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John T. Addison

Principles of Market-Oriented Labor Market Policies

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Karl-Marx-Str. 67, D-14482 Potsdam, Germany, Tel.: (0)331-9774614, Fax: (0)331-9774631
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Abstract

In this paper, we examine the development of employment policy in western nations with a view to providing some insights for the transformation economies. Primary emphasis is accorded the United States because of the diversity of that nation's manpower programs and its long-standing attempt to evaluate their impact, although European policies are also addressed. An introductory statement of the case for active labor market policies is followed by a review of the mainstream U.S. training measures and their evaluation. If controversy surrounds many of the conventional instruments, a more positive gloss can be given other measures such as early childhood interventions and certain reforms of the unemployment insurance system. The wider issues raised by the U.S. experience are next summarized, prior to a review of the European evidence. Lessons for the transition economies are addressed in a concluding section.

Zusammenfassung

In diesem Beitrag untersuchen wir die Entwicklung der Arbeitsmarktpolitik in westlichen Ländern mit dem Ziel, daraus Erkenntnisse für Transformationswirtschaften zu gewinnen. Das Hauptaugenmerk liegt dabei primär auf den Vereinigten Staaten aufgrund der breiten Palette unterschiedlicher Arbeitsmarktprogramme in diesem Land und dessen langer Evaluierungsgeschichte im Hinblick auf die Wirkungen der Programme. Auf ein Eingangsstatement zur aktiven Arbeitsmarktpolitik folgt ein Überblick zu den wichtigsten US-Förderprogrammen und ihrer Evaluierung. Wenn auch viele der traditionellen Instrumente kontrovers diskutiert werden, so erscheinen andere Maßnahmen wie Eingriffe in die frühkindliche Erziehung und bestimmte Reformen des Systems der Arbeitslosigkeitsversicherung in einem besseren Licht. Weitergehende Aspekte, welche die Erfahrungen in den Vereinigten Staaten aufwerfen, werden als nächstes zusammengefaßt, bevor ein Überblick über den europäischen Forschungsstand gegeben wird. Schlußfolgerungen für Transformationswirtschaften werden am Ende des Beitrags gezogen.

John T. Addison
Department of Economics
The Moore School of Business
University of South Carolina
Columbia, S.C. 29208
U.S.A

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1. Introduction

Gradually in the western industrialized nations we have seen a movement toward active labor market policies and away from measures that simply support the individual's job search while offering a modicum of relocation assistance. The new emphasis is on training, job subsidies, and job search policies. To be sure, there has also occurred a reduction in the generosity of income maintenance – either in unemployment benefit replacement rates and/or entitlement periods – imparted by periodic deregulatory thrusts, but such developments have been distinctly secondary in importance to the development of proactive labor market policies.

The immediate backdrop to active labor market policies offering skills acquisition and reemployment assistance is of course the deteriorating position of less skilled workers as a result of (biased) technological change and heightened international trade. Such forces have led to falling relative (even absolute) wages of unskilled workers and, in those circumstances where wage flexibility is constrained, to rising levels of unemployment and greater unemployment persistence. The United States is typically cast as an example of the former case and continental Europe as illustrative of the latter. More generally, active labor market policies have been advocated as a means of addressing actual and prospective threats to competitiveness.

As a practical matter, however, despite the widespread popularity of manpower policy – as it used to be called – our understanding of the efficacy of such measures is still rather rudimentary. Much of what has been learned stems from the U.S. experience, because of that nation's long tradition of subjecting policy to appraisal and moving forward on the basis of demonstration projects. That said, account has also to be taken of regime differences between the United States and Europe and, within Europe, between advanced western nations and the transition economies. Fortunately, the sheer diversity of U.S. policy initiatives serves to cover many of the bases while the evaluations have general relevance.

2. The Case for Active Manpower Policy

Arguments in favor of active labor market policies have a basis in market failures and distortions. Among the more obvious distortions are unions, so-called 'efficiency wages,' and statutory or de facto wage minima. Minimum wages may prevent employers from employing low skill workers – or at least training them – while unions and efficiency wages may similarly elevate wage rates above market clearing levels. The (second-best) solution takes the form of skills training or subsidized employment.

In the same vein, capital market imperfections can be used to justify training programs (for relevant skills) because liquidity constraints can lead to underinvestment in human capital. In addition, it has been argued that excessive turnover and uncertainty lead firms to underinvest in both the general and firm-specific capital of their workers (the maintained hypothesis being that training is neither exclusively general nor specific)(see the essays contained in Booth and Snower, 1996). However, these underinvestment arguments are by no means uncontroversial in advanced market economies because of the lack of strong empirical support for credit constaints (see Cameron and Heckman, 1992) coupled with evidence that job shopping is a productive activity (Topel and Ward, 1992; Neumark, 1998).

Less controversial are the externalities associated with the public goods nature of labor market information. In principle, these externalities offer support for a state agency that collects and disseminates labor market information. The efficiency of the public job broking function is of course another matter (see the pessimistic evaluation in Addison and Portugal, 2000, and the empirical studies referred to therein). Externalities of a social nature also provide the basis for programs geared toward unskilled youth. Raising the income of such workers can mitigate crime and teenage pregnancy, and justify programs that do not pass a conventional benefit-cost test.

Relatedly, Heckman et al. (1998) have recently stressed the important role of dysfunctional families in the United States. The argument here is that some families do not provide adequate learning environments, thereby justifying policies aimed at disadvantaged children. The policies aim to improve the home environment of children, impart parenting skills, or provide alternative places for learning. This is by way of a long-term solution to tackling the skill deficit problem and is crucially linked to the empirical finding of a strong positive association between formal schooling and postschool training investments – the notion of universal complementarity (*vulgo*: skill begets skill). At issue is the point in the life cycle at which skill remediation fails.

Another justification for intervention arises from negative duration dependence, namely, the tendency for escape rates out of unemployment to decline with the spell length of joblessness. Abstracting from other imperfections, the tendency for unemployment duration to be time dependent does not by itself provide a basis for policy intervention since it may simply reflect a sorting phenomenon, whereby the least employable come to make up an ever larger share of the unemployment pooled. However, true duration dependence resulting from labor market "scarring" may give rise to inefficiencies. When unemployed individuals come to be ranked by employers on the basis of their spell length of unemployment, the better (among those workers separated for economic reasons) will not be able to signal their higher productivity and will be screened out in favor of employed job seekers and the shorter-term unemployed (see Blanchard and Diamond, 1994). Here a case can be made for direct job creation to break the cycle. True duration dependence can also arise from skill depreciation but the

policy implications are here altogether less transparent – in terms of both compensation and training – outside of situations where major regime shifts have occurred; the most obvious case in point being the change from a centrally-planned economy to a market economy.

In the above we have commented on the broad efficiency case for an active labor market policy. Equity considerations may be no less important – both goals are formally recognized in the U.S. Job Training Partnership Act (see section 3). An important issue in the analysis of manpower policy is the extent to which the two are in conflict. If so, they should be reflected in the design of policy; for example, it may be inefficient to train older workers but appropriate to subsidize their employment if not their human capital.

To varying degree, all the above considerations are relevant to the transformation economies – particularly the social externalities of poverty and unemployment and credit market imperfections – and are underwrittten by the requirement for different skills than those previously acquired. But as usual the devil is in the detail.

3. Active Labor Market Policy in the United States

U.S. active labor market policy can be traced back to the 1962 Manpower Development and Training Act (MDTA). The intention of the Act was to offer training to technologically dislocated workers, but the emphasis was to shift in favor of disadvantaged workers as a result of the Economic Opportunity Act of 1964. Thereafter, the MDTA targeted welfare recipients and low income youth via classroom training, referrals to vocational schools, and on-the-job training. A key component of the MDTA was the Job Corps – established in 1964 and ongoing – which offered training for disadvantaged youth. The distinguishing characteristic of the Job Corps was, and remains, the provision of an extensive range of program services in a residential setting.

MDTA was replaced by the Comprehensive Employment and Training Act (CETA) in 1973. CETA gave states and local authorities the authority to operate training and other programs, and provided them with federal grants. The legislation also introduced public service job creation in response to the economic downturn 1970-71. Through time the latter program was to expand and account for a little under one half of all manpower outlays.

The stance of public policy again shifted in 1982 with the passage of the Job Training Partnership Act (JTPA). The legislation retained and strengthened the

decentralized apparatus of CETA, giving primary responsibility for its implementation to the states, local authorities, and the business community. It also eliminated the public service employment component and generally reduced real outlays on the reemployment services offered the disadvantaged under Title IIA of the program. But the JTPA did include – under Title III – special assistance for dislocated workers, thereby supplementing the provisions of separate legislation on trade adjustment assistance.[1]

The Economic Dislocation and Worker Adjustment Assistance (EDWAA) Act of 1988 amended Title III of the JTPA. Under EDWAA, states were required to develop 'dislocated worker units' with the ability to react to major layoffs and plant closings, and help workers find and qualify for new jobs. Dislocated workers could receive retraining services (classroom, occupational skills, and/or on-the-job training), readjustment assistance (testing and counseling, job search and placement, supportive services to include child care and transportation allowances, and relocation assistance and pre-layoff assistance), as well as needs-related payments in the event of unemployment benefit exhaustion. As we shall see, the JTPA is in the process of being replaced by the Clinton administration's Workplace Investment Act.

The most important services offered disadvantaged workers under JTPA-Title IIA were classroom training in occupational skills plus basic education where this was adjudged deficient (44% of JTPA enrollees): job search assistance, covering instruction on how to locate jobs, prepare for interview, and write resumes (15% of enrollees); subsidized on-the-job training and work experience in private sector firms (15% of enrollees), providing up to 50% of the wage over 6 months; and short-term work experience in the public sector and not-for-profit organizations (6% of enrollees). But, as noted by LaLonde (1995, p. 154), training services varied markedly by demographic group. Thus, adults participating in classroom training were more likely to be females receiving vocational instruction, while youths were are more likely to be receiving remedial education. On-the-job training participants were disproportionately job-ready men, unlike those in work experience programs who were mainly adult females and youths without recent labor market experience.

Before addressing recent changes to mainstream manpower policy, it is important to note that welfare recipients, covered by the Aid to Families with Dependent Children (AFDC) program, have long been *mandated* to participate in separate employment and training programs. Under the 1967 Work Incentive program (WIN), such welfare recipients were provided with job search assistance. Subsequently, a work experience (i.e. workfare) component was added. WIN was superseded by the Job Opportunities and Basic Skills Program (JOBS) in 1988. This more comprehensive legislation was designed to extend the type of assistance offered AFDC recipients via classroom and job training; in practice, however, funding limitations meant that the degree of supplementation was modest.

The 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) replaced the JOBs program (and AFDC) with the Temporary Assistance for Needy Families (TANF) block grant. The TANF grants give states the flexibility to design their own welfare programs in circumstances where they require recipients to engage in work or work-related activities for their welfare checks (now limited to 5 years in total). Post PRWORA initiatives include the Welfare-to-Work Grants Program and a Welfare-to-Work Tax Credit. Both seek to create job opportunities for the hardest-to-employ TANF recipients. Under the former initiative, resources can be used for job creation, on-the-job training, job placement and post-employment services, and job retention and supportive services. For its part, the tax credit for each eligible worker hired is equal to 35% of qualified wages for the first year of employment and 50% for the second year. Qualified wages are capped at $10,000 per year.

The most recent variant of mainstream manpower policy is the 1998 Workforce Investment Act (WIA) (see Manpower Report of the President, 1999, pp.127-129; Manpower Report of the President, 2000, pp. 162-164). The legislation seeks to integrate/consolidate a variety of programs/services at the street level in so-called 'one-stop centers,' designed to make the delivery system more accessible to individuals and businesses. WIA also sets up self-directed 'individual training accounts.' These are supposed to allow workers more choice over their training and retraining. To facilitate this process, the legislation requires training providers to give information on their performance along the dimensions of job placement, earnings, and job retention.

The one-stop centers offer universal access to core employment services, including unemployment insurance.[2] About 1,000 such centers, funded by the federal government in partnership with the states, have already have been set up. Their performance is to be carefully monitored. That is, states and local authorities will have to meet performance standards, and are subject to sanctions for failure to meet job placement, earnings, and worker retention goals. Training providers for their part have to be certificated and a condition for their continued eligibility is their meeting performance standards issued by the local Workforce Investment Board (dominated by members of the business community) - information on which is then transmitted to the one-stop centers. Youth councils are also established under each WIB. The goal is to improve coordination among the various agencies serving youth and to provide 12-month follow up services in respect of each program.[3]

4. Evaluation of U.S. Employment and Training Programs

A distinguishing characteristic of the U.S. evaluations, apart from their sheer number, is that they have often been conducted on an experimental basis. That is, assessment proceeds on the basis of random assignment of individuals either to a treatment group, which is allowed access to the program or to a nontreatment group, which is excluded from it. In principle, random assignment ensures that the two groups do not differ systematically in any way other than in their access to the program, so that outcome differences between the treatment group and the controls can with more assurance be attributed to the program. Classical experiments of this nature get around the difficult problem of controlling directly for characteristics that affect program outcomes but cannot be observed. (In practice, as we shall see in the next section, the process of randomization is never complete, neither is it a panacea.)

The emphasis on experimental methods in the United States reflects that nation's preoccupation with reducing welfare rolls/dependence. As noted by Heckman et al. (1999, fn. 96), experiments proliferated in the 1980s after the federal government authorized states to operate as demonstration projects workfare programs (i.e. community work experience programs) for their welfare populations.

Following the literature (and, in particular, the surveys by LaLonde, 1995; Heckman et al., 1999), we shall first distinguish between experimental and nonexperimental studies of programs designed for these disavantaged workers before turning to examine the effects of policies geared to the nondisadvantaged dislocated worker population. We conclude with findings from experimental unemployment insurance (UI) initiatives and some 'early intervention' policies. Additional interpretation and caveats are remitted to section 5.

Disadvantaged Workers – Nonexperimental Studies

We begin by summarizing the results of nonexperimental studies of the early manpower programs – MDTA and CETA – that usually involve the construction of some control group having characterics similar to those of program participants (on which, see section 5).

Studies of MDTA and CETA indicate that, where successful, the programs raised subsequent earnings by between $1,000 and $2,000 per year (see LaLonde, 1995, Table 1, for 13 such studies). Given the modest cost of the programs – other than the Job Corps – earnings gains of this magnitude would, if sustained, imply a high rate of return on the investment. But most such studies usually follow program participants for only several years after the training event, so that much hinges on extrapolation.

No less important, gains of this order of magnitude are by no means observed across the board. Indeed, variability in impact is the hallmark of the nonexperimental literature, and not simply across cohorts but also within program cohorts and by the particular training service provided. Nonetheless, certain patterns are discernible in the studies. First, earnings improvement is consistently largest for disadvantaged adult females. For example, Ashenfelter's (1978) famous study of programs funded under MDTA reports earnings gains for white (nonwhites) adult females of $1,740 ($1,540) in the 12 months following the training event (1990 dollars). Second, although adult females always record earnings improvement, the same in not always true of adult males (see for example Kiefer, 1979; Dickinson et al., 1986). Where positive, earnings gains for adult disadvantaged males are always smaller than for their female program counterparts. Third, when one considers the results for disdavantaged youth it is typically found that exposure to program services typically *lowers* subsequent earnings. These findings are prima facie consistent with stigma effects attaching to program participation and/or opportunity costs in the form of sacrificed labor market experience. The principal exception is a study by Mallar et al. (1982) of the Job Corps, which points to large earnings gains (particularly for male youth) that persist for at least four years after the training event. Although substantial, these gains fall far short of covering the costs of the intensive Job Corps program, support for which thus hinges on a wider benefit-cost calculation emphasizing social savings from reduced criminal activity, the value of reduced use of other transfer and training programs, the value of output produced by participants during training, as well as earnings projections beyond the initial four-year follow-up interval. Not surprisingly, controversy attaches to each component magnitude (see LaLonde, 1995, p.164; Heckman et al., 1999, p. 2068).

Disadvantaged Workers – Experimental Studies

The experimental literature was in no small part generated by the substantial variance in outcome indicators reported in the nonexperimental studies. Detailed summary findings from the now substantial literature on the WIN/JOBS and JTPA programs, and separate initiatives such as the National Supported Work demonstration, are provided by Heckman et al. (1999, Table 22), who identify social cost and employment/earnings outcomes by program service and demographic group. Overall, the experimental studies yield smaller earnings gains than their nonexperimental counterparts but nonetheless suggest that access to program services can benefit both the individual and society.

As before, the strongest results are observed for adult females. Although annual earnings gains are typically modest, they tend to persist over the survey interval and to accrue over a variety of (incremental) program services, such as job search assistance, work experience, and classroom training/on-the-job-training. Furthermore, they are

often internally efficient. (The issue of deadweight losses and other external costs are discussed in section 5.) Some of the strongest evidence that access to training can improve the position of disadvantaged adult females comes from the National Supported Work (NSW) demonstration (see Hollister et al., 1984). The NSW operated from 1975-1979 and was designed to provide subsidized employment opportunities of 9 to 18 months' duration to trainees. It was targeted on long-term AFDC recipients (as well as ex-drug addicts, ex-criminal offenders, and disadvantaged youth). Adult females in the program experienced higher earnings than the controls for at least seven years after the training event ended (Crouch, 1992, Table 1). Earnings gains of similar magnitude are recorded for adult females under the more recent National JTPA study, with persistence over a 30-month post-training interval (see Bloom et al, 1992; Bloom et al.,1997).

The evidence from a smaller number of experimental studies pertaining to adult males is less conclusive, with some studies showing earnings gains vis-a-vis the controls and others pointing to zero or even negative impacts. Here the results are sensitive to the type of treatment offered. Thus, there is the suggestion that work experience does not lead to significantly higher earnings (e.g. the NSW demonstration). Rather, positive earnings effects appear to hinge on the provision of more intensive services such as job search assistance combined with on-the-job training if not classroom training (the National JTPA study).

Finally, there is no evidence from any U.S. experimental study that disadvantaged youth benefits from access to employment programs. Thus, the NSW demonstration indicates that none of the estimates of the treatments' relative earnings effects was statistically significant in the seven years following their exposure to lengthy work experience. Similarly, analysis of the JOBSTART demonstration that offering participants a wide range of program services (broadly replicating Job Corps services, but without the residential setting) fails to indicate earnings gains vis-à-vis the controls over a four-year follow up period (Cave and Doolittle, 1991). Similarly, the National JTPA provides no evidence that youths gained from any service strategy (classroom training, on-the-job training and/or job search assistance, or other services), this time over a 30-month follow-up period (Bloom et al., 1997).

Evaluations of Dislocated Workers

Reflecting the preoccupation of U.S. manpower instruments with disadvantaged workers, there is comparatively little information on the effects of training and other programs for dislocated workers. Most of the evidence comes from some early demonstration projects. This evidence, while pointing to clear gains to dislocated workers from their exposure to job search assistance, fails to detect any incremental

earnings advantage from the provision of additional services in the form of classroom training or even on-the-job training (see the survey by Leigh, 1990). More recent work suggests that the effects of some services, most notably classroom training, may have been more positive than first thought. Thus, for example, the New Jersey UI demonstration points to positive incremental effects of classroom training on participant earnings over a 10-quarter follow- up interval (Anderson et al., 1991).[4] Moreover, a study by Jacobson et al. (1994) reports that dislocated workers derive long-term earnings gains from completing rigorous vocational and academic community college level courses. As far as on-the-job training is concerned, however, there has been no real clarification of the disappointing results reported in the earlier literature.

Other Evaluations

We conclude this section with some results from U.S. unemployment insurance (UI) experiments and a very different literature on the effects of early intervention policies for the disadvantaged.[5] For their part, the UI experiments have taken two main forms: cash bonus and job search programs. Both have been carefully surveyed by Meyer (1995).

The first UI bonus program was operated in Illinois in 1984. It offered all claimants – irrespective of their previous earnings or level of benefits – a $500 bonus (about four times the then average weekly UI benefit).[6] To receive the bonus, participants had to start a job within 11 weeks of filing (the qualification period) and retain the job for over 4 months (the re-employment period). The apparent success of the Illinois program directly led to three other bonus experiments in Pennsylvania, Washington, and New Jersey, which were more varied. For example, the two former experiments used different combinations of benefit amount and qualification period, while New Jersey experiment combined the provision of the bonus with job search assistance (see above).

The Illinois experiment produced very favorable results. Specifically, mean weeks of benefits fell by 1.15 weeks for those offered the bonus, while their earnings were modestly higher in the third quarter after filing the UI claim and significantly higher over a 12-month period (consistent with more weeks being worked). The reduction in benefits was achieved without having to pay a large number of bonuses and thus produced savings to the UI system. Because earnings also increased, the benefit-cost calculation was also favorable to the government and society. However, the results of the other experiments were less compelling. All yielded a reduction in benefits for those assigned to the claimant treatment, but this amounted on average to just one-half a week (average compensated jobless spells of the controls ranged

between 15 and 20 weeks). As a result, there were mostly negative effects on the UI system. In addition, earnings gains among the treatment group were found in just one of the experiments, so that the costs to government and society were also broadly negative.

One reason for the dissonance in results between Illinois and the three follow-up experiments reflects the fact that its reemployment bonus effect combines a very large estimated effect for workers eligible for extended benefits (under the federal program) with a much smaller effect among workers eligible for the regular state funded benefits (Davidson and Woodbury, 1991). The former group appears to have disproportionately increased its search activity in response to the bonus payments. But any inference that such bonus payments might be of especial interest to continental European countries has to be qualified by the likelihood of 'displacement' effects, discussed in the next section.

Results of the UI job search experiments on the other hand are rather more favorable. There have been five such experiments – in addition to the hybrid New Jersey demonstration – each of which sought to intervene relatively early in the unemployment spell and, in varying degree, to provide enhanced job search assistance and more frequent checks of claimant eligibility. As noted, the effects of the experiments on weeks of benefits and earnings are reviewed in Meyer (1995). In all cases, the combination of job search assistance with enhanced enforcement of the job search requirement led to fewer weeks of unemployment benefits for the treatments. (Interestingly, in the case of the Washington experiment, one of the treatments eliminated all checks on work search, as compared with the traditional UI programs which required three employer contacts per week to justify continued receipt of benefits. For this treatment it was found that found that the duration of benefit receipt *increased* by 3.34 weeks.) At the same time as benefit duration decreased among the treatments – the magnitude being similar to that observed in the more costly bonus experiments – earnings actually increased for the more intensive approaches. While cautioning that the earnings effects are estimated with imprecision, Meyer concludes that program benefits exceeded costs from the perspectives of the UI system, the government, and society.

At issue, of course, is the relative contribution of job search assistance on the one hand and tighter enforcement of the work requirement on the other. To the extent that most treatments were a combination of both, it is impossible to determine the component magnitudes – a similar problem arises in evaluating the mainstream manpower policies reviewed earlier. It is important to make this determination for a number of reasons. For example, if better job matches dominate then it is less likely that we will observe displacement – namely, a reshuffling of the unemployed – and also more likely that employers will make greater use of the public employment service.

Another bright spot in the employment and training literature, now very broadly defined, is the apparent success of high quality early childhood interventions in yielding subsequent labor market benefits to the disadvantaged participant Heckman et al. (1998, Table 6) review ten of these programs. One such (experimental) study is the Perry Pre-school program in which disadvantaged subnormal children were randomly assigned to a treatment group (offered intensive high quality pre-school services for up to two years coupled with weekly home visits with parents) and a control group denied these benefits (see Schweinhart et al., 1993). Over the 22-year follow up period, the treatments not only achieved better test scores throughout but also higher high-school graduation rates (21%), lower grade retention (21%), reduced criminal activity (e.g. 2.3 lifetime arrests by age 27 as compared with 4.6 arrests for the controls), and considerably improved monthly earnings ($453 at age 27). Not surprisingly, the benefit-cost appraisal is highly favorable.

Here as elsewhere there is an issue of external validity since controversy attaches to the efficacy of the broad-based federal program Head Start (see, for example, Haskins, 1989). Nevertheless, the findings summarized in Heckman et al. do suggest that early intervention has considerable promise as a long-term solution to the labor market problems confronted by the disadvantaged.

5. Interpretation of the U.S. Evidence

Interpretation of the U.S. evidence has proceeded at the policy and technical levels. Dealing with policy first, one interpretation of the evidence would be that the benefits of employment policy have been modest at best and incapable of addressing the earnings and unemployment problems of low skilled workers. Opponents of manpower policy would focus on the low returns to conventional programs compared with private-sector training and argue that private sector training is broadly optimal to begin with or perhaps seek to encourage it through tax incentives and subsidies rather than through inefficient manpower programs. On this view, the only solution to skill deficits resides in early intervention – ensuring that students have a proper educational foundation – reflecting the complementarity between schooling investments and subsequent labor market investments, underwritten by the long period over which returns may be realized. A market solution would be no less relevant here, with a focus on school vouchers and charter schools rather than further infusions of monies into the public school system.

An alternative view would be that manpower policy has not yielded greater payoffs largely because public spending on low-income individuals has been modest, at

least in respect of the scale of the skill deficits that policy-makers are seeking to address. Note that this view does not imply immediate across-the-board increases in subventions but, rather, increased expenditures that are targeted toward programs with the greatest payoffs on the basis of what has been learned from the experimental and nonexperimental studies. Its proponents would therefore emphasize the gains made by disadvantaged adult females across a variety of treatments; gains that have persisted through time and often yielded societal benefits. But implicit in this view is the argument that large gains do require more expensive services, even if the content has yet to be determined on the basis of additional demonstrations (e.g. the jury being still out on the efficiency of the Job Corps). Presumably, this view would also see scope for wage subsidies to facilitate the employment of residual groups.

An intermediate view would see the U.S. experience as illustrating that helping the existing disadvantaged must always be an incremental process, and one that is buttressed by favorable economic conditions. Sustained economic growth in the United States has already produced a modest reversal of rising skill differentials and begun to redress in part the large deficits in earnings that manpower policy is supposed to address. Tight labor markets have also encouraged employers to revise their stereotypes and offer employment and training to those previously statistically screened out. Unfortunately, the appropriate use of active manpower policy in favorable states of nature has received inadequate attention from researchers.

In all of this there is little disagreement on the need for policies that tackle skill deficits prior to labor market entry. Although the methods to improve schooling are at issue, there is much interest in early intervention programs based on the findings of the suggestive experimental studies. There are also signs that the thrust of manpower policy is also changing to reflect lifelong learning considerations through support for continuing human capital investments, though it is palbably the case that this shift has not been informed by as much evidence as has been assembled in the debate over more conventional manpower instruments.

It is sometimes claimed that the extant U.S. experience is of marginal relevance to Europe because of the focus of its policy on the disadvantaged. Thus, for example, might not less disadvantaged European youth gain more from their exposure to training programs than their U.S. counterparts? We have therefore also to consider European studies. In an important sense, however, this charge misses the important point that perhaps the main relevance of the U.S. experience resides in the framework used to inform discussion in the United States. We refer of course to the experimental and nonexperimental evaluation exercises that have helped shape public-sponsored programs since the mid-1970s.

A number of problems attach to both types of evaluation. Nonexperimental studies have sought to identify the (mean direct) effect of programs on those who receive program services (i.e. the effect of the treatment on the treated). The

unobserved counterfactual is what program participants would have earned had they not received program services. The counterfactual is estimated using data on nonparticipants. Program participants are paired with an externally selected comparison group and adjustments made for inherent differences beween the two groups using a variety of estimators. There are a number of difficulties in using nonparticipant outcomes as the counterfactual. In particular, when individuals choose (or are chosen for) a treatment group, the participation decision will reflect unobservables that are likely linked to earnings. For example, if participants select to participate because of the poor alternatives otherwise available to them, there will be negative selection bias – that is, nonparticipants will have higher earnings than participants would have had if they had not participated. Accordingly, mean program effects will be understated. Conversely, if participants have unobservables that make them more likely to have higher earnings in the absence of a program, there will be positive selection bias and hence an overstatement of program effects. (Note, however, that it may still be more efficient to provide training to these individuals.)

An influential paper by LaLonde (1986) provided evidence that nonexperimental strategies involving the construction of comparison groups from alternative data sets and the application of available econometric techniques – most notably for dealing with selection bias – failed to replicate the experimentally generated results. That is, LaLonde compared actual results from the National Supported Work (NSW) demonstration with nonexperimental estimates that would have been obtained if no information had been available from the NSW's control group. The nonexperimental results were based on a variety of samples from general population surveys and various approaches for dealing with selection bias. As noted, none of the nonexperimental estimators passed muster – either in replicating the experimental estimates or passing conventional specification tests. Not surprisingly, LaLonde's study stimulated the use of experimental methods. Even if subsequent work was to call into question the generalizability of LaLonde's findings (see below), it is not in doubt that the comparisons used in the evaluations of the MDTA and CETA programs were inappropriate (Heckman et al., 1999).

In principle, random assignment gets round the problem of selection bias arising from missing data on common factors that affect participation decision and outcomes. (Strictly speaking it does not remove selection bias but, rather, balances the bias between the participant and nonparticipant samples.) However, as Heckman and Smith (1995) and Heckman et al. (1999) demonstrate, there are some important limitations of the experimental approach, quite apart from its cost and the ethical issues associated with denial of treatment. In particular, the actual implementation of experiments actually creates new forms of selection that require the application of nonexperimental procedures for making statistical adjustments. First, there is the problem of treatment group dropout and control group substitution important for all manpower programs

other than the NSW).[7] The dropout problem means that the majority of experimental studies measure the mean-difference effects of an offer to treat rather than the effect of a treatment on the treated. Substitution bias refers to a situation in which members of the control group have access to substitutes for the experimental treatment, so that control-group outcomes may no longer correspond to the untreated state. Although this problem is common to both experimental and nonexperimental studies, the effect is that an important advantage of an experimental study is lost, so that auxiliary nonexperimental analysis may again have to be performed. Second, a problem unique to experimental studies is randomization bias, which arises when "random assignment causes the type of persons participating in a program to differ from those who would participate in the program as it normally operates," or "from changes in participant behavior due to the threat of service denial" (Heckman and Smith, 1995, p. 99). Third, there is the problem of calculating the effects of individual components of program services that are offered in sequence. This requires that randomization be conducted at each stage in the sequence. In practice, this has not been achieved under extant experimental evaluations of programs offering tiered services. Fourth, it is impossible to use an experimental design to obtain estimates of the impact on training on post-program wages or the duration of subsequent jobless spells – as opposed to the effects on earnings and employment rates. Consider wages for example. If the treatment affects employment, as any well-designed program should, then the sample of employed treatments will have different characteristics than the employed controls. If, as is likely, individuals recording zero wages are less skilled, then the experimental impact estimate will compound wage effects with its selection into employment effects. Here again a complementary nonexperimental analysis would be required to recoup the wage effects.

These and other criticisms,[8] do not vitiate experiments – indeed, since some of them not a by-product of random assignment, they contain suggestions for improved experimental design – but they do make the sensible point that experiments are not a panacea. Experimental data need on occasion to be supplemented with nonexperimental analysis. For the future, there are promising developments in the theory and practice of nonexperimental methods that may offer unique solutions to many of the problems with experimental studies, given appropriate control group data. These theoretical insights and nonparametric estimators are set out in Heckman et al. (1999). The fact remains that evaluative research using extant datasets in nonexperimental analysis have provided less reliable conclusions of program impact than their experimental counterparts.

A final issue that cannot really be addressed through the application of experimental methods – even if the results of such exercises can inform as to the scope of the problem – is the need to consider indirect as well as direct effects. This consideration is especially relevant to Europe. Analyses of the programs considered

here have examined the effect of treatment on the treated or the offer of treatment. In each case, it is typically assumed that the no-treatment outcomes in a given regime mirror those would obtain in a no-program regime. If the programs cause a substitution of program graduates for the non-treatments, however, program effects will be incorrectly estimated. In low-skill labor markets with overly high wage minima, the introduction of wage subsidies can produce sizeable displacement effects among previously employed low-skill workers even if unemployment and output is unaffected. Heckman et al. (1999, p. 2035) show that in these circumstances all commonly used estimators will produce misleading estimates of program impact since they focus only on the program effects on subsidized workers (i.e. direct effects).

The ideal way to evaluate programs in these circumstances is via a general equilibrium approach. One such approach has been applied in the context of the Illinois UI bonus experiment, considered earlier. Davidson and Woodbury (1993) offer an equilibrium model of the labor market, that allows them to determine the direct effect of the Illinois program through its effect on the search behavior of those offered the bonus as well as its indirect effect via the impact on equilibrium employment and the search behavior of those who do not receive bonus offers. Data from the Illinois experiment is used to infer values for the unobservable parameters of their search model, which is then solved to derive estimates of the displacement effect. The model distinguishes between eligibles who search more and ineligibles who have at once less incentive to search because of the competition from eligibles and more incentive because a job gained today offers a second bite of the cherry, namely, the prospect of a bonus payment in the future. Although the two effects are a wash, the rate of job acquisition falls, while for those ineligibles who will never qualify for the bonus (permanent ineligibles), only the reduced search effect obtains – unambiguously raising their unemployment. Unemployment falls on net, and even though the magnitude of the displacement effect nowhere exceeds 1.9 workers per thousand, this is sufficient to generate a displacement effect that is 30 to 60% of the gross employment effect of the bonus program.

6. European Evidence

It should come as no surprise to learn that micro studies of the effects of active labor market policies in Europe produce very mixed results. The vast majority of studies are nonexperimental and thus involve all the usual problems in constructing a relevant comparison group and selection issues. That said, many of the studies do contain more detailed information on trainee/control characteristics than their U.S. counterparts using

administrative data, and have the added advantage of being able to exploit wage (rather than overall earnings) data.

Heckman et al. (1999, Table 25) summarize the results of 39 studies of employment and training programs in 9 European countries. Just three of the studies are experimental. Two of the three experimental studies look at UI job search-type programs in Sweden and the United Kingdom (see, respectively, the summary in Bjorklund and Regner, 1996; White and Lakey, 1992). Consistent with the U.S. evidence, both studies find that job search assistance plus 'prodding' serve to significantly raise employment rates of the treatments – for up to five years in the British (Restart) experiment.

Most of the studies point to reductions in unemployment rates/higher unemployment hazards or increased employment rates/lower employment hazards across a variety of programs. There is, however, no discernible hierarchy of effects with respect to the measures (classroom training, on-the-job training, and work experience, and so on). That said, some studies report statistically insignificant and even negative effects of employment and training policies.

Of the 39 studies reviewed by Heckman et al. (1999), significantly positive wage effects (if not earnings) are much less commonly encountered and a number of these are anyway rather implausible given the limited training exposure of the treatments. Again, some studies yield negative effects on wages. A case in point is Dolton et al. (1994), whose analysis of the British Youth Training Scheme (YTS) using data from the third cohort of the Youth Cohort Study indicates that YTS is associated with either lower earnings (for females) or insignificantly higher earnings (males). However, subsequent British research has suggested that there may be 'good' and 'bad' types of YTS training: bad where it is the only form of training provided, and good when it is accompanied by other types of private-sector training (Makepeace and Johnson, 1995). (But even the good YTS is not necessarily justified, since it may have been provided by employers in the absence of government subsidies.)

If European programs have had little positive impact on wages but an often (if not universally) positive impact on employment, the suggestion may well be that displacement of nonparticipants has occurred. Heckman et al. (1999, p. 2080) argue that the very scale of the European programs, taken in conjunction with their tendency to focus on on-the-job training, implies that "cost-benefit analyses based on the [observed] impact effects ... probably overstate the net social benefit derived from active labor market policies in Europe."

Necessarily, Heckman et al. (1999) can only go so far in summarizing the European evidence. Most notably, they rather neglect German research and nowhere consider the transition economies. The recent German evidence is evaluated in Fitzenberger and Speckesser (2000). (On the wider training issue, see also Pfeiffer,

2000.) Fitzenberger and Speckesser survey evidence on two important components of German active labor market policy, namely, continuing training and retraining on the one hand, and subsidized employment and wage subsidies on the other. They provide evidence from 20 training studies (12 for the former DDR and 8 for western Germany) and 4 subsidized employment studies (all for eastern Germany).[9] As far as the results of training programs in eastern Germany are concerned, only one study reports distinctly positive effects; the majority of the studies obtain no significant effects on either unemployment/employment or wages. The results for training in western Germany are mixed, arguably because of the linkage between public- and private-sector training. Yet three studies report significantly negative effects on such outcomes as labor market search, employment stability, employment, and wages. For its part, subsidized employment yields consistently positive outcomes for unemployment in only one study, the three other evaluations finding negative effects on unemployment hazards.

These are rather disappointing results, especially for eastern Germany. Abstracting from the very real problems of interpretation stemming from selection bias in nonexperimental studies – and the fact the positive effects, where observed, are modest – it remains the case that the research deals with only two aspects of active labor market policy in Germany while at the same time neglecting their acknowledged heterogeneity.

Although the labor market problems (and constraints facing) the transition economies have been well documented, together with certain institutional innovations to the employment problem – see, in particular, the comparative evidence assembled on ten transitional economies by Nesperova (1999) – very little is known about the impact of their employment policies. One of the more interesting evaluations is Burda and Lubyova's (1995) comparative analysis of active labor market policy in the Czech and Slovak republics, 1991-94. The authors use a 'matching function' – not to be confused with the matching method used in cross-section evaluations of employment and training measures – that links, at the district level, monthly flows of exits from unemployment to lagged stocks of unemployment and vacancies (see also Boeri, 1997). Additional determinants of the outflows from unemployment are individual district and fixed-time effects and of course various active labor market policies pursued (in the Czech case, proxied by either the number of participants in publicly-provided jobs and job creation schemes, or spending on those measures; in the Slovak case, captured by administrative staffing levels or expenditures).

For their preferred specification and most comparable results, it is reported that the expenditure measure is associated with significantly higher outflows from unemployment, the coefficient estimates being broadly similar for the two countries. In addition to providing estimates of the efficiency of policy in the two countries (in terms of bang per Koruna), since the Slovak republic slashed its manpower subventions by

71% in 1993, the authors attempt to estimate how much higher the outflows would have been – and, inferentially, how much lower steady state unemployment – without this cutback. They report that outflows from unemployment would have been 30% higher and the steady state unemployment rate 9.7% lower. Actual unemployment in the Slovak republic was 14.4% in 1993.

The actual magnitudes reported by Burda and Lubyova have to be taken with more than a pinch of salt; most generally because of the parsimonious nature of the matching model, and more specifically as regards the above projections because of omitted general equilibrium considerations and the implied regime change. Nevertheless, the consistency of the expenditure effects of employment policies in the two countries is of interest.

As was alluded to earlier, indirect effects are likely to be of especial concern in (western) Europe because of the prevalence of wage subsidies. Although there is some cross-country evidence that active labor market measures do serve to reduce aggregate unemployment, the estimates are often small and sometimes insignificant. Even if some of the smaller (absolute) point estimates reflect the presence of outliers, the overall conclusion would seem to be one of material displacement effects even where, say, the derivative of the unemployment rate with respect to the participation rate in manpower programs exceeds unity (see Scarpetta, 1996, p. 63).

The negative effects of employment and training measures have been addressed by Calmfors (1994). In addition to some effects that would hopefully be captured in microevaluations (such as reduced search activity and stigmatization effects), he also identifies deadweight effects (jobs that would have been created in irrespective of hiring subsidies), tax effects (changes in output in sectors taxed to pay for training and employment subventions), substitution effects (the change in relative wage costs causes the substitution of some categories of worker for others), and displacement effects (jobs generated by a program are at the expense of other jobs). Calmfors provides evidence from the Swedish experience that the deadweight and substitution in particular can tip the balance and outweigh the benefits. (It is interesting that Sweden is the principal outlier referred to above.) More formally, we have argued that there is no real substitute for general equilibrium techniques for measuring the indirect effects of training and employment practices. The European literature has instead used reduced form approaches (see Heckman et al., 1999, p. 2036), although general equilibrium models have been used in Denmark to quantify the consequences of other measures to include the potential effects of EU enlargement.

In such reduced form analyses it is now conventional to account for the endogeneity of policy, that is, to formally recognize that policy initiatives may be undertaken in response to anticipated adverse labor market conditions in the future. For this reason, it is sometimes argued that the point estimates from microevaluations might provide a lower bound of the true effects. But there are other sources of policy

endogeneity. Labor market policies are not solely to be viewed as a technocratic response. They can also be analyzed in public choice theoretic terms. Crudely put, active labor market policies might provide a cover for politicians who wish to appear to be doing something about unemployment, perhaps unemployment resulting from other policies (thus, the 'social chapter' inserted into the Treaty of Amsterdam might be construed as a means of mopping up unemployment resulting from the imposition of pan-European labor standards introduced under the social charter and social chapter initiatives).[10] This is doubtless overstating the case, but the political nature of employment and training policy has to be reckoned with. One important issue here is that these policies may massage the unemployment problem in the same manner as have early retirement measures and disability programs. Participants in most of the programs considered here will not be counted as unemployed, and the phenomenon of cycling between training or subsidized unemploynent and traditional UI benefits may create the illusion that the key problem of long-term unemployment has been lessened (see also Boeri, 1997). It follows that analysts should be particularly wary of using 'narrow' measures of unemployment in aggregative studies. More generally, assessment of policy should be informed by the demand for (e.g. on the part of employed 'insiders') and supply of (by politicians) of active labor market policy measures. It is of course a moot point whether interest groups that help determine the shape of policy are less entrenched in the transition economies

7. Concluding Remarks

Given four decades of experience, the lessons from market-oriented labor market policies – and for reasons of tractability I have restricted my attention to active labor market policies rather than the gamut of employment protection mandates (for an evaluation of which, see Addison and Hirsch, 1999) – might appear thin. One view of active labor market policy in western nations would be that it is all too easy to design and implement ineffective training programs, an assertion underscored by public choice considerations. But we have argued that some programs have been successful, even if we have yet properly to account for program heterogeneity, and much has been learned about how to measure program impact. The main lessons may nonetheless have more to do with what does not work than what does.

As for the transition nations, wherein rising unemployment may indeed be indicative of the progress made in the reforming their economies, and where the generosity of UI systems (replacement rates and/or duration of benefits) has already been cut, there is no alternative to the use of active labor market policies on a fairly wide scale. Policies should focus in the first instance on a very much improved job

broking function (to include mobility incentives), start-up loans coupled with technical support, subsidized employment measures for youth to create new jobs, and some targeted wage subsidies for the disadvantaged. The training function would at all times benefit from close employer involvement which would also seem to imply the extension of training to employed workers. But programs have to reflect the real risk of long-term unemployment, and attempts to reduce this exposure will inevitably involve some cycling between program participation and unemployment benefits and limit the intensity of service provision.

Policy must be responsive to experience and labor market conditions, and must increasingly be guided by evaluation exercises and foreign experience in this regard. Thus far we have some idea about placement rates of various programs (see Nesperova, 1999, p. 59) but this falls far short of what is needed, namely, an assessment of the internal effectiveness of programs and calculation of indirect effects. Extant assessments on the basis of labor market matching functions do show evidence of increased turnover of the unemployment pool. This is useful in terms of the immediate need to limit exposure to long-term unemployment, but it says nothing about net program impact. Pilot projects are now under way under the aegis of World Bank and EU financing that should shed light on how program participants have fared relative to nonparticipants. Here as elsewhere, however, attention will then have to turn to evaluate the effects of policies on nonparticipants.

Endnotes

1. Trade adjustment assistance (TAA) was first authorized under the Trade Expansion Act of 1962. It offered supplemental unemployment benefits to workers who lost their jobs because of trade liberalization. The program was expanded in 1974 under the Trade Act, as a result of which workers had only to show that import competition had 'contributed importantly' to their job loss. Amendments to the enabling legislation in 1981 sought in the first instance to reduce the cost of the program – by restricting supplementary benefits to the unemployment compensation levels obtaining in the relevant state – and, second, by placing more emphasis on training. Under the 1988 Omnibus Trade and Competitiveness Act, job training was made a specific requirement for eligibility under the program. Finally, as a result of the North American Free Trade Agreement, TAA was augmented by a so-called 'transitional adjustment assistance program' (NAFTA-TAAP). This offers the same benefits and services as TAA proper for those workers displaced by trade with Canada or Mexico, or whose jobs were relocated to either country. The claimant has again to be enrolled in an approved job training scheme in order to receive program benefits and services.

2. A group of internet tools have been established under a so-called 'career kit.' This includes web sites listing job vacancies (http://www.ajb.dni.us/), resumes (http://www.atb.org), and information on occupational employment trends and job training requirements (http://www.acinet.org/acinet/).

3. Another element of current policy is lifetime learning. This has two main components: a lifetime learning tax credit and a Learning Anytime Anywhere Partnership program. The former applies to the first $5,000 of a family's qualified educational expenses ($10,000 after 2002). The latter supports partnerships between the educational sector, businesses, and community organizations to address challenges in lifelong learning and postsecondary education.

4. See also the study by Corson et al. (1993) of classroom training under the trade adjustment assistance program.

5. On UI self-employment demonstrations, see Benus et al. (1994).

6. We do not dwell on that component of the Illinois experiment offering the $500 bonus to the employer (rather than the individual), other than to note that there were no statistically significant differences in duration of UI payments and subsequent earnings as between the treatment and control groups in this case – although it has been alleged that the voucher mechanism may have stigmatized participants by identifying them as less skilled or less motivated (Burtless, 1985).

7. Note that dropping out is different from sample attrition since treatment dropouts will remain in the sample. The problem is that since attrition rates are nonrandom (being higher for those with poorer labor market characteristics) and larger for the

control sample, experimental estimates of program impact will be biased. This selection bias must again be tackled using nonexperimental techniques

8. There are additional limitations of experimental studies. These include issues such as the determinants of program applications (which can generally not be addressed through experiments), and the general inability of experimental data to yield useful information about the overall distribution of program effects (i.e. the experiments yield only mean-difference estimates of a program's effects).

9. Also examined are five macroeconomic evaluations, dealing with short-and long-term unemployment, and labor market mismatch. The results are generally more favorable for subsidized employment than before in suggesting some diminution in structural unemployment, while training is associated with some reduction in long-term unemployment (but see below).

10. This is not to deny certain positive features of the employment chapter, most notably its emphasis on benchmarking to best-practice standards which might improve the evaluation exercise.

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