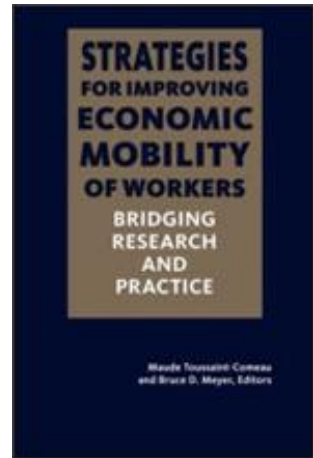

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What We Know About the Impacts of Workforce Investment Programs

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10

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This chapter briefly reviews the recent literature that seeks to evaluate employment and training programs, as well as important older papers. We focus on the question of whether the programs have measurable and economically relevant impacts on labor market outcomes.

We do not focus on the economics of such programs but do lean on the “dismal science” when interpreting the findings in the literature. We also do not focus on the econometrics of program evaluation, though our views about the credibility of various combinations of econometric strategies and data affect our choice of which evaluations to highlight and how we interpret the overall literature.

Readers interested in more in-depth surveys of the substantive literature should consult Heckman, LaLonde, and Smith (1999). Smith (2000, 2004) provides a relatively nontechnical guide to the evaluation literature, while Abbring and Heckman (2007); Angrist and Krueger (1999); Friedlander, Greenberg, and Robins (1997); Heckman, LaLonde, and Smith (1999); Heckman and Vytlacil (2007a,b); and Imbens and Wooldridge (forthcoming) provide technical overviews.

EVALUATIONS OF THE MAJOR U.S. FEDERAL PROGRAMS

Employment and training programs in the United States have a relatively brief history. In addition to the public employment programs of

the Great Depression, the Manpower Development and Training Act (MDTA, 1962–1972), the Comprehensive Employment and Training Act (CETA, 1973–1982), the Job Training Partnership Act (JTPA, 1982–1998), and the Workforce Investment Act (WIA, 1998–present) have provided vocational training, along with remedial education, subsidized on-the-job training, and job search assistance to disadvantaged youth and adults as well as displaced workers. CETA also provided public service employment.

Perry et al. (1975) review the literature on the MDTA. Except for Ashenfelter (1978), this literature largely reflects the nascent stage of evaluation methodology at the time. The U.S. Department of Labor (USDOL) funded a number of evaluations of the CETA program, all of which relied on the same data source, the Continuous Longitudinal Manpower Survey (CLMS), which combined random samples of participants with nonexperimental comparison group data from the Current Population Survey (CPS) and included matched calendar year Social Security earnings data for both groups. Barnow (1987) summarizes these nonexperimental evaluations, which relied largely on crude matching estimators or difference-in-differences strategies, and obtained widely varying estimates. The sensitivity of the difference-in-differences estimates in the CETA studies to the choice of the “before” period foreshadows a similar finding in Heckman and Smith (1999). Despite the high-quality (but only annual) administrative outcome data, the CLMS lacked the detailed information on local labor markets found to be important in Heckman et al. (1998) as well as the information on recent labor market and program participation choices (at a fine level of temporal detail) found to be important in Card and Sullivan (1988); Dolton, Azevedo, and Smith (2006); and Heckman et al. (1998).

The wide variety of CETA estimates led to a decision by the USDOL to evaluate the JTPA using a social experiment, called the National JTPA Study (NJS), which operated at a nonrandom sample of 16 (of about 600) local JTPA sites from approximately November 1987 to September 1989. Doolittle and Traeger (1990) describe the details of the experiment, and Bloom et al. (1997) and Orr et al. (1996) present the results. The NJS included disadvantaged adults and out-of-school youth but not in-school youth and dislocated workers.

The U.S. General Accounting Office (USGAO 1996) provides impact estimates for five years after random assignment based on Social

Security earnings data. The USGAO finds stable impacts of around \$800 a year for adult (22 and older) men and women, but these impacts lose statistical significance over time. In contrast, the estimates for male and female youth remain near zero throughout the follow-up period. The NJS found substantial treatment-group nonparticipation (around 40 percent) and control group substitution (also around 40 percent) into alternative providers of similar services. As a result, these estimates approximate (because of differences in service intensity between the treatment and control groups) what Imbens and Angrist (1994) call local average treatment effects: average impacts on those who receive services if assigned to the treatment group but who would not have received JTPA services if assigned to the control group. Heckman, LaLonde, and Smith (1999, Table 20) show that JTPA produced a net social benefit for adults but not for youth, generally irrespective of (reasonable) assumptions about benefit duration beyond five years, the discount rate, or the welfare cost of taxation.

Mueser, Troske, and Gorislavsky (2007) employ modern matching methods, as described in, for example, Smith and Todd (2005), combined with relatively rich administrative data, to estimate the earnings impact of JTPA in Missouri for program years 1994 and 1995, using a comparison group of individuals registering with the Employment Service. In real terms, their preferred estimates resemble those from the NJS.

Finally, although the WIA program has been operating nationwide since July 2000, there exist no published econometric evaluations. In 2008, the USDOL funded a random assignment evaluation of WIA.

EVALUATIONS OF SELECTED OTHER U.S. PROGRAMS

Job Corps

Job Corps, established in 1964, provides intensive and comprehensive services, including vocational and academic activities as well as support services, to about 60,000 disadvantaged youth, ages 16–24, in 119 residential centers. The program has had two major evaluations: a thoughtful, nonexperimental evaluation in the 1970s, summarized

in Long, Mallar, and Thornton (1981), and an experimental evaluation in the 1990s, summarized in Schochet, Burghardt, and McConnell (2006). The two have remarkably parallel findings; we focus on the experiment.

The first key finding is that removing disadvantaged young men from their local neighborhood dramatically reduces their criminal behavior in the short run. Second, there is a notable effect on educational attainment in the short run, measured in terms of hours, literacy and numeracy, and GED and vocational certificate receipt. Third, the Job Corps program generates substantial sustained earnings impacts for 20- to 24-year-old participants, but not for younger participants. As a result, because of its high cost, the program does not come close to passing a cost-benefit test (which includes the impacts on crime) for younger participants but does come close for the 20- to 24-year-olds. Despite the lack of an efficiency justification for the program, at least for the 20- to 24-year-olds it actually has a substantial impact on labor market outcomes, which puts it well ahead of many other youth programs, such as JTPA, where the impacts equaled approximately zero.

Worker Profiling and Reemployment Services

The Worker Profiling and Reemployment Services (WPRS) system assigns mandatory reemployment services to new Unemployment Insurance (UI) claimants predicted to have long spells of UI receipt or high probabilities of UI benefit exhaustion. A desire to proactively serve UI claimants likely to exhaust their benefits early in their benefit spells, rather than waiting to serve them until after they have experienced a long spell, motivates the program. The WPRS poses two separate evaluation problems. First, what effect do the mandatory services have on those who receive them and, second, how well does the existing system, which is based on predicted labor market outcomes in the absence of the mandatory services, do at allocating such services?

We know of two evaluations that address the first question. Dickinson, Decker, and Kreutzer (2002) summarize the results of a larger project that includes linear selection-on-observables estimates of the impact of WPRS referral on weeks and amount of UI received as well as earnings and employment for six states. They find substantively important and statistically significant impacts on the UI variables but

no systematic effects on labor market outcomes; this suggests that the WPRS system reduces UI usage without imposing a large cost on referred claimants via lower-quality job matches, although neither does the program provide any benefits to the recipients.

More recently, using data from Kentucky and exploiting the particular institutional features of the profiling system in that state, Black et al. (2003) provide experimental evidence of the impact of the re-employment services requirement on claimants who are on the margin for the service requirement, given their employment histories and local area characteristics. They find that the program has a substantial effect relative to its (very small) cost, with that effect consisting largely of a deterrent effect, whereby some claimants immediately find employment upon receiving notice of the requirement that they receive services.

Black et al. (2003) also address the second question, and they find little difference in the impacts by profiling score. Keeping in mind the relative imprecision of their estimates, this suggests that the existing allocation mechanism does not advance economic efficiency. Pope and Sydnor (2007) argue that the existing mechanism fails on normative grounds as well, though their argument hinges critically on the view that the WPRS treatment represents a burden rather than a benefit.

EMPLOYER-FOCUSED PROGRAMS

Although it might sound obvious that workforce programs should focus on the labor demand side as well as the labor supply side, until recently there has been a disproportionate emphasis on the latter. In this section we briefly review the literature on three approaches to employer-focused programs: on-the job training (OJT), customized training, and sectoral training.

On-the-Job Training

Subsidized on-the-job training (OJT) at private firms dates back at least to MDTA. This service provides a (typically 50 percent) wage subsidy for a limited period (typically six months) to firms hiring and informally training certain specified types of workers. Program staff

members recruit firms to provide OJT positions (a time-consuming task), and firms always retain the right to reject candidates prior to hiring and to dismiss workers during or after the subsidy period. Though the training provided is supposed to exceed that provided to other new workers, anecdotal evidence strongly suggests that OJT recipients often receive the same training as unsubsidized workers (and, in some cases, little or no training at all).

Subsidized OJT has several rationales. The wage subsidy component seeks the purely redistributive goal of getting employers to try out workers who may appear more risky because of weak labor market histories or other problems. As the OJT participants are not considered regular employees, employers are more willing to risk hiring them because if the OJT participants are let go at the end of the OJT period, it is not the same as terminating a regular worker. Tying training by the firm to the wage subsidy aims to increase the skills of workers lacking the resources or credit to obtain training either directly from providers or indirectly from firms via lower wages (where the minimum wage may also limit the ability of workers to trade lower wages for training).

Most evaluations suggest positive impacts of OJT on participant employment and earnings. For example, Barnow's (1987) review of the CETA evaluations finds OJT to have greater impacts than all other service types. The NJS provides suggestive evidence on this point as well. However, OJT impacts likely embody more displacement than impacts for classroom training and other services that focus exclusively on increasing human capital and not also on redistributing jobs. As a result, partial equilibrium estimates like those noted here do less well at capturing the impacts relevant for a social cost-benefit calculation.

Customized and Sectoral Training

Customized training is defined as training characterized by 1) employer input and approval authority for the curriculum, 2) employer authority to establish eligibility criteria for participants and to select participants if the employer desires, and 3) a commitment by the employer to hire successful program completers. Sectoral training projects consist of customized employment and training services provided to a group of employers in the same industry or sector of the economy; see, e.g., Dresser and Rogers (1998) and Elliott and King (1999) for discus-

sions. Though program advocates enthuse about these programs, they do so without good evidence regarding their impacts.

Sectoral programs, like OJT, have the potential to provide opportunities for human capital enhancement to disadvantaged workers who might be overlooked by employers. To warrant government support, more evidence is needed on their effectiveness in increasing earnings, and care should be taken to ensure that the training is provided to workers who ordinarily would not be trained at employer expense. Thus, we recommend that rigorous evaluations be conducted to determine whether these programs produce earnings gains that exceed their (full social) costs. We further recommend that programs be structured so that workers who receive the training have labor market disadvantages, and so that the training is general in nature and useful at other firms in addition to the one hiring the workers.

ANALYTIC ISSUES

This section highlights the four most important analytic issues in the literature.

The first concerns heterogeneity in the effects of active labor market policies. This heterogeneity arises in part from the fact that programs themselves often provide quite heterogeneous services under headings such as “classroom training.” The substantial differences across groups defined by sex and age in average treatment effects, noted earlier in the chapter, strongly suggest that even relatively homogeneous services will have varying effects across individuals as well. In such an environment, evaluation researchers must pay close attention to exactly what treatment effect their analysis estimates, and policy analysts must take care to link the estimates they consider to the policy questions of interest. For example, an experiment with no control-group substitution estimates the mean impact of “treatment on the treated.” This mean impact represents the correct impact estimate for a cost-benefit analysis that seeks to address the question of either keeping or scrapping the existing program. It does not provide the correct impact estimate for an analysis of whether the program should receive a larger budget so as to allow it to expand the set of persons served; a simple economic

model of program participation in which those with the largest impacts choose to participate suggests that average impacts for individuals on the margin of service receipt will lie below the mean impact of treatment on the treated.

Second, many studies do not even attempt a cost-benefit analysis, and those that do often provide relatively low-quality analyses, either because of lack of required inputs or failure to follow the best practices outlined in the literature. Without a serious cost-benefit analysis, even a relatively strong positive impact estimate has little to say about policy. Without data on all relevant outcomes (as when relying solely on administrative earnings data for outcomes when programs may also affect, say, criminal behavior and health), policymakers end up making decisions based on incomplete information about impacts. Many government programs lack even rudimentary information on either average or marginal program costs, let alone detailed information on the marginal and average costs for particular services and client types. Finally, as noted in Heckman, LaLonde, and Smith (1999), many cost-benefit analyses fail to take full account of the costs of tax funding by omitting consideration of the marginal excess burden of taxation, and proceeding instead as if a dollar of tax funding costs society only a dollar.

Third, most evaluations estimate impacts over relatively short periods from the time of service initiation or random assignment. Recent evidence indicates the dangers this poses to correct inferences about program value. In the negative direction, the early positive impacts found in the National Job Corps Study turned out to largely fade away when longer-term follow-up data became available. In the positive direction, classroom training sometimes takes several years to yield its full impact, as in the long-term follow-up of the California GAIN program by Hotz, Imbens, and Klerman (2006) and the long-term evaluation of German classroom training by Lechner, Miquel, and Wunsch (2004). At the same time, the long-term follow-ups of the Supported Work experiment by Couch (1992) and of the JTPA experiment in USGAO (1996) show that sometimes program impact estimates remain rock solid at the level observed shortly after program participation. With only a handful of studies that provide credible impact estimates more than two or three years out (this paragraph lists nearly all of them), we cannot draw any conclusions about program types or client characteristics associated with particular patterns of long-term impacts.

Fourth, and finally, only a handful of papers look seriously at general equilibrium effects. Put differently, most evaluations ignore the effects that programs may have on the behavior of those who do not participate in them. In addition to indirect effects working through the tax system, these include displacement effects, whereby individuals induced to search harder (or smarter) by a program, or whose skills increase as the result of a program, take jobs that would otherwise have gone to individuals not participating in the program. Programs can also have price effects; for example, a program that produces large numbers of trained auto mechanics or nurses' aides should drive down wages in those labor markets. In many cases, failing to take account of general equilibrium effects leads to overly positive conclusions about program performance.

Calmfors (1994) and Johnson (1980) provide early conceptual discussions of these issues. The small but growing empirical literature includes Davidson and Woodbury (1987), who find modest but not trivial displacement effects of UI bonuses in a search context. Heckman, Lochner, and Taber (1999) find large price effects of a subsidy to university tuition, effects that imply that a partial equilibrium analysis wildly overstates the enrollment effects of the subsidy. Lise, Seitz, and Smith (2006) consider the Canadian Self-Sufficiency Project, which provided a generous earnings subsidy to some welfare recipients, and find that taking account of displacement and changes in the amount of effort applied to searching by those without the subsidy changes the sign of the cost-benefit calculation for the program. Finally, Kabbani (2001) finds evidence using data from the NJS that training programs may increase the earnings of nonparticipants by moving the participants into a different labor market.

CONCLUSION

First, 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 spill-

over effects on nonparticipants from displacement or price changes. In general, employment and training programs work best for adult women and least well for youth. The literature provides no good explanation for this demographic pattern.

For reasons of space we have omitted a variety of topics, such as recent studies that examine program design by looking at performance management systems (Barnow and Smith 2004; Heckman, Heinrich, and Smith 2002), at the efficacy of caseworkers (Bell and Orr 2002; McConnell, Decker, and Perez-Johnson 2006), and at statistical treatment rules as an alternative to caseworkers (Eberts, O’Leary, and Wandner 2002; Lechner and Smith 2007). We have also omitted some program categories, such as welfare-to-work programs (Ashworth, Cebulla, Greenberg, and Walker 2004; Bloom, Hill, and Riccio 2003) and the Trade Adjustment Act as well as all evidence from outside the United States (Betcherman, Olivas, and Dar 2004; Kluve 2006). The general lessons from the omitted literature parallel those from what we have covered.

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