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Background

It may often be the case that policymakers or program administrators are presented with conflicting or seemingly conflicting[1] evidence about the effectiveness of a program. One study may report that the impact of the program is small or not significantly different from zero; another study may report significant impacts. I will use the term conundrum to describe this situation. Compounding the difficulty that is posed by a conundrum may be the fact that the policymaker or program administrator does not have the technical training to fully dissect the seemingly conflicting studies to ferret out the "truth."

It is in interest of both the analysts who conducted the study and the policymakers to try to resolve conundrums. Absent careful investigation into the reasons for conflicting results, policymakers are likely to "believe" the results that most closely conform to their priors. Analysts should be dissatisfied with this behavior because it is antithetical to the scientific method. Furthermore, policymakers should be dissatisfied with this behavior because erroneous policy decisions may be made if priors were wrong.

The purpose of this paper is to try to bring evidence to bear on the following two conundrums:

- Summary articles seem doubtful about the effectiveness of federal workforce development policy, and yet there is considerable evidence that seems to show substantial success; and
- Studies using very similar data and similar methodology come to different conclusions about the efficacy of training that is provided through federal workforce development policy, especially for dislocated workers.

These issues have some urgency. Congress is in the process of reauthorizing the Workforce Investment Act (WIA), and evaluative evidence should enter the debate. Furthermore, the way that the current program directs individuals into training is contentious. A number of policy "white papers" are recommending that training be more widely available, as opposed to being made available only to individuals who do not become employed with core/intensive services.

[1] For brevity purposes, in the rest of the paper, I will use the term conflicting to mean actually conflicting or seemingly conflicting. The latter may refer to results that are quite different, but whose differences are not statistically significant or to results that are generated by methodologies that are potentially biased in opposite directions.

Perhaps more important than the policy direction in the reauthorization is simply providing the best possible program services to the people who need assistance because of barriers to employment and/or because of becoming unemployed. The present recession has swollen the ranks of the unemployed, and as long as it is efficient or equitable, federal policy needs to provide assistance.

Evidence about the Effectiveness of WIA (or JTPA)

A number of studies have evaluated public workforce development programs in the U.S., and several authors have attempted to summarize the results of these evaluations. Summary articles seem to be consistent in their faint praise or condemnation. Friedlander, Greenberg, and Robins (FGR) (1999) note,

> The broadest generalization about the current knowledge of government training programs for the disadvantaged is that they have produced modest positive effects on employment and earnings for adult men and women that are roughly commensurate with the modest amounts of resources expended on them. …Moreover, they have failed to produce positive effects for youth.

Barnow and Smith (2008) conclude,

> …most employment and training programs have either no impact or modest positive impacts. Many do not pass careful social cost-benefit tests, though some that fail may be worth doing on equity grounds. Existing evaluations have important analytic limitations that bias them in favor of programs with short-term impacts and large spillover effects on non-participants via displacement or price changes.

The GAO (1996) says,

> Although our statistical analysis showed some positive effects of JTPA in the years immediately following training, we found no significant effect of JTPA on earnings or employment rates after 5 years.

Yet, a number of studies using different data sets and different methods have found reasonably sanguine results for government workforce development programs. Mueser, Troske, and Gorislavsky (2007) use several quasi-experimental approaches to estimate the impact of JTPA on adults in the state of Missouri, and their preferred specification results in an earnings impact of about 14 percent for men and 23 percent for women. Heinrich, Mueser, and Troske (2008) report a significant quarterly earnings impact of WIA adult program services of about $580 for women and $400 for men (2005:Q1 $). For the WIA dislocated worker program, these authors report a significant quarterly earnings impact of about $350 for women and $310 for men[2]. Hollenbeck and

[2]Heinrich, Mueser, and Troske (2008) indicate that a difference-in-difference estimate for dislocated workers attenuates these impacts toward zero.

Huang (2006) found earnings impacts of about 16 percent for Washington State. Hollenbeck, Schroeder, King, and Huang (2005) report employment net impacts of around 12 percent and earnings impacts of about 18 percent in a study that covered seven states. Hollenbeck and Huang (2008) found earnings impacts of about 8 to 10 percent in Virginia, and Hollenbeck (2009) found similar impacts using Indiana data. As a point of comparison, these impacts are larger than the return that is estimated for a year of education. But WIA services tend to cost less and take much less time than education.

While it is generally agreed that a random assignment approach is methodologically superior to the matching estimators used in the above mentioned studies, it should be noted that the National JTPA Study (NJS) that used a random assignment process resulted in a 12 percent real earnings impact for adult men and women according to the U.S. General Accounting Office (1996).[3]

Interestingly, FGR (1999) themselves report hefty rates of return to public training programs. In their table 2, they display results from the NJS. In accord with the U.S. GAO study, they show positive and significant annual earnings impacts for adult men and women for overall services and for on-job-training (OJT). They show substantial positive earnings impacts, although not statistically significant, for classroom training for both men and women. But what is perhaps most surprising given the generalization cited above are the rates of return shown in this table. By their calculation, FGR use the NJS data to show real rates of return assuming that the mean effect lasts 3 years of 74%, 80%, and 70% for men for overall services, OJT, and classroom training, respectively and 41%, 94%, and <0% for women for the same treatments.

While it is true that very few of the studies that have examined the effectiveness of WIA or JTPA for youth have found positive impacts, it does seem that many studies—experimental and nonexperimental—find substantial impacts for adults; both men and women.

Evidence about the Effectiveness of Training through WIA (or JTPA)

It should be recognized, of course, that even though we refer to WIA (and JTPA) as federal job training programs, not all clients participate in training. In fact, with WIA, less than half of the clients participate in training. Several studies have attempted to estimate the net impacts of training for the clients who, in fact, do participate in training. Tables 1 and 2 summarize these studies for adult and dislocated workers, respectively. The tables display impact estimates for participants who received any service and for the subset of participants who received training. Note that the tables display published results, although the earnings impacts were inflated to 2008 $, and in some cases, the results come from interpolations of graphical data.

[3] I converted the earnings data presented in Appendix II, p. 20, to real terms. Then I calculated a difference-in-difference estimator for annual earnings two years prior to assignment and two years after assignment. For men, this impact is $784 (1989$) and the pre-program treatment group mean is $6,546 (1989$). For women, the impact is $471 (1989$) and the pre-program treatment group mean is $3,892 (1989$).

Table 1 Estimates of Training Impacts for JTPA/WIA Adults, by Study

Study/Outcome	Any service	Training	Training impact percentage	Percent trained
Study 1 (2003)				93.7
Employment	7.4	7.9	13.7	
Quarterly earnings	$697	$767	13.7	
Study 2 (2005)				54.3
Employment	8.7	4.4	na	
Quarterly earnings	$929	$836	na	
Study 3 (2006)				56.0
Employment	6.6	8.1	15.9	
Quarterly earnings	$504	$709	23.7	
Study 4 (2008)				28.6
Employment	6.5	5.5	9.1	
Quarterly earnings	$565	$782	33.2	
Study 5 (2009)				58.1
Employment	13.7	18.2	28.6	
Quarterly earnings	$463	$692	21.6	

NOTE: Earnings are in 2008 $. All entries, unless denoted with a † are significant at the 0.05 level. na = not available.
Study 1 is Hollenbeck and Huang (2003); Area: WA; Treatment: exit in '97/'98; Follow-up period: 8–11 quarters after exit.
Study 2 is Hollenbeck, Schroeder, King, and Huang (2005); Area: 7 states; Treatment: exit in '00/'02; Follow-up period: 2–7 quarters after exit.
Study 3 is Hollenbeck and Huang (2006); Area: WA; Treatment: exit in '01/'02; Follow-up period: 9–12 quarters after exit.
Study 4 is Heinrich, Mueser, and Troske (2008); Area: 12 states; Treatment: entry in '03/'05; Follow-up period: 11–14 quarters after entry.
Study 5 is Hollenbeck (2009); Area: IN; Treatment: exit in '05/'06; Follow-up period: 7 quarters after exit.

The first column of data in table 1 shows that participants in the adult titles of JTPA or WIA tended to receive substantial positive employment and quarterly earnings impacts. With exception of one study, the employment rate impacts are tightly banded between 6.5 and 8.7 percentage points, and the quarterly earnings impacts range from about $500 to $900 (2008$). The second column of data shows the impacts for those with training. In four of the five studies, the earnings impact is larger than for any services, and in three of the five, employment rates went up. Again, with the exception of the employment rate impacts in study 5, the results are fairly tightly banded. In short, it seems as though the nonexperimental studies suggest that encountering JTPA/WIA has a positive impact on adults, on average, and that receiving training results in slightly more positive impacts.

The results that are given in the first column of table 2 show that dislocated workers who receive services from JTPA/WIA also have substantial employment and earnings outcomes. There is a little more variation than in table 1, but all of the results are positive and significant. The second column shows that participants who received training had poorer outcomes than the average, however, unlike the adult results in the first table. All five studies show a reduction in the employment rate outcome, and four of the five show a reduction in quarterly earnings. One of the studies—Heinrich, Mueser, and Troske (2008) (HMT)—find that training for dislocated workers results in no employment or earnings gain.

Table 2 Estimated Impacts of Training Impacts for JTPA/WIA Dislocated Workers, by Study

Study/Outcome	Any service	Training	Training impact percentage	Percent trained
Study 1 (2003)				66.5
Employment	7.3	6.7	9.8	
Quarterly earnings	$598	$354	4.0	
Study 2 (2005)				57.5
Employment	13.5	5.9	na	
Quarterly earnings	$1,189	$483	na	
Study 3 (2006)				61.9
Employment	6.4	4.2	6.5	
Quarterly earnings	$855	$391	6.8	
Study 4 (2008)				31.5
Employment	6.8	1.4†	2.3†	
Quarterly earnings	$371	−$36†	−0.1†	
Study 5 (2009)				49.1
Employment	16.5	15.9	21.2	
Quarterly earnings	$310	$394	6.0	

NOTE: Earnings are in 2008 $. All entries, unless denoted with a † are significant at the 0.05 level. na = not available.
Study 1 is Hollenbeck and Huang (2003); Area: WA; Treatment: exit in '97/'98; Follow-up period: 8–11 quarters after exit.
Study 2 is Hollenbeck, Schroeder, King, and Huang (2005); Area: 7 states; Treatment: exit in '00/'02; Follow-up period: 2–7 quarters after exit.
Study 3 is Hollenbeck and Huang (2006); Area: WA; Treatment: exit in '01/'02; Follow-up period: 9–12 quarters after exit.
Study 4 is Heinrich, Mueser, and Troske (2008); Area: 12 states; Treatment: entry in '03/'05; Follow-up period: 11–14 quarters after entry.
Study 5 is Hollenbeck (2009); Area: IN; Treatment: exit in '05/'06; Follow-up period: 7 quarters after exit.

Tables 3 and 4 show results from three of the studies that reported net impacts separately by gender. In virtually every case displayed in those tables, women ended up with larger average impacts than men. Interestingly, in one of the studies other than HMT, the quarterly earnings for male dislocated workers is not significantly different from zero.

So the published evidence seems to suggest that encountering JTPA/WIA resulted in positive labor market impacts, on average, for disadvantaged adults, and that training "worked" for them. On the other hand, for dislocated workers, encountering JTPA/WIA had positive impacts, on average; although the subset of participants who received training did not benefit nearly as well. When analyzing results by gender, it is the case that women receive higher payoffs than men.

Variation in Methodologies

In conducting these studies, literally dozens of methodological decisions are made along the way. Some of the decisions are dictated by data availability or accessibility. Other decisions are made by the analysts. Table 5 lists some of the methodological choices made in the five nonexperimental studies listed in the initial tables. The table gives a sense of the variation across studies, but it still does not list all of the ways in which the studies vary. For example, the studies differ in the extent of and methods for editing the underlying data, and they differ in the specifications of the logit model used to estimate propensity scores.

Table 3 Estimates of Training Impacts for JTPA/WIA Adults, by Gender and Study

	Men				Women			
Study/Outcome	Any service	Training	Training impact percentage	Percent trained	Any service	Training	Training impact percentage	Percent trained
Study 2 (2005)								50.1
Employment	6.2	2.1	na	60.2	10.6	6.5	na	
Quarterly earnings	$856	$690	na	—	$983	$968	na	
Study 4 (2008)								31.7
Employment	5.8	2.0	na	24.2	7.0	7.5	na	
Quarterly earnings	$500	$525	na		$725	$1,062	na	
Study 5 (2009)								62.8
Employment	6.8	9.8			17.3	22.6		
Quarterly earnings	$240	$707			$488	$655		

NOTE: Earnings are in 2008 $. All entries, unless denoted with a † are significant at the 0.05 level. na = not available.
Study 2 is Hollenbeck, Schroeder, King, and Huang (2005); Area: 7 states; Treatment: exit in '00/'02; Follow-up period: 2–7 quarters after exit.
Study 4 is Heinrich, Mueser, and Troske (2008); Area: 12 states; Treatment: entry in '03/'05; Follow-up period: 11–14 quarters after entry.
Study 5 is Hollenbeck (2009); Area: IN; Treatment: exit in '05/'06; Follow-up period: 7 quarters after exit.

Table 4 Estimates of Training Impacts for JTPA/WIA Dislocated Workers, by Gender and Study

	Men				Women			
Study/Outcome	Any service	Training	Training impact percentage	Percent trained	Any service	Training	Training impact percentage	Percent trained
Study 2 (2005)								51.7
Employment	11.8	5.0	na	63.3	15.2	7.1	na	
Quarterly earnings	$1,119	$446	na	—	$1,260	$527	na	
Study 4 (2008)								33.1
Employment	6.0	−0.4†	na	29.8	7.5	2.2†	na	
Quarterly earnings	$347	−$78†	na		$392	$0†	na	
Study 5 (2009)								47.4
Employment	14.4	15.2			17.9	21.0		
Quarterly earnings	$32†	−$70†			$493	$793		

NOTE: Earnings are in 2008 $. All entries, unless denoted with a † are significant at the 0.05 level. na = not available.
Study 2 is Hollenbeck, Schroeder, King, and Huang (2005); Area: 7 states; Treatment: exit in '00/'02; Follow-up period: 2–7 quarters after exit.
Study 4 is Heinrich, Mueser, and Troske (2008); Area: 12 states; Treatment: entry in '03/'05; Follow-up period: 11–14 quarters after entry.
Study 5 is Hollenbeck (2009); Area: IN; Treatment: exit in '05/'06; Follow-up period: 7 quarters after exit.

Table 5 Methodological Variation by Study

	Study 1	Study 2	Study 3	Study 4	Study 5
Area	WA	7 states	WA	12 states	IN
Program	JTPA	WIA	WIA	WIA	WIA
Treatment/time frame	Exit from program in '97/'98	Exit from program in '00/'02	Exit from program in '01/'02	Enter program in '03/'05	Exit program in '05/'06
Source of comparison group	ES	ES	ES	ES/UI claimants	ES
Comparison group – Adult/DW	Matched ES records	Matched ES records	Matched ES records	Matched ES/UI records	Matched ES records
Training	Matched ES records	Matched core/ intensive or ES records	Matched ES records	Matched core/ intensive records	Matched ES records
Match technique	Propensity score w/ replacement; caliper	Block matching; weighted multivariate matching; propensity score	Propensity score w/ replacement; caliper	Log-odds of propensity score with replacement; many-to-1 radius	Propensity score w/ replacement; caliper
Follow-up period	8–11 quarters	2–7 quarters	9–12 quarters	11–14 quarters	7 quarters
Estimator – Adult	Regression-adjusted difference-in-difference	Weighted combination of techniques	Regression-adjusted difference-in-difference	Linear-adjusted levels	Regression-adjusted levels
– DW	Regression-adjusted levels	Weighted combination of techniques	Regression-adjusted levels	Linear-adjusted levels	Regression-adjusted levels

NOTE: Study 1 is Hollenbeck and Huang (2003); Study 2 is Hollenbeck, Schroeder, King, and Huang (2005); Study 3 is Hollenbeck and Huang (2006); Study 4 is Heinrich, Mueser, and Troske (2008); Study 5 is Hollenbeck (2009).

In the remainder of this paper, I discuss the various approaches that are noted in the table and provide evidence and/or speculate about how the approach may influence the estimated outcomes.

Business cycle. The impacts that I am focusing on from these studies are labor market outcomes, and would thus be affected by the relative strength of the economy at the time of participating in the program. There is some disagreement about how the business cycle affects training outcomes. Theoretical arguments can be made either way. One might suspect that when unemployment is relatively high, the pool of the unemployed includes relatively higher-skilled and more motivated individuals suggesting more positive outcomes. On the other hand, a soft labor market makes it more difficult to find employment after being trained.

Empirically, Greenberg, Michaloupus, and Robins (2003), using U.S. data, find "no evidence that a higher unemployment rate makes training more effective, except possibly at very high levels of unemployment." Hamalainen (2002), using Finnish data, finds that the effectiveness of labor-market training appeared to be negatively related to overall unemployment. Finally, using a 10-year panel of German administrative data, Lechner and Wunsch (2006) suggest that programs are more effective when unemployment is higher at the time when the individuals enter.

Assuming that the latter study holds true, then we would expect that the results from study 2 and 3 would be relatively stronger than the other studies. The U.S. experienced a relatively long expansion in the 90s and between the end of 2001 to the end of 2007, and a relatively sharp, but short contraction in 2001. Thus program entry and exit would have been in expansionary period in studies 1, 4, and 5; whereas program entry would have been in a soft labor market in studies 2 and 3.

Treatment point. Four of the studies defined treatment as having exited from a program in a given time period, whereas the other study defined treatment as entering a program in a given time period. This seems like a substantial difference. When using exiters, the issue is framed as once an individual has encountered a program and received some services,[4] how do they fare in the labor market compared to an individual who resembled them prior to entering the program, but did not receive services. When using entrants, the issue is framed as how do individuals who have experienced similar circumstances ultimately fare in the labor market if one receives services from a program and the other doesn't. An analogy might be a running race, such as a half-marathon, and the treatment is some form of special coaching. Using exiters is tantamount to taking a runner at time t with certain characteristics and providing him or her with the special coaching (which they may or may not complete), and then starting a race after the coaching session against another runner who has the same pre-coaching characteristics. Using entrants is tantamount to starting the race at time t between two matched individuals, and then giving the coaching to one of the individuals. The obvious difference between the two approaches is that in using entrants, the short-term results will reflect the fact that the individual is participating in the program ("being coached"), but longer-term outcomes should be similar in the two approaches.

I attempted to find out empirically whether the alternative treatment points altered results. Using one state's data from Hollenbeck, Schroeder, King, and Huang (2005), I was able to re-estimate the net impacts based on matching using exit quarter and based on matching using entry quarter.[5] Table 6 displays the results of this test. Note that the impacts in the table are post-exit outcomes; they are identically estimated. There do seem to be systematic differences in the results, but for one treatment, the results increased in magnitude when using entry point rather than exit point, and for another treatment they were smaller. For almost all of the outcomes and groups presented in the

[4] Note that exiters include individuals who do not complete.

[5] With two consecutive years of exits, I was able to retrieve entry quarter information for over 80 percent of the sample.

Table 6 Estimates of Net Impacts of Training Using Different Treatments

	Matching on exit		Matching on entry	
	Any service	Training	Any service	Training
		Males		
Adults				
Employment (%)	11.9	12.3	12.6	11.6
Earnings ($)	133.1†	297.5	281.5	84.8
Dislocated workers				
Employment (%)	10.9	9.9	13.4	12.1
Earnings ($)	227.9	205.8	456.6	478.0
		Females		
Adults				
Employment (%)	8.4	10.2	12.3	9.4
Earnings ($)	393.7	402.9	375.8	324.9
Dislocated workers				
Employment (%)	8.7	11.1	14.8	10.6
Earnings ($)	140.6	272.9	341.7	255.9

NOTE: † not significant at the 0.05 level. Note entries in the table are mean differences between treatment and comparison groups for non-zero employment and earnings from all available data for 2nd full quarter after exit and beyond. Estimates generated from data from one of the states in Hollenbeck, Schroeder, King, and Huang (2005). Earnings in 2000$.

table, the matching on entry quarter estimates are larger for "Any service" and smaller for "Training." I could not come up with a reasonable explanation for this anomaly. And indeed, in looking across other estimators not reported in the table, it seemed as though the differences in results were not systematic.

If we could capture data about individuals at the time that they decide that they may need assistance from a public training program, i.e, before they apply for services, and do the statistical matching at that point, then it would be better to "start the experiment" then rather than waiting for the individual to exit from services. In this case, the evaluation would capture the average treatment effect (ATE), which is the mean effect over the entire potential population. Matching at the exit point would capture the local average treatment effect (LATE), which is the experimental effect for individuals who would choose to be treated if the treatment were offered. In general, the LATE is greater than the ATE because the latter includes individuals who ultimately do not apply for services. But the administrative data that are available for JTPA and WIA are only available for individuals who apply for services, and so matching at point of entry or at exit point should both result in LATEs, and any differences are random.

Source of comparison group. In most of the studies, the comparison group is a subset of the Employment Service registrants. However, in one of the studies, unemployment insurance (UI) claimants were used as the superset from which the comparison group was selected. A question that might be raised is whether UI claimants would be an appropriate source for the comparison group because unemployment insurance is intended for individuals who were involuntarily separated from their jobs. That is, individuals who are fired or who quit voluntarily are not eligible for benefits. Furthermore, monetary eligibility for UI requires that workers be employed for a long enough period to meet minimum quarterly earnings requirements. So even if workers

were involuntarily separated, they may not have worked long enough to qualify for benefits. If individuals know that they are not eligible for UI, they may not apply and therefore would not be in the comparison group pool. On the other hand, these are not eligibility requirements for WIA services, so it may be the case that UI claimants have more skills and labor market attachment than the treatment group. If that is the case, and if these characteristics are not completely controlled in the matching process, then the study that used UI claimants may underestimate the labor market impacts of the treatments.

Comparison group for training. As noted in table 5, in order to estimate the effect of training per se, some of the studies matched individuals who had participated in WIA and received training services (again may or may not have completed) to individuals who had participated in the program, but only received core or intensive services. In other words, these studies did two statistical matches - - one to estimate the net impacts from receiving any services and the second to estimate the impact of training. Other studies did not do a second match, but rather used the subgroup of individuals in the treatment group from the first match who had participated in training and *their* matched observations. Both approaches have disadvantages.

The problem with the "two-match" approach is that the basic design of WIA is to provide services sequentially. The intent of the program is that individuals will apply for services and receive basic, core services (assessments and job leads). If these services do not seem to be adequate for the individual as assessed by WIA staff members, then the individual may be given intensive services (job search assistance, referrals to other agencies to overcome barriers, etc.). Finally, if the individual is still unable to become employed after core and intensive services, they may be referred to training services and given a voucher (individual training account). In short, if the design of the program is followed, the individuals who receive training are those who didn't succeed with only core/intensive services. By statistically matching individuals who received training to those who received only core/intensive services, the studies that use this approach are trying to identify the "pure" effect of training. The treatment group will have received core/intensive services and training; the comparison group will have just received the core/intensive services; and observable differences between the two groups would be controlled by the statistical match. Unfortunately, there are strong reasons to believe that there are unobservable differences between the two matched groups that will bias the treatment effect toward zero. Basically, the argument is that the members of the comparison group were successful and were not referred to training, whereas the treatment group members were not.

The problem with the other approach is that training is not disentangled from core/intensive services. In other words, in this approach, individuals who received core, intensive, and training services are compared to individuals from the Employment Service who did not participate in WIA at all.

Matching technique. The studies that provide impact estimates for WIA use several different techniques for conducting statistical matching. A considerable literature

has arisen concerning the various empirical techniques used in nonexperimental evaluations (see the February 2004 Review of Economics and Statistics collection of papers and the many studies referenced there.[6]) The general theme of this literature seems to be that there are many different econometric techniques for estimating program effectiveness that have appropriate asymptotic properties. Some papers in this literature go on to speculate about which estimators seem to work best under which conditions. In Hollenbeck (2007), I report the results of a sensitivity analysis of many different matching techniques. In general, the results were quite robust to technique. Based on this work, I am confident that the program results that are being reported in the various studies are not biased based on matching technique.

Estimator. Once a study has derived a comparison group through a statistical matching process for nonexperimental approaches or has derived a control group through a random assignment process, the question of appropriate estimator needs to be addressed. In general, estimators can be differences in means between the treatment and comparison (or control) groups, or they can be regression-adjusted difference in means. Studies usually have sufficient pre-program data, so that two other estimators could be generated: difference-in-difference in means or regression-adjusted difference-in-differences.

All of the studies discussed reported regression-adjusted estimates.[7] This simply recognizes the facts that the matching variables are limited to observed characteristics and that the populations can be considered to be random samples. Thus differences in characteristics may occur randomly and need to be controlled. An advantage of longitudinal data is that it allows a difference-in-differencing technique that partially controls for unobservable characteristics (specifically those unobservable factors that are person-specific and time-invariant.) However, regression-adjustments to difference-in-difference estimators requires the assumption that the theoretical model explaining outcomes does not change between the pre-program and post-program periods of time. This is a strong assumption, especially for dislocated workers.

Thus, I believe that the preferred estimators are regression-adjusted difference-in-differences for WIA adults and regression-adjusted post-program outcome levels for dislocated workers.

Conclusions

Despite the rather modest, if not gloomy, summaries about federal job training programs, i.e, WIA (or JTPA), recent evidence seems to provide fairly sanguine impact estimators for labor market outcomes for adults and dislocated workers. Studies that were quite varied in many technical aspects all report statistically significant positive

[6] One of the articles in that collection, Michalopoulos, Bloom, and Hill (2004), addresses the question that is central to this paper, namely the advisability of using administrative data for program evaluation purposes.

[7] Hollenbeck, Schroeder, King, and Huang (2005) use a meta-analytic estimator that is essentially a weighted average of differences in levels and regression-adjusted differences in levels.

outcomes for the treatment labeled "any service." (For the most part, studies find little evidence of the efficacy of WIA youth programs, although in some studies, there are statistically significant outcomes for youth.) The conclusion seems to be that individuals who encounter these programs and receive some services have positive post-program labor market outcomes, on average. The Workforce Investment Act, as administered, seemed to work. Two important caveats need to be mentioned. First, as just mentioned, the results are fully conditional on how the programs were administered. WIA has not provided states with nearly enough resources to serve all individuals in need. So if the individuals who received services were systematically different from the general population who might have benefited (programs may have "cream-skimmed" for example), then the positive outcomes may not be externally valid, i.e. not generalizable. Second, all of the results discussed here have been for the average participant. The distribution of those outcomes has not been examined. So there may be groups of participants who do not benefit.

Turning to results about training, the studies reported in table 1, seem to indicate that training is effective for individuals in the Adult title of WIA (or JTPA). However, the results displayed in table 2 suggest that the dislocated workers who received training end up with labor market benefits that are much lower than the average program participant. The Heinrich, Mueser, and Troske (2008) study finds that the labor market impacts for trained dislocated workers are indistinguishable from zero. It should be noted, however, that this study used UI applicant data to derive comparisons groups in most states and it used core/intensive service recipients as the comparison group. Arguably, both of these study characteristics will tend to attenuate impacts. However, in considering the training results, the same two caveats as those mentioned above hold--the results may not be externally valid and the results may not be evenly distributed across the population. Furthermore, it should be recognized that dislocated workers tend to have substantial labor market histories with relatively high earnings. Thus when they lose their jobs, they will have considerable foregone earnings so that even if the impacts from training are positive, they are unlikely to cover the workers' implicit investment costs. WIA policy makers may wish to consider strengthening their support mechanisms for dislocated workers. For example, stipends may be called for.

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