneficiaries were employed in the manufacturing sector. Note that slightly over 80% of the petitions were filed by firms in the manufacturing sector, with the largest number filed by the apparel, electronics, and motor vehicle industries. Moreover, GAO (2007) reported that 44% of the 2,500 petitions denied TAA certification in Fiscal Year 2006 were denied because workers were not involved in producing a product.[13] Finally, we limit our sample to those workers who were displaced between 2003 and 2005 and who exited the TAA program in the fourth quarter of 2005.[14] The number of TAA participants in the final dataset is 5,125.

Table 1 summarizes the characteristics of the TAA beneficiaries in our sample, and lists the specific TAA program provisions that these individuals profited from. We observe an overwhelming majority (77.5%) of workers enrolled in a TAA-funded training program. Slightly more than 90% of the workers who chose not to participate in a training program received a waiver from the training requirement, thus were still eligible to receive TRA payments. Of those that received a waiver, at least one-quarter qualified for a waiver because they possessed "marketable skills," while approximately 10% qualified because training was unavailable in their area.[15]

A much smaller percentage of beneficiaries (59%) in the sample received TRA payments. Of those workers who did not receive TRA

11. Some evaluations of the propensity score matching techniques have questioned the ability of the econometric technique to effectively control for selection bias. Smith and Todd (2005) found that results from propensity score techniques are highly sensitive to the choice of the comparison sample and the set of variables included in the estimation. Dehijia (2005) responded that propensity score matching techniques can be a powerful tool as long as researchers examine the sensitivity of their results to small changes in the specification of their propensity score.

12. Participants are deemed to have exited the program if (1) they have a known date of completion for all TAA-funded services or (2) they have not received any TAA-funded services for 90 days and are not scheduled for future services.

13. An analysis of certifications between 2003 and 2005 suggests a similar pattern; of the 2,962 petitions denied certification, 43% were in nonmanufacturing industries.

14. We limit the sample in this way in order to better match the period during which we observe the control observations in the CPS dataset. We also exclude from the analysis 48 participants who reported that they were choosing to participate in the ATAA program and 13 individuals for which we had no employment outcome data.

15. The percentage of waivers attributed to each reason is likely much lower than the true percentage of workers who received a waiver due to these reasons. Officials failed to record a reason for the waiver in 62% of observations.

TABLE 1
Summary Statistics

	Trade Act Participant		CPS Displaced Worker	
	Mean	**Standard Deviation**	**Mean**	**Standard Deviation**
Male	0.563	0.496	0.631	0.483
Age	47.136	11.083	45.134	11.768
Education (highest level)				
Years of education	11.872	2.378	13.373	2.634
High school graduate	0.606	0.488	0.388	0.487
Some college	0.134	0.340	0.272	0.445
BA+	0.068	0.251	0.217	0.412
Tenure (in years)	9.502	9.933	8.836	8.903
Import sensitivity	0.325	0.164	0.295	0.185
One-year growth, imports	0.089	0.045	0.087	0.061
Intraindustry trade	0.670	0.281	0.661	0.247
Industry unionization	0.118	0.116	0.116	0.113
Average industry wage	14.181	2.703	14.356	2.784
Industry layoffs	56.232	37.266	55.233	44.764
State unemployment rate	0.059	0.008	0.056	0.010
Average state wage	15.152	1.692	15.618	1.878
State layoffs	19.370	15.900	21.223	19.006
TAA benefits				
Training	0.775	0.417	—	—
Job search	0.013	0.113	—	—
Relocation	0.014	0.116	—	—
TRA	0.589	0.492	—	—
Postdisplacement				
Employed (% sampled)	0.789	0.407	0.850	0.356
Change in wages	−0.305	0.740	−0.179	0.499
Number of observations	5,125		469	

payments, 66% were still receiving basic unemployment insurance. In other words, they exited the TAA program prior to exhausting their basic 26 weeks of unemployment insurance. Less than 2% of program beneficiaries collected job search or relocation benefits.

To conduct an evaluation of the TAA program, we need to compare the employment outcomes of the TAA participants to the employment outcomes of a group of control observations. We use workers from the January 2006 Displaced Worker, Employee Tenure, and Occupational Mobility Supplement File of the U.S. Census Bureau's Current Population Survey (CPS). Like the TAPR data, the CPS Displaced Worker Survey includes information on workers who lost their job because their company closed or moved, their position was abolished, or there was insufficient work to support their position, although these workers did not necessarily lose their job due to increased imports.[16]

The Displaced Worker Survey includes data on 5,611 workers who reported that they were displaced from their job between 2003 and 2005; nearly 80% of these workers were employed in construction or a service industry. Because the TAA program was targeted to workers displaced from firms that produce a product, we limit our sample to manufacturing workers; the final CPS comparison sample includes data on 469 individuals.

Table 2 shows some disparity between the industries represented in the TAA sample of workers when compared to the manufacturing workers included in the CPS. For example, 20% of TAA participants were displaced from the textile, apparel, and leather manufacturing sectors compared to only 7% of CPS workers. The differences in the worker's industry of employment

16. We exclude from the analysis those workers who reported they were displaced due to the completion of a seasonal job or the failure of a self-operated business. We do not observe whether workers in the CPS sample participated in the TAA or other labor adjustment program. To the degree that there is overlap in the two samples, our results would be biased toward finding no impact of the TAA program on workers.

TABLE 2
Top Industries of Lost Employment

Industry	Percentage of TAA Participant Sample	Percentage of CPS Control Sample
Nonmetallic minerals	3.8	1.7
Primary metals/fabricated metal products	9.0	13.6
Machinery	14.2	8.1
Computer and electronic products	17.2	18.9
Electrical equipment	5.9	4.2
Transportation equipment	5.6	9.6
Wood products	2.4	3.4
Furniture manufacturing	3.7	4.2
Miscellaneous manufacturing	4.2	6.1
Food, beverage, and tobacco	1.6	7.7
Textile, apparel, and leather	19.5	7.5
Paper and printing	5.7	6.6
Chemicals	1.7	5.3
Plastics and rubber products	5.3	2.8

are also reflected in the average import penetration ratio of the industry from which the workers were displaced; the average import penetration ratio in industries displacing TAA workers was 32.5% compared to 29.5% in the CPS sample.

Table 1 shows several other distinctions between the two groups. TAA participants are more likely to be female and from states with lower manufacturing wages and higher unemployment rates. The participants are also slightly older and had more experience at the job from which they were displaced when compared to their CPS counterparts.

Workers from the CPS sample are more educated than TAA beneficiaries: 80.8% of the TAA beneficiaries in our sample have a high school diploma compared to 87.7% of those sampled from the CPS. Similarly, only 7% of TAA beneficiaries have a college degree compared to 22% of the CPS sample of displaced workers. The CPS sample is more reflective of the educational attainment of the U.S. population as a whole—87% of the U.S. population has a high school diploma while 29% has a college degree. According to GAO (2001), officials in communities with a large number of TAA participants reported needing to "improve local educational systems, which often had high school dropout rates much higher than the national average."

The most dramatic differences between the two samples may be the post displacement outcomes. TAA beneficiaries were less likely to have found new employment when compared to their CPS counterparts: 78.9% of TAA participants were employed at the time of survey compared to 85.0% of CPS workers. Perhaps of more concern, TAA workers earned on average 30% less than they made at their previous job. Displaced workers from the CPS also suffered from reduced wages, but they earned only 18% less in their new place of employment.

These summary statistics do not indicate that the TAA program *caused* the wage loss or made it more difficult for displaced workers to find employment. In fact, the differences in the two samples suggest that the TAA program successfully targets those workers who have a more difficult time finding new employment following displacement. As GAO (2001) notes, because TAA beneficiaries tend to be older and less educated than other workers, they are less mobile and have a harder time reentering a workforce that increasingly requires more skills and training. The propensity score matching technique tries to control for the selection bias that could result from the unique characteristics of TAA beneficiaries to allow us to determine the impact of the TAA program.

V. PROPENSITY SCORE MATCHING

As hinted to in Section IV, the ability of the propensity score matching technique to successfully estimate the ATE in the presence of potential selection bias relies on two fairly strong assumptions. First, the conditional independence assumption (CIA) must hold: conditional on a set of observable characteristics, outcomes are independent of whether the individual participates in the program under investigation. In other words, all variables that influence whether the individual participates in the program and the outcome variable are observed by the researcher.[17] Second, the common support assumption must hold: individuals with the same covariates must have a positive probability of being both TAA participant and nonparticipant.

As discussed in Caliendo and Kopeinig (2008), researchers must make a number of

17. Heckman, Ichimura, and Todd (1998) demonstrate that this assumption is overly strong for identification of average treatment effects; instead, all that is needed is for the *mean* outcome to be independent of whether individuals participate in the program.

decisions when implementing propensity score matching to help ensure that these two assumptions hold. First, the researcher must choose which observable characteristics to include in the propensity score estimation. Next, the researcher must choose which matching algorithm to use. This section discusses each decision in turn, as well as the specification tests and sensitivity analyses we conducted during our analysis.

A. *Estimation of the Propensity Score*

We estimate the propensity score using a probit regression in which the dependent variable equals one for TAA participants.[18] In order for CIA to hold, the researcher must include in the propensity score estimation all variables that jointly influence the decision to participate in the TAA program and the employment outcome variables. Heckman, Ichimura, and Todd (1997) show that omitting variables can significantly increase the bias in the estimated average treatment effect. However, Caliendo and Kopeinig (2008) also note that including too many variables in the propensity score estimation can increase the variance of the average treatment effect. Thus, they suggest that economic theory and a clear understanding of institutional settings should be used to build the model.

As Heckman and Navarro-Lozano (2004) note, the propensity score matching literature provides no clear guidance as to how to choose which variables should be included in the propensity score model. Specifically, they state that "[t]here is no support for the commonly used rules of selecting matching variables by choosing the set of variables that maximizes the probability of successful prediction into treatment or by including variables in conditioning sets that are statistically significant in choice equations." We choose to present results from two specifications of the propensity score. The first specification includes a wide variety of industry, geographic, and individual-level characteristics that potentially impact both the individual's participation in the TAA program and his or her employment outcome. Variables included in this specification are discussed below.

The leading determinants of whether a worker is eligible for the TAA program are associated with the characteristics of their industry. First, the more import sensitive the worker's industry, the more likely it is that their displacement is due to a surge in imports and, thus, the more likely that the worker is eligible for the TAA program. Using trade data from the U.S. International Trade Commission and production data from the Bureau of Economic Analysis (BEA), we construct the industry's import penetration ratio (*Import Sensitivity*) by dividing industry imports by domestic consumption, or the value of the industry's production less net exports.[19] We also construct the industry's 1-year growth in imports and a measure of the industry's intraindustry trade.[20] The propensity score estimation includes squares of both the import sensitivity and intraindustry trade variables in order to account for potential nonlinearities in these measures.

Given the role that unions play in filing for TAA benefits, we include the unionization rate of the industry (*Unionization Rate*).[21] We also include the average hourly wage in the industry (*Average Industry Wage*), which we calculate using data from the full January 2006 CPS.

It is important to control for individual-level characteristics to capture other determinants of the decision to participate in the TAA program and the employment outcome. The TAPR and CPS datasets include a number of demographic variables that have traditionally been used to explain employment outcomes, including the age of the individual, gender, the level of education, and the length of time the worker had been employed with the firm from which he or she was displaced (*Tenure*).[22]

To control for both geographic differences and differences in the macroeconomic conditions facing workers in the TAA and CPS samples, we include the state unemployment rate and a dummy variable for those workers who were displaced from their jobs in 2003, the

18. The results from estimations using a logit model were not qualitatively different from those presented here.

19. We use 2003 import and production data. BEA and CPS concordances were used to match BEA production data to six-digit North American Industry Classification (NAICS) import and export data and the NAICS-level data to Census Industry Classification Codes.

20. Intraindustry trade is calculated by dividing the minimum of the industry's imports or exports over one-half of the sum of imports and exports in the industry.

21. Between 2003 and 2005, 14% of TAA petitions were filed by a union.

22. Although the employment literature also suggests that race plays an important role in employment outcomes, we exclude race variables from our estimation. Race variables included in the TAPR were too unreliable to be included.

TABLE 3
Estimates from Probit Estimation of Propensity Score[a]

	TAA Beneficiary		Training Participant	
	Coefficient	**Standard Error**	**Coefficient**	**Standard Error**
Male	0.033	0.057	0.137*	0.045
Ln (age)	0.384*	0.113	−0.730*	0.094
Ln (years education)	−0.731*	0.234	0.656*	0.070
Education				
High school	0.324*	0.101	−0.159*	0.067
Some college	−0.220	0.137	−0.041	0.092
BA+	−0.138	0.171	−0.397*	0.116
Tenure	−0.022	0.089	0.266*	0.062
Tenure^2	0.000	0.000	−0.001*	0.000
Trade				
Import sensitivity	6.332*	0.622	1.034**	0.556
Import sensitivity^2	−7.087*	0.818	0.008	0.705
Intraindustry trade	−3.027*	0.623	0.412	0.537
Intraindustry trade^2	2.771*	0.481	0.135	0.407
Year import growth	0.166	0.642	3.870*	0.624
Unionization rate	−0.302	0.803	1.190*	0.494
Unionization rate^2	4.999*	2.215	−4.111*	1.094
Average industry wage	−0.057*	0.014	−0.068*	0.011
Industry layoffs	—	—	16.721*	3.560
State unemployment rate	17.039*	3.161	27.352*	2.741
Year 1993	0.600*	0.060	—	—
Number of observations	5,472	—	5,018	—
Pseudo r^2	0.1609	—	0.125	—
F-test	503.47	—	655.53	—

[a]Results from constant not reported.
*,**indicate those parameters significant at the 5% and 10% levels, respectively.

first year of the sample. Recall that our sample includes CPS workers interviewed in January 2006 and TAA participants who exited the TAA program and appeared in the TAPR dataset in the fourth quarter of 2005. Workers from both samples were displaced between 2003 and 2005. However, a higher proportion of our TAA sample was displaced in 2003 when compared to the CPS comparison group because workers are included in the TAPR dataset only after they exit the TAA program; the longer the time since displacement, the more likely TAA workers are to appear in the dataset. The longer the time since displacement, the more likely workers are to have found new employment, thus the year of displacement will have a significant impact on employment outcomes measured in January 2006.

Estimates from two probit regressions are presented in Table 3. Columns 2 and 3 include the estimates from the regression explaining participation in the TAA program, while columns 4 and 5 include the estimates from a regression explaining participation in the training component of the TAA program; the latter results are discussed on page 25. Most of the coefficient estimates from the regression explaining participation in the TAA program are statistically significant and of the expected sign. For example, coefficient estimates confirm that individuals are more likely to participate in the TAA program the higher the import penetration ratio of their industry, while the degree of intraindustry trade in the industry reduces the likelihood of participation. Individuals are more likely to participate in the TAA program the higher the unionization rate and the lower the average wage in their industry. Parameter estimates confirm the results of the summary statistics that younger and more educated workers are less likely to be TAA beneficiaries than other displaced workers. Finally, individuals are more likely to be TAA participants if they are from states with higher unemployment rates or were displaced in 2003.

The second specification uses leave-one-out cross-validation to select the set of conditioning variables, a methodology suggested in Black and Smith (2004). In this method, we start with a minimal model containing only two conditioning variables. We then add blocks of additional variables and compare the resulting mean-squared errors of the models, choosing the model that best predicts the effect of the TAA program.

The problem with computing the model's mean-squared error is that we do not observe the missing counterfactual for TAA participants, their employment outcome if they had chosen not to participate in the TAA program. Leave-one-out cross-validation solves this problem by using the observations in the comparison group (the CPS workers) to compute the mean-squared error.[23]

The leave-one-out cross-validation method suggests that we should use a specification with significantly fewer variables. Specifically, specification 2 includes only the age, import penetration ratio, union and tenure variables, along with squares of the later two variables. Although we do not present the results from this specification in the paper, coefficient estimates were qualitatively similar to those from specification 1.

B. Matching

There are a number of potential matching estimators that researchers can use. Each estimator constructs an estimate of the expected counterfactual (i.e., the employment outcome of the treated individual if they had chosen not to participate in the TAA program) by taking a weighted average of the employment outcomes of the comparison group. The estimators differ in the weights assigned to each comparison group observation matched to the treated individual. Although asymptotically each method should produce the same results, in small samples the results can be quite different.

To ensure that our results are not being driven by the matching algorithm, we present results from two alternative matching algorithms. First, we match each TAA participant in the sample with a CPS counterpart using the nearest neighbor matching method. In this method, each TAA participant is matched with the displaced worker from the CPS sample that has the closest propensity score. We use nearest neighbor matching with replacement of the control observations; in other words the same CPS observations can serve as the match to more than one TAA participant.[24]

Next, we present results from a local linear matching estimator with an Epanechnikov kernel. Like kernel matching, local linear matching is a nonparametric matching estimator that calculates the missing counterfactual outcome by taking a weighted average of employment outcomes from virtually all of the comparison observations. The weights are a function of the difference in the propensity score of the comparison observation and the treated individual.[25]

We use the leave-one-out cross-validation method described above to choose the optimal bandwidth. As will be seen in the next section, both matching estimators result in qualitatively similar results, although a leave-one-out cross-validation comparison of the two models suggests that the local linear matching performs better (results in a lower mean square error) when compared to the nearest neighbor algorithm.

The final decision the researcher must make is how to ensure that the common support

23. To compute each specification's mean square error using the leave-one-out cross-validation method, we first estimate propensity scores for each observation in the comparison group (in this case, displaced workers from the CPS) using the probit coefficients from the proposed specification. We then drop the ith observation from the comparison group and match this observation with one or more (depending on the matching technique selected by the researcher) of the remaining $N - 1$ observations in the comparison group to estimate the ith observation's employment outcome. In other words, the estimated employment outcome for the ith observation is a weighted average of the employment outcomes of all of the comparison observations matched with the ith observation, where the weights are a function of the difference between the propensity score of the ith observation and that of the matched observation. The error is the difference between the outcome of the ith observation and this estimated employment outcome, while the mean square error is the average of these errors over all N observations in the comparison group. The result essentially represents an out of sample forecast error.

24. In specifications not presented here, we tried matching each TAA beneficiary with more than one CPS counterpart. We also used a variant of the nearest neighbor matching, caliper matching, in which we exclude those matches in which the difference in propensity scores exceeds a threshold set by the researcher. Results from specifications with oversampling and caliper matching were not significantly different from those presented here.

25. Local linear matching tends to perform better than kernel matching when comparison observations are distributed asymmetrically around the treated observation, like at boundary points or where there are gaps in the propensity score. While kernel matching can be thought of as a weighted regression of the employment outcomes of the comparison observations on an intercept term, local linear matching can be thought of as a weighted regression of these outcomes on an intercept term and the propensity score.

TABLE 4
Covariate Balancing after Matching, TAA Participants versus Nonparticipants[a]

	Means after Matching		% Bias	% Bias
	TAA Participants	CPS Controls	Before Matching	After Matching
Male	0.555	0.586	−14.2	−6.2
Ln (age)	3.819	3.811	19.8	3.1
Ln (years education)	2.498	2.482	−43.8	−2.4
Education (highest level)				
High school graduate	0.640	0.559	45.5	16.8
Some college	0.134	0.199	−47.9	−15.3
BA+	0.050	0.071	−29.1	−7.0
Tenure (in years)	9.470	9.633	11.5	−1.7
Import sensitivity	0.328	0.333	18.2	−3.0
Year growth imports	0.089	0.092	2.7	−3.9
Intraindustry trade	0.669	0.657	3.7	4.3
Industry unionization	0.113	0.119	1.0	−4.8
Average industry wage	14.213	14.235	−6.9	−0.8
State unemployment rate	0.060	0.059	41.6	12.6
Year 1993	0.834	0.772	56.8	13.7
Mean standardized bias[b]	6.825	—	—	—
Pseudo r^2	0.027	—	—	—
F-test	332.64	—	—	—

[a]This table reports covariate balancing statistics using the probit specification reported in column 1 of Table 3 and local linear matching with an Epanechnikov kernel and a bandwidth of 0.9. A common support is imposed by dropping 10% of the treated observations at which the density of the propensity score density of the control observations is the lowest.

[b]Mean standardized bias has been calculated as an unweighted average of all covariates. The standardized bias is calculated as $100 \times (\overline{x}_{\text{tapr}} - \overline{x}_{\text{cps}})/\sqrt{\text{Var}_{\text{tapr}}(x) + \text{Var}_{\text{cps}}\ (x)/2}$.

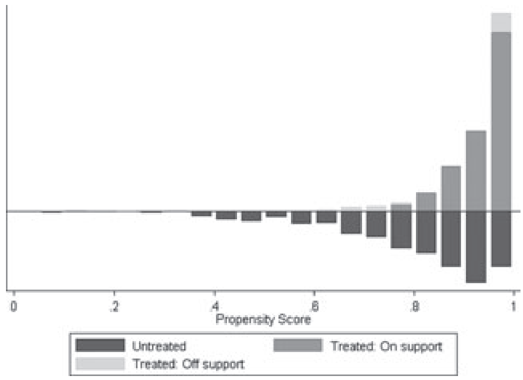
assumption holds. As explained in Caliendo and Kopeinig (2008), researchers typically use one of the two methods. The first, the minima and maxima comparison, deletes all observations whose propensity score is smaller than the minimum and larger than the maximum in the opposite group. In our case, this amounts to eliminating the 124 TAA participants whose propensity scores were higher than the highest propensity score observed in the CPS comparison group. The second method, known as trimming, eliminates a specific percentage of treated observations whose propensity scores lie in regions where the density of the propensity scores of the comparison observations is the lowest. We choose to present results from both methods, choosing in the later case to trim 10% (or 501 observations) from the TAA sample.

Before estimating the impact of the TAA program, we assess how well the matching procedure has been able to balance the distribution of covariates in the TAA treatment and CPS control groups. As suggested by Rosenbaum and Rubin (1985), we assess the matching quality using the standardized bias of the covariates. The standardized bias for each covariate is the difference between the sample means in the TAA treatment group and the matched comparison CPS control group as a percentage of the square root of the average of the sample variances in both groups. Another method of assessing the matching quality is the pseudo r^2. Ideally, the pseudo r^2 should be high prior to matching but low after matching; in other words, after matching there should be no systematic difference in the covariates in the TAA and CPS samples, and the covariates should have little explanatory power.

Table 4 includes the weighted mean values of the observable characteristics from the TAA and *matched* CPS comparison samples, as well as the standardized bias in these variables before and after matching.[26] Although there is no standard measure of how to determine whether the two samples are balanced, Rosenbaum and Rubin (1985) suggest that a standardized bias greater than 20 should be considered "large." As can be seen from Table 4, prior to matching six of the covariates had a standardized bias greater

26. Table 4 shows covariate balancing statistics following the local linear matching procedure. Statistics from other matching procedures are available from the authors upon request.

FIGURE 1
Distribution of Propensity Scores



Notes: Propensity scores following estimation of the probit regression of the likelihood of participating in the TAA program as reported in columns 2 and 3 of Table 3. Observations not on the support include the 10% of the TAA participants whose propensity scores lie in the region where the density of the propensity scores of the comparison observations is the lowest.

than 20, including all of the education variables and the state unemployment rate. Matching reduces the bias for all covariates to below 20; the mean standardized bias across the covariates is reduced from 24% to just 6.8%. The pseudo r^2, which was 0.1609 in the original probit specification, is just 0.027 after matching.

Figure 1 shows the distribution of propensity scores for both the TAA and CPS comparison samples. Both the covariate balancing statistics and the distribution of propensity scores in the two samples suggest that the matching procedure does a good job of balancing the covariates in the two samples, making the observations as similar as possible in observable characteristics.

VI. IMPACT OF THE TAA PROGRAM

Using the matching specification discussed in Section V, we estimate the effect of the TAA treatment on two postdisplacement job outcomes: (1) ability to find reemployment and (2) the ability to replace lost wages. We use the TAPR data to create an indicator variable (*Work*) that measures whether the TAA participant was employed in any of the three quarters following their exit from the program. The displaced worker survey from the CPS includes slightly different postdisplacement employment information. CPS workers were asked in January 2006 whether or not they had worked for pay since they were displaced, as well as how long they were unemployed after displacement and how many jobs they had held between displacement and the time of the interview. We define the indicator variable (*Work*) to equal one if the CPS worker indicated that they had worked since displacement in any of these questions.

Columns 2 and 3 of Table 5 present our estimates of the average effect of the TAA program on the ability of participants to find new employment. A naïve comparison of the probability of reemployment would suggest that workers who participated in the TAA program were 6.1 percentage points *less* likely to obtain reemployment, as reported in the first row. As discussed earlier, this difference is likely driven by differences in the employment prospects of the two samples rather than the impact of the TAA program itself. In comparison, none of the propensity score matching specifications using the full probit specification find any statistically significant impact of the TAA program on the likelihood that beneficiaries will find new employment.

The second specification suggested by the leave-one-out cross-validation tests, which controls for fewer observable characteristics, finds that on average the TAA program reduces the likelihood that the participant will find new employment by anywhere from 3% to 4%. Despite the fact that the minimal probit specification results in a lower mean-squared error when compared to the full specification, like Black and Smith (2004) we would be hesitant to say that these propensity score matching results are more accurate than those from the full specification. The minimal specification was basically chosen to maximize the model's goodness of fit, without taking into consideration which variables impact both TAA participation and employment outcomes. As a result, it is harder to believe that the CIA holds. Thus, these results likely suffer from more selection bias than those presented in the first column.

Although our results are not directly comparable to earlier studies of the TAA program, Marcal (2001) estimated a logit regression in which the dependent variable was the proportion of months since layoff (36 months) that the worker was employed. She found that workers who participated in TAA training programs were employed 9% more than non-TAA participants, while TAA beneficiaries who chose not to participate in a training program were employed

TABLE 5
Propensity Score Estimates of the Effects of the TAA Program[a]

	Probability of Reemployment		Wage Change	
	Specification 1	**Specification 2**	**Specification 1**	**Specification 2**
Not matched	−0.061* (0.019)	—	−0.120* (0.045)	—
Nearest neighbor matching with replacement (trimming)	−0.021 (0.033)	−0.042** (0.024)	−0.061 (0.067)	−0.101* (0.039)
Local linear matching, Epanechnikov kernel (trimming)	−0.027 (0.022)	−0.032** (0.018)	−0.048 (0.050)	0.102* (0.033)
Local linear matching, Epanechnikov kernel (common support)	−0.027 (0.021)	−0.039* (0.018)	−0.049 (0.046)	−0.094* (0.033)
Number of observations	TAA (5,348) CPS (454)		TAA (3,821) CPS (283)	

[a]Standard errors in parentheses.

*,**indicate those average treatment effects significant at the 5% and 10% levels, respectively. Propensity scores are estimated using the probit model reported in column 1 of Table 3. Bandwidths, chosen by minimizing the mean square error of the model using leave-one-out cross-validation, are 0.9 in columns 1 and 2, 0.3 in column 3, and 0.6 in column 4.

3% less than those in the comparison sample; the latter result was statistically insignificant.

It is possible that our results are being driven by remaining differences between the TAA and CPS samples that we are unable to control for. First, the results may still suffer from selection bias due to unobservable differences between the two samples. For example, because we do not observe the local labor market of CPS workers we cannot control for labor market conditions more specific than the state unemployment rate. It is possible that workers who participate in the TAA program are more likely to be in geographic areas that are experiencing a surge in unemployment, making it more difficult for workers to find new jobs.[27]

In addition, Heckman, Ichimura, and Todd (1997) note that it is important for data on the treated and comparison observations to come from the same questionnaire. Unfortunately, the only source of information on TAA participants is the TAPR, which does not include comparison observations. Differences in the definition of the *Work* variable in the TAA and comparison samples could bias our results of the average treatment effect. Consider two workers displaced in 2004. The TAA worker is classified as reemployed if he or she worked for pay at any time between April and December 2005. The CPS worker is classified as reemployed if he or she worked for pay at any point between the time of displacement in 2004 and January 2006. It seems more likely that the CPS worker will report having worked for pay at some point over the two or more years since displacement than the TAA worker will report having worked for pay in the 9 months since they exited from the TAA program. In this case, the propensity score matching procedure would underestimate the positive impact of the TAA program on the ability of beneficiaries to find new employment.

We next turn to the average effect of the TAA program on wage replacement. The TAPR dataset includes the quarterly earnings of participants in each of the three quarters prior to their displacement and the three quarters following their exit from the TAA program. We calculate the wage change of each worker as the percentage change in their calculated weekly wage from the third quarter prior to displacement to the third quarter following exit from the TAA program. The CPS displaced worker survey includes information on weekly wages prior to displacement, and current weekly wages in January 2006. Like the TAPR workers, we calculate the wage change of each CPS worker using the percentage change in the two values.

27. We observe the local area of employment for only a small subsample of metropolitan workers in the CPS comparison group. In specifications not reported here, we control for the local area unemployment rate in the subsample of workers displaced from metropolitan areas. The results from these specifications also indicate that the TAA program has no statistically discernible impact on the employment outcomes of participants.

Estimates of the effect of the treatment on the change in weekly earnings are provided in the last two columns of Table 5. Note that there are fewer observations when compared to the employment analysis because we observe weekly earnings in only a subset of observations. The results should be interpreted as the change in weekly earnings due to the TAA program for those workers who were reemployed. Note that we reestimated the propensity score for the subsample of workers who were reemployed to ensure that the TAA and CPS comparison samples continued to have observable characteristics as similar as possible.[28]

As illustrated in the first row of the table, the naïve estimate of the impact of the TAA program would suggest that those who participate in the TAA program experience an extremely large loss of weekly earnings; the wage loss of TAA participants is 12 percentage points greater than that of nonparticipants. After controlling for covariates using propensity score matching techniques, we find no statistically significant difference in the wage loss of the TAA beneficiaries when compared to their matched counterparts. In other words, the TAA program has no statistically significant impact on the wages of beneficiaries. This result mirrors the result in Marcal (2001), who also found an insignificant impact of the TAA program on wages after she controlled for selection bias.

Once again, the second, more minimal specification suggested by the leave-one-out cross-validation tests finds that the TAA program actually has a negative impact on the ability of participants to replace their predisplacement wages. Estimates suggest that on average the wage loss of TAA participants is about 10 percentage points greater than that of nonparticipants. However, we believe that because the minimal specification fails to control for key observable characteristics that impact both TAA participation and wages, these estimates continue to suffer from selection bias.

In summary, we find no evidence that the TAA program has a positive impact on the employment outcomes of the average participant. Although our results could still suffer from selection bias, they are strikingly similar to the findings of Marcal (2001), which evaluated the effectiveness of the TAA program between 1988 and 1989.

28. Although not reported here, coefficients from the propensity score estimation and balancing statistics are available from the authors upon request.

VII. DOES TRAINING MAKE A DIFFERENCE?

A natural hypothesis is that TAA participants are more likely to find new employment if they take advantage of program-funded training opportunities. In the two studies of the impact of training on the employment outcomes of TAA beneficiaries, both Marcal (2001) and Decker and Corson (1995) found that workers who participated in the training component of the TAA program had better employment outcomes than those who chose not to participate.

More generally, there is a large literature devoted to the evaluation of various training programs, although the conclusions of these studies have by no means been identical or overwhelming in support of more funding for such programs. For example, Heckman, LaLonde, and Smith (1999) conclude in their review of the literature that government-funded training programs may increase the probability of reemployment, but have only a modest positive impact on earnings. They also find that while formal classroom training appears to help women, men gain little from such training. A more recent meta-analysis of 97 studies conducted between 1995 and 2007 by Card, Kluve, and Weber (2009) found that classroom and on-the-job training programs are more likely to yield favorable medium-term than short-term results.

We estimate the ATE of training on TAA beneficiaries using the propensity score matching technique described in Section IV; the TAA beneficiaries who do not participate in training serve as the control sample. As we did with the evaluation of the TAA program as a whole, we estimate two specifications of the propensity score—the first a full specification motivated by what observable characteristics should impact a worker's decision to participate in a TAA-funded training program and a second specification with covariates suggested by leave-one-out cross-validation. We also present results from two matching techniques, nearest neighbor and local linear matching, and two methods of ensuring that the common support assumption holds.

Columns 4 and 5 of Table 3 present the coefficient estimates associated with the probit regression explaining the likelihood of a TAA participant also participating in a TAA-funded training program. We include most of the same variables as discussed in Section V. In addition, we include the number of claimants in the mass layoffs that occurred in worker's industry during their year of displacement (*Industry Layoffs*),

TABLE 6
Covariate Balancing after Matching, Training Participants versus Nonparticipants[a]

	Means after Matching		% Bias	% Bias
	Training Participants	No Training	Before Matching	After Matching
Male	0.565	0.574	13.1	−1.7
Ln (age)	3.826	3.822	−33.6	1.6
Ln (years of education)	2.475	2.512	34.4	−7.6
Education (highest level)				
High school graduate	0.620	0.638	−0.8	−3.6
Some college	0.142	0.141	19.7	0.4
BA+	0.062	0.072	0.5	−4.3
Tenure (in years)	9.433	8.932	−11.1	4.8
Import sensitivity	0.334	0.328	16.2	3.7
Year growth imports	0.090	0.090	42.6	−0.4
Intraindustry trade	0.668	0.645	−11.7	8.1
Industry unionization	0.114	0.120	−14.7	−4.3
Average industry wage	14.052	13.871	−31.6	6.3
Industry layoffs	0.107	0.107	25.2	4.6
State unemployment rate	0.060	0.059	44.4	11.6
Mean standardized bias[b]	4.478	—	—	—
Pseudo r^2	0.009	—	—	—

[a]This table reports covariate balancing statistics using the probit specification reported in column 3 of Table 3 and local linear matching with an Epanechnikov kernel and a bandwidth of 0.8. A common support is imposed by dropping 10% of the treated observations at which the density of the propensity score density of the control observations is the lowest.

[b]Mean standardized bias has been calculated as an unweighted average of all covariates. The standardized bias is calculated as $100 \times (\bar{x}_{\text{train}} - \bar{x}_{\text{notrain}})/\sqrt{\text{Var}_{\text{train}}(x) + \text{Var}_{\text{notrain}}\ (x)/2}$.

which we collected from the Bureau of Labor Statistics' Mass Layoff Statistics database. We believe that workers displaced from jobs in industries with a high number of layoffs may be more likely to engage in training to obtain employment in a new industry.

Many of the estimated coefficients are significant and of the expected sign. For example, we find that workers who already have a high school diploma are less likely to participate in TAA-funded training programs when compared to high school dropouts, and those with a college degree are even less likely to participate in training. Younger individuals with more tenure with their employer are more likely to participate in TAA training programs, as are men when compared to women TAA beneficiaries. Workers from states with higher unemployment rates, or that were displaced from industries with a higher number of mass layoffs, are more likely to participate in training.

The second specification, chosen by leave-one-out cross-validation and not reported here, includes a subset of these variables: gender, age, and years of education (but not the dummy variables for high school graduate, some college, and college graduate).

Table 6 presents covariate balancing statistics following the local linear matching procedure. As described in Section IV, we use an Epanechnikov kernel and use leave-one-out cross-validation to choose the optimal bandwidth. The matching technique significantly increases the degree of similarity of the two samples. Prior to matching, a number of the observed characteristics had standardized biases greater than 20, including the participants' age and education level, as well as the state unemployment rate and average industry wage of the average participant. Matching reduces the bias of all observable characteristics; after matching, the maximum standardized bias is slightly under 12, while the mean standardized bias falls to 4.5. Results suggest that the matching procedure does a good job of creating a comparison sample of individuals who have similar characteristics as those TAA beneficiaries who choose to participate in a TAA-funded training program.

Columns 2 and 3 of Table 7 present the propensity score matching estimates of the average effect of TAA-funded training programs on

TABLE 7
Propensity Score Estimates of the Effects of Training[a]

	Probability of Reemployment		Wage Change	
	Specification 1	**Specification 2**	**Specification 1**	**Specification 2**
Not matched	0.210* (0.014)	—	0.084* (0.032)	—
Nearest neighbor matching with replacement (trimming)	0.103* (0.025)	0.048 (0.039)	0.093* (0.047)	−0.005 (0.050)
Local linear matching, Epanechnikov kernel (trimming)	0.123* (0.019)	0.157* (0.017)	0.089* (0.034)	0.084* (0.031)
Local linear matching, Epanechnikov kernel (common support)	0.125* (0.019)	0.151* (0.017)	0.106* (0.034)	0.093* (0.030)
Number of observations	Training participants: 3,944 Nontrained: 1,074		Training participants: 3,195 Nontrained: 626	

[a]Standard errors in parentheses.
*indicates those average treatment effects significant at the 5% level. Propensity scores are estimated using the probit model reported in column 3 of Table 3. Bandwidths, chosen by minimizing the mean square error of the model using leave-one-out cross-validation, are 0.8 in column 1 and 0.7 in columns 2, 3, and 4.

program beneficiaries. As noted in row 1, the naïve (unmatched) estimate suggests that participating in a training program increases the likelihood that a TAA beneficiary will find new employment by 21% when compared to those who do not participate in training. The propensity score matching procedure also finds a positive impact of training on the reemployment prospects of beneficiaries. Estimates suggest that on average participating in a TAA-funded training program increases the likelihood that beneficiaries will find new employment by 10–12 percentage points.

The results from the second specification of the propensity score equation suggest a slightly higher average effect of 15 percentage points; as discussed earlier, we believe the first specification gives more accurate results. Our results, particularly from the first specification, are similar to those found in other studies. Marcal (2001) found that TAA beneficiaries who participated in training were employed 6% more than those who did not engage in training.

We next investigate whether participating in TAA-funded training opportunities increases the wages of TAA beneficiaries. When we control for worker characteristics using the propensity score matching technique, we find that training reduces the average earnings loss by TAA beneficiaries by somewhere between 8 and 10 percentage points when compared to those beneficiaries who receive a training waiver. Again, this is fairly similar to other studies of the TAA program; Marcal (2001) found that training participants earned 9% more than those that did not participate in training.

It is possible that these results are being driven by differences between the training and nontraining participant samples that we are unable to control for. Recall that although TAA beneficiaries must participate in training in order to receive extended unemployment benefits, nearly 20% of TAA participants receive a waiver from the training requirement. Program administrators are allowed to grant waivers for a wide variety of reasons, including the health, age, and skill level of the worker. Waivers are also granted to workers who can prove that training is unavailable in their area. Although we control for such characteristics as the age and education level of the participant, we do not have information on other characteristics such as the health status or the local labor market conditions of the participant. It is likely that workers in poor health would be both more likely to receive a waiver and more likely to remain unemployed. Moreover, workers in small rural areas may be limited in both the number of training and the number of new employment opportunities. Nevertheless, our results seem to indicate that TAA participants who qualify for a waiver would significantly benefit from training.[29]

29. Although it would be informative to analyze whether our results are sensitive to the reason for the training waiver, we are unable to conduct such analysis because officials failed to record a reason for the waiver in 62% of observations.

VIII. CONCLUSIONS AND POLICY IMPLICATIONS

This paper uses data collected by the Department of Labor in their Trade Act Participant Report (TAPR) database to provide a statistical evaluation of the effectiveness of the TAA program. Summary statistics indicate that TAA beneficiaries have a much harder time finding new, well-paying jobs when compared to other displaced workers. As GAO (2001) notes, TAA beneficiaries tend to be older and less educated than other workers, thus they have a harder time reentering a workforce that increasingly requires more skills and training. These facts suggest that there may be an important role for the TAA program to play in helping those workers most at risk following displacement.

Unfortunately, we find no statistical evidence that the TAA program improves the average employment outcome of beneficiaries over a comparison group of nonbeneficiary displaced workers with characteristics similar to those workers in the TAA program. Our results imply that while the TAA program may provide an income safety net, it does not help the average displaced worker who is enrolled in the program find new, well-paying employment opportunities. To answer the question posed in the title of this paper, our initial results indicate that the TAA program does *not* make a difference.

Upon further examination, however, we find strong evidence that those workers who participate in a TAA-funded training opportunity are more likely to obtain reemployment, and at higher wages, when compared to the TAA beneficiaries who do not participate in training. Specifically, participating in the training component of the TAA program increases the likelihood that the average TAA beneficiary will find new employment by 10–12 percentage points, and reduces the earnings losses of the average worker by 8–10 percentage points, when compared to a group of similar TAA beneficiaries who do not participate in the training component. Although the income support, job and relocation payments, and other TAA benefits may not help workers find new, well-paying employment, training seems to improve employment outcomes for these workers.

Policy makers should take these results into consideration when making future decisions regarding the TAA program. Although program rules require beneficiaries to participate in a training program in order to receive increased unemployment benefits, almost one-quarter of TAA beneficiaries are able to opt out of training after receiving a waiver from program administrators. Administrators should consider reducing the number of waivers from training requirements given to participants in order to improve employment outcomes. Moreover, at least 10% of the workers who received a training waiver did so because training was unavailable in their area. As reported in GAO (2001), government officials in areas with large numbers of TAA beneficiaries have admitted that they need to "improve local educational systems, which often had high school dropout rates much higher than the national average." Investing more in educational or vocational training programs in the disadvantaged areas that currently have few training opportunities may improve the efficacy of the TAA program.

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