Evaluating active labor market programs in Romania

Núria Rodríguez-Planas · Benus Jacob

Abstract   Using unusually rich (for transition economies) follow-up survey data and propensity score matching techniques, this paper seeks to increase our knowledge on what active labor market programs (ALMPs) work in South-East European countries by providing estimates of the effects of four ALMPs implemented in Romania in the late 1990s. We find that three programs (training and retraining, self-employment assistance, and public employment and relocation services) had success in improving participants' economic outcomes. In contrast, public employment was found detrimental for the employment prospects of its participants. Our sensitivity analysis also finds evidence that, in the case of training and retraining, self-employment assistance, and public employment and relocation services, operators "cream off" the most qualified candidates among the unemployed; whereas public employment seems to be used as a regional policy by "bringing work to the workers", that is, creating jobs in high unemployment regions.

N. Rodríguez-Planas (✉)
Departament d'Economia i Història Econòmica,
Universitat Autònoma de Barcelona, 08193 Bellaterra, Spain
e-mail: Nuria.Rodriguez@uab.cat
URL: http://selene.uab.es/_cs_u_fonaments/fitxes/nrodriguez.htm

B. Jacob
IMPAQ International LLC, Columbia, MD, USA

1 Introduction

While the literature on evaluations of active labor market programs (ALMPs hereafter) in developed countries has experienced a substantial increase in recent years, the evidence in transition countries is considerably scarcer and has tended to concentrate on several Central European countries (such as, the Czech and Slovak Republics, Hungary, Poland, Slovenia, and East Germany).[1] At the same time, little is known on the effectiveness of ALMPs in South-East European countries, which differ from Central European countries by the fact that they had a slower transition process.[2] Although many findings from Central European countries may well apply broadly to South-East European countries, the latter countries' common underlying specificities—such as, the higher land fragmentation, the growing subsistence agricultural sector, the much larger informal urban labor markets, and the weaker capacity to implement programs—may limit what some programs can achieve in terms of creating formal employment or increasing wages (see Nesporova 2002; Irac and Minoiu 2006 and Vidovic (2004), among others, for thorough discussion on the economic and institutional specificities of South-East European countries).

In addition, one of the major shortcomings of the growing body of evaluations in post-communists countries is the quality and quantity of the data—see Earle and Pauna (1996) and Kluve et al. (1999), for discussion on the poor quality of ALMPs' data in transition economies. Most of the studies rely on survey data.[3] Although these data are rich with respect to informative covariates, most previous evaluation studies were missing several important variables, such as employment history and earnings before the treatment—see Heckman and Smith (1999), for discussion on the importance of controlling for employment dynamics prior to program participation to correct for selection bias. To our knowledge, only the evaluations from the Czech Republic (Terrell and Sorm 1999), Poland (Kluve et al. 1999, 2008), and East Germany (Bender and Klose 2000; Bender et al. 2005; Lechner et al. 2005a,b and Fitzenberger and Speckesser 2007, among others) have information on the length of the unemployment spell that took place right before program participation. Studies with earnings data prior to the treatment are limited to Terrell and Sorm's Czech Republic study, and the East Germany studies.

In an attempt to increase our knowledge on what experiences work in post-communist countries, in general, and, South-East European countries, in particular,

---

[1] See Katz (1994), Fay (1996), Martin (1998), Martin and Grubb (2001), Dar and Tzannatos (1999), Bechterman et al. (2004) and Kluve (2006) for good reviews of the literature.

[2] To our knowledge, implementation of ALMPs in South-East European countries has been evaluated in Bosnia and Herzegovina, Bulgaria, and Macedonia. See the conclusion for a discussion of the literature and how it relates to our results.

[3] One important exception are the recent evaluations on the effectiveness of training programs in East Germany that use a unique integrated data set from various administrative sources, including social insurance data for employment, data involving transfer payments during unemployment, and survey data for all training participants (Bender and Klose 2000; Bender et al. 2005; Lechner et al. 2005a,b; and Fitzenberger and Speckesser 2007). In addition, earlier studies based on regional data used aggregated quarterly or monthly panel district data on unemployment outflows and ALMP expenditures (Boeri and Burda 1996; Burda and Lubyova 1995; Terrell and Munich 1995).

this paper seeks to: i) provide estimates of the effects of four ALMPs implemented in Romania in the late 1990 s; and ii) test the sensitivity of the results to the availability of information on employment history and earnings before the treatment. This analysis, although focusing on the Romanian case, is relevant to a larger set of transition countries, including Moldova, Albania, Bulgaria and Ukraine, as these countries experienced a slow transition process, and shared underlying specificities, such as the importance of rural sector and high poverty levels. In addition, using survey data unusually rich for studies conducted in transition economies, we present empirical evidence of the importance of having information on employment history and earnings before the treatment to address the selection issues in a reasonable way.

In this paper, we analyze the effectiveness of participating in one of four active labor market programs implemented in Romania in the late 1990 s compared to the no-program state. The four programs under evaluation are: (1) training and retraining (TR), (2) self-employment assistance (SE), (3) public employment (PE), and (4) public employment and relocation services (ER). Our analysis is based on a follow-up survey specifically designed and collected for this evaluation. The most important reasons for using survey data instead of administrative data were that the former allowed us to track individuals' earnings and employment status at different points in time over a four-year period, and provided us with good quality data on key variables—such as earnings for both the employed and the self-employed. However, it should be noted that there are also drawbacks with the survey data used, such as, the limited sample size, the imperfect recall of the interviewed individuals for events that have occurred some time ago, and the possible misleading replies.

The analysis, based on matching methods, reveals that three of the four programs (TR, SE and ER) had success in improving participants' economic outcomes. We find that ER succeeded in increasing the likelihood of participants' employment and their earnings, and reducing the likelihood of receiving unemployment benefits. We also find that SE improved its participants' employment prospects, although it did not have a significant impact on their earnings; and that TR increased the earnings of its participants and reduced the likelihood of receiving unemployment benefits. In contrast, our analysis reveals that PE was found detrimental for the employment prospects of its participants. Finally, our sensitivity analysis finds evidence that, in the case of TR, SE, and ER, operators "cream off" the most qualified candidates among the unemployed; whereas PE seems to be used as a regional policy by "bringing work to the workers", that is, creating jobs in high unemployment regions. These results highlight the importance of having good quality data on individuals' characteristics before participation to correct for selection bias. In particular, our paper documents that pre-treatment labor force and earnings information contain indispensable information regarding selection into treatment and that controlling for these variables can eliminate a large part of the overt bias between treated and comparison units. This result confirms and reinforces the point made by Heckman and Smith (1999) and Heckman and Smith (2004), in a completely different labor market, and Kluve et al. (2008), in another transition country.

This paper is organized as follows. The next section gives a short discussion of the economic and institutional background, including program description and

participation figures for the ALMPs under evaluation. Section 3 describes the data, and displays the descriptive statistics. Section 4 describes the methodological approach to estimate the treatment effects. Section 5 discusses the estimation results and presents the sensitivity analysis, and Sect. 6 concludes. An additional appendix, which can be find in the authors' webpage, provides further information on the data and detailed empirical results. Finally, we refer to the working paper for a thorough discussion of the sample design and the survey effort.

2 Economic and institutional background

2.1 The economic context

Romania's transition to a market economy has been slow and painful partly as a result of its stop-and-go approach to the restructuring process. Since the 1989 Revolution, successive governments have adopted a cautious approach to market-oriented reforms. This slow pace of reform—relative to some of its neighbors in Central Europe—delayed needed structural changes and added greater difficulties to the already unfavorable set of initial conditions inherited from the previous regime.

After an initial economic contraction in the early 1990s due to the increase of external competition and the abolition of the Council of Mutual Economic Assistance, Romania applied a macro-stabilization program and experienced a partial economic recovery beginning in 1992, similar to the one observed in leading transition economies in Central Europe. In contrast with these leading economies, Romania lived a second period of economic decline beginning in 1996, which was mainly caused by the lack of enterprise restructuring. In the second half of 1996, Romania's authorities took a series of decisions with the aim of accelerating the privatization, restructuring and liquidation of unprofitable business. However, the recovery was slow and did not produce significant economic results until the year 2000. Since then the Romania economy has grown at an average of 4 or 5% per year.

With the collapse in output, labor surplus soared and registered unemployment reached over 10% of the labor force in 1994. The unemployment rate then fell temporarily during 1995 and 1996, only to rise rapidly thereafter, reaching 11.5% in 1999. Since then, the registered unemployment rate has fallen gradually to 9% of the labor force in 2001.

However, data on registered unemployment in Romania understate the real problem with dislocated workers for at least the following three reasons. First, during the 1990s the increase in open unemployment was contained by Romania's policy approach of limiting job destruction by adjusting through real wages, combined with a series of early retirement programs. However, these two policies pushed workers out of the labor force and into low productivity jobs, primarily in subsistence agriculture and the urban informal sector. Second, a high share of Romania's employment was in subsistence agriculture—the share of agricultural employment in Romania in 2001 was 42% of total employment (up from 28% in 1989). And third, the existence of border-line employment categories such as unpaid family helpers, involuntary part-timers, or people in "technical" unemployment or unpaid leave initiated by the employer to

measure employment in Romania substantially overstates employment and influences key indicators of labor market performance.[4]

2.2 The institutional environment

As early as 1991, Romania adopted passive labor programs, including unemployment benefits, allowance for vocational integration and support allowance. To be eligible for these benefits, unemployed individuals had to: be registered at the local employment office; be aged eighteen and over; have an income less than half of the indexed national minimum wage; and be in one of the following two covered groups: (1) employees having worked for at least 6 months during the last 12 months; or (2) be a recent graduate from school or university unable to find suitable employment. Unemployment benefits were paid for a maximum duration of 9 months. The level of these benefits ranged from 50 to 60% of the average monthly salary during the last 3 months of employment for displaced workers. For new entrants, benefits varied by the level of education and years of experience for those with prior work experience. After exhausting unemployment benefits, those who remained unemployed received a support allowance (of 60% of the indexed minimum wage) for a maximum period of 18 months.[5]

In 1997, the Romanian government launched the real start of active programs on a significant scale by signing a loan agreement with the World Bank. The four ALMPs offered were: (1) training and retraining (TR), (2) self-employment (SE), (3) public employment (PE), and (4) employment and relocation services (ER). Although these programs were designed and implemented by county (*judet*) level Agencies for Employment and Vocational Training, the services were not provided by the county agencies themselves, but were contracted out to public or private service providers. Contracts to service providers were awarded with built-in incentives to improve labor market impact such as negotiated levels of job placement and business start-up, with financial incentives to meet objectives and disincentives if objectives were not met. Thus, service providers were likely to select those unemployed individuals most likely to succeed in completing their program and accessing employment.

The four programs were clearly differentiated as evident from the description of their key characteristics described below:

- **Training and retraining (TR).** TR included vocational training, general education and literacy skills for those who lacked these basic skills or needed to learn new marketable ones. Although the maximum duration of these services was limited to 9 months per individual, in practice, the length was considerably shorter than the established maximum duration. The program could increase the period in which individuals received benefits since it offered clients a subsistence stipend at the minimum wage level for a period equal to the difference between the months of

---

[4] See Brown et al. (2006) for a careful study on nonstandard forms and measures of employment and unemployment in Romania.

[5] See Earle and Pauna (1998) for a detailed description and thorough analysis of this program in Romania.

unemployment benefits left and the months of training. Service providers had to agree to a negotiated job placement rate of at least 60%.

- **Self-employment assistance (SE).** Provision of these services included initial assessment of the aptitude and skills of unemployed persons to start businesses, developing business plans, advising on legal, accounting, financial, marketing and sales services issues, assistance in the dialogue with local authorities, short-term entrepreneurial courses and training and other consulting services to unemployed workers who intended to start a business. There were also provisions for short-term working capital loans of up to $25,000 US dollars to program participants. Service providers had to agree to a negotiated business start-up rate of at least 5% of clients initially contacted. Maximum length of initial contract was 12 months.
- **Public employment (PE).** PE was frequently considered as fully subsidized labor, and was mainly offered in those regions with the least economic opportunities. It included the possibility of working in a variety of projects, such as environmental cleanup, ecological projects related to infrastructure, refurbishment of public infrastructure, and provision of assistance and support to social agencies, such as schools, or retirement homes. The maximum participation in PE was 6 months, and, a stipend was set at a maximum of the average wage level of the type of activity provided and for the duration of the program (thus, as with TR, participation in PE increased the period in which individuals could receive benefits by the length of the program). Service providers had to agree to a negotiated job placement rate of at least 10%.
- **Public employment and relocation services (ER).** Clients eligible for this service were offered a variety of employment services, including job and social counseling, labor market information, job search assistance, job placement services, and relocation assistance. Notice that ER focused on counseling but did not include monitoring or sanctioning eligible workers who did not comply with the job search planning. The duration of these services was limited to 9 months per individual. The program also offered up to 2 months of salary at the minimum wage. Service providers had to agree to a negotiated job placement rate of at least 10%.

In addition, there were some requisites that prevented duplication of payment and services. First, individual clients could not receive income support payments (e.g., minimum wage during TR or PE) if they were receiving other types of state financed income support, such as unemployment benefits. Second, individuals could not participate in both TR and PE. And third, individuals were not allowed to participate more than once in a program in a period of 24 months.

As indicated in Table 1, among these four ALMPs, there were 767 contracts completed as of 1 September 2001, and over sixty-four thousand clients served. The overall placement rate among these contracts varied largely by program—ranging from 41% for TR to 13% for PE. The program with the largest number of clients (ER) provided assistance to 31,679 individuals at an average cost of only 123.74 thousand lei per client (about $12 US per client). In contrast, the PE served a much smaller number of clients (9,496); the cost per client for this program was 2,915.77 thousand lei per client (about $294 US per client).

Table 1 Completed ALMP contracts as of 1 September 2001

	Number of contracts	Clients served	Clients placed	Placement rate (%)	Total cost (Lei)	Cost per client (Lei)	Cost per placement (Lei)
TR	54	2,892.00	1,197	41.39	1,564,771,985.06	541,069.15	1,307,244.77
SE	92	20,293.00	3,568	17.58	3,635,562,636.30	179,153.53	1,018,935.72
PE	533	9,496.00	1,248	13.14	27,688,156,974.32	2,915,770.53	22,186,023.22
ER	88	31,679.00	6,610	20.87	3,920,060,312.43	123,743.18	593,049.97

Costs figures have been deflated using 1998 deflator. Source: USDOL Technical Assistance Support Team

3 The data and descriptive statistics

3.1 The data

Unfortunately, we were unable to use the Labor Force Survey because it is cross-section and it is missing several important variables, such as earnings, ALMPs participation, or retrospective questions on labor market history. Similarly official unemployment data was of no use because it reports total number of unemployed and the number in a few basic categories (such as sex, age groups, counties, broad educational groups, and ALMPs), but does not report individual characteristics and experiences of the unemployed. We therefore decided to collect survey data from computer-assisted telephone interview especially designed for this study. To do so, from each program, we randomly drew participants whose ALMP contract began in 1999. As we wanted to compare them with non-participants, we had to choose a potential comparison group. We restricted this comparison group to those who were registered at the Employment Bureau around the same time and in the same county than participants but who did *not* participate in an ALMP during 1999. Additionally, participants and non-participants received the same questionnaire.

To select non-participants, we first determined, the number of participants who were selected for the participant sample in each of the counties. Next, in each county, we selected an equal number of non-participants from the same Employment Bureau register list. Participants and non-participants were interviewed during January and February of 2002, and asked questions on employment and earnings: (1) at the time of the survey, (2) during the years 2000 and 2001, and (3) during the year 1998, that is, the year before participating in the ALMPs. Restriction that all data be available led to a sample of 3,127 individuals. Of these 1,626 had participated in one of the four ALMPs under evaluation during 1999, and 1,501 were non-participants during the same year. All the results presented below are robust to using all of the observations available for each of the different outcome variables. However, in order to work with the same sample in the whole paper we restricted our sample to having all data available. We refer to the working paper and the appendix for a thorough discussion of the sample design, survey effort, and robustness of the results.

Compared with existing administrative data, our survey provides detailed information on: (1) individual labor market histories and earnings prior to 1999 unemployment

spell; (2) individual socio-demographic information; and (3) information on individuals' employments and earnings at least 24 months after the program started. We combined these data with data capturing the local labor market conditions. Table 2 contains sample mean values of the most relevant variables (a full list of the sample mean values for all available variables can be found in Table A.3. in Electronic Supplementary Material).

As mentioned earlier, there are drawbacks with the survey data used. One of the biggest concerns with these data is the possibility that recall bias for events that have occurred some time ago may affect our estimates. Empirical evidence has shown that unemployment that occurred some time ago tends to be underreported in relation to more recent unemployment (see Jürges 2005 for a thorough study on retrospective errors and inconsistencies in the unemployment information using survey data.). Similarly, there is evidence that unemployed workers tend to comparatively underreport unemployment and wage losses when these are associated with less salient events (Oyer 2004; Song 2007.) However, for recall bias to be affecting our estimates, the size and direction of the recall bias would need to differ between the treatment and control groups. This could easily occur if there are compositional differences between the two groups. But, the matching methodology used to calculate our estimates balances the distribution of covariates in the treatment group and the matched comparison group and, thus, aims to reduce the compositional differences between the two groups. We argue that although recall bias is likely to occur with our survey data, the odds that it affects our estimates are considerably reduced by the methodology and the rich data used.

It is also important to highlight that our sample contains information only on individuals who are registered unemployed, excluding all those who might be unemployed according to a standard labor force survey definition, but who, for whatever reasons, have failed to register at the local office.[6] In addition, we need to keep in mind that the sample of non-participants selected does not represent a random sample of registered unemployed, as they were selected to match the regional distribution of our sample of participants. Although this does not affect our estimation and interpretation strategy, it should be kept in mind when interpreting the differences between groups.

3.2 Descriptive statistics

Table 2 displays descriptive statistics for socio-economic variables for the different sub-samples that are defined by treatment status. We restrict the data to the 25 to 55 years old in order to rule out periods of formal education or vocational training as well as early retirement. The descriptive statistics conform to our expectations that different types of displaced workers participated in the different ALMPs. The results are summarized below.

---

[6] Registered unemployment in Romania is measured according to national legislation and differs considerably from unemployment as recorded by the Labor Force Survey (Romania National Institute of Statistics 2006; Earle and Pauna 1996).

Table 2 Selected characteristics of ALMP participants and non-participants (percentages except where noted)

	TR (1)	SE (2)	PE (3)	ER (4)	Non-participants (5)
Pre-program characteristics					
Male	45.83	50.69	89.89	45.92	63.82
Education completed					
Primary school	5.56	9.97	21.12	13.25	14.86
Secondary school	63.89	32.41	56.85	45.92	44.30
High school	27.78	37.67	18.65	28.65	29.31
University	2.78	19.45	3.71	12.82	11.26
Region					
Rural	8.33	5.82	35.06	11.24	17.92
Urban (less than 20,0000 inhabitants)	18.06	35.46	19.10	18.34	18.45
Urban (20–79 thousand inhabitants)	16.67	14.13	39.10	20.08	28.11
Urban (80–199 thousand inhabitants)	27.78	27.15	5.39	39.89	25.98
Urban (200 thousand inhabitants)	29.17	17.45	1.35	10.44	9.53
County's unemployment rate	10.67	11.37	15.76	11.86	13.12
Work experience (years)	21.43	22.99	21.74	23.99	23.63
	(7.13)	(8.04)	(8.23)	(8.28)	(8.91)
Not employed in 1998	45.83	23.82	59.10	22.36	19.19
Employed in 1998	54.17	76.18	40.90	77.64	80.81
Employed between 1 and 3 months	4.17	1.39	5.62	4.42	2.53
Employed between 4 and 6 months	12.5	6.37	16.85	8.70	7.40
Employed between 7 and 9 months	4.17	3.05	8.09	10.71	5.53
Employed between 9 and 12 months	33.33	65.37	10.34	53.82	65.36
1998 average monthly earnings (in thousand lei)	522.92	881.72	384.16	758.07	926.60
	(559.72)	(756.83)	(552.48)	(618.68)	(719.77)
Average unemployment length during 1998 (months)	6.26	3.38	8.75	3.90	2.99
	(4.98)	(4.87)	(4.11)	(4.70)	(4.56)
Received training during 1998	18.06	8.86	4.04	6.69	3.13
Post-program Outcomes					
Current experience (January or February 2002)					
Employed	57.81	50.86	31.74	51.28	39.24
Average monthly earnings (in thousand lei)	311.76	303.28	160.96	309.64	232.62
	(360.70)	(384.02)	(256.32)	(485.19)	(389.45)
During the two year period 2000–2001					
Employed for at least 6 months	75.00	78.86	48.17	78.87	68.22
Employed for at least 12 months	65.62	59.71	33.56	63.39	51.97
Average monthly earnings (in thousand lei)	449.42	398.60	256.12	394.34	322.42
	(516.23)	(475.21)	(279.13)	(426.58)	(357.41)
Months unemployed	9.52	10.36	16.22	9.45	12.14
	(10.01)	(9.43)	(9.34)	(9.39)	(9.78)
Months receiving UB payments	0.06	1.44	1.78	0.79	1.79
	(0.47)	(4.70)	(5.42)	(3.03)	(5.05)
Sample size	72	362	445	747	1, 501

Standard deviation in parenthesis for continuous variables

Clearly, participants in PE are the most disadvantaged among the unemployed both in terms of level of education and employment history. They are the least educated, with one fifth of them having only primary school education. And they have the worse employment prospects since almost two thirds of them were not employed in 1998, and their average unemployment spell in 1998 was 9 months. Moreover, these participants are also the most likely to live in rural or small urban areas with high unemployment. Finally, compared to the other programs, they are disproportionately more likely to be male workers. Similar to participants in PE, participants in TR have low educational levels and poor employment perspectives. In contrast, they tend to be concentrated in large urban areas, and a higher proportion of them are females. Participants in SE and in ER have relatively more stable employment history during 1998 than participants of the other two ALMPs, as three fourths reported working during 1998. There are, however, clear differences between these two groups. While, participants in SE tend to be more educated, participants in ER are more likely to live in large urban areas.

4 Methodological approach

4.1 Identification

We follow the potential–outcome–approach to causality (Roy 1951;Rubin 1974) and base our analysis on comparing the outcomes of two alternative strategies available to displaced workers: to participate in a particular ALMP, or to continue searching for a job as openly unemployed.[7]

The two potential outcomes are Y^1 (individual receives treatment, $D_i = 1$) and Y^0 (individual does not receive treatment, $D_i = 0$). However, the observed outcome for any individual i can be written as: $Y_i = D_i \cdot Y_i^1 + (1 - D_i) \cdot Y_i^0$. The treatment effect for each individual i is then defined as the difference between her potential outcomes: $\tau_i = Y_i^1 - Y_i^0$. As we can never observe both potential outcomes for the same individual at the same time, the fundamental evaluation problem arises. We focus our analysis on the average treatment effect on the treated (ATT), that is, the differential impact the treatment shows for those individuals who actually participate in a program. The ATT is given by:

$$\Delta = E(Y^1|D = 1) - E(Y^0|D = 1) \tag{1}$$

Given Eq. (1), the problem of selection bias arises as the second term on the right-hand side, $E(Y^0|D = 1)$, that is, the hypothetical outcome without treatment for those persons who received the treatment, is unobservable. Since the data is non-experimental, the condition $E(Y^0|D = 1) = E(Y^0|D = 0)$ is usually not satisfied, and estimating ATT based on Eq. (1) will lead to a selection bias. This bias arises because participants and non-participants are selected groups that would have different outcomes, even in the absence of the program.

---

[7] We considered basing our analysis on the "multiple treatments" model. However, the large socio-economic differences across the different treatments combined with the relative modest samples, lead to large losses of observations due to the common support requirement, and poor matching.

To correct for possible selection bias, we use matching, which intends to mimic a randomized experiment *ex post* by balancing the distribution of covariates in the treatment group and the matched comparison group.[8] This strategy is feasible if there is only "overt bias" (Rosenbaum 1995), that is, treatment and comparison group differ prior to treatment only in observable variables that matter for the outcome under study. Let X denote the vector of observed pre-treatment variables, or covariates. Then the concept of "selection on observables" is formalized in the following *conditional identifying assumption* (*CIA*): The assignment mechanism D is independent of the potential outcomes (Y^1, Y^0) conditional on X (Rubin 1974, 1977). This assumption is commonly referred to as *unconfoundedness*. As we are interested in ATT only, we only need to assume that Y^0 is independent of D conditional on X (that is, $Y^0 \perp D|X$, where $\perp$ denotes independence) because the moments of the distribution of Y^1 for the treatment group are directly estimable. Clearly, this assumption may be a very strong one and has to be justified on a case-by-case basis, as the researcher needs to observe all variables that simultaneously influence participation and outcomes. In the next section, we discuss the plausibility of this assumption for our evaluation. Additionally, it has to be assumed that there is *weak overlap*: $\Pr(D = 1|X) < 1$, for all X. This implies that there is a positive probability for all X of not participating, that is, that there are no perfect predictors which determine participation. These assumptions are sufficient for identification of the ATT, which can be written as:

$$\Delta^{\text{MATCHING}} = E(Y^1|D = 1) - E_x[E(Y^0|X, D = 0)|D = 1] \qquad (2)$$

where the first term can be estimated from the treatment group and the second term from the mean outcomes of the matched comparison group. The outer expectation is taken over the distribution of X in the treatment group. As matching on X can become hazardous when X is of high dimension (*curse of dimensionality*), Rosenbaum and Rubin (1983) suggest the use of balancing scores $b(X)$. These are functions of the relevant observed covariates X such that the conditional distribution of X given $b(X)$ is independent of the assignment to treatment. The propensity score, $P(X)$, also known as the probability of participating in a program, is one possible balancing score. For participants and non-participants with the same balancing score, the distributions of the covariates X are the same, that is, they are balanced across the groups. Hence, the assumption of *unconfoundedness* can be re-written as $Y^0 \perp D|P(X)$, where $\perp$ denotes independence, and the new *overlap condition* is given by $\Pr(D = 1|P(X)) < 1$.

4.2 Validity of the CIA

Even though our survey data is unusually rich for studies conducted in transition economies, it is comparatively less informative than most data available for developed countries. Nevertheless, we argue that the most important variables affecting program participation are available in our data, and that the CIA holds in our application.

---

[8] See Imbens (2004) or Smith and Todd (2005) for recent overviews regarding matching methods.

Many have argued that program participation probability depends upon the variables determining re-employment prospects once unemployment began (Heckman and Smith 1999; Sianesi 2004; Fitzenberger and Speckesser 2007; among others). Following these authors, we argue that the level of previous earnings, and some information about pre-program unemployment history are important factors in determining whether an individual will participate in any program, as well as in which of the programs. In addition, these variables are also likely to influence the future labor market outcomes, and thus, in order for CIA to be plausible, they should be included in the estimation of the propensities.[9] Finally, previous earnings are also a good proxy for workers' pre-displacement job characteristics and workers' motivation, ability and soft skills.

We also included in the propensity score estimation socio-demographic and human capital variables. Among the first group of variables, we have information on age, and gender, as well as family composition and whether the person is the family's main wage earner. Among the second group of variables, we included information on the individual's education level, his experience prior to participation, and whether he had participated in any training program during 1998 and the duration of that program.

In addition, we included variables that capture the local labor market conditions. These variables measure the different employment opportunities in the counties. Moreover, since differences in labor market conditions may favor a different mix of program and unemployment policies, these variables are also a proxy for different policy approaches across counties. Finally, we included county dummies to capture unobserved local aspects that are likely to be correlated with program implementation, utilization, and delivery, or local offices' placement policies, and thus relevant for program-joining decisions and individuals' potential labor market performance.

4.3 Estimation of the propensity score and matching details

We selected four comparison groups (one for each of the four groups of ALMPs participants) from the sample of potential comparison group members. We used propensity scores to select comparison groups for *each* treatment group, according to the following three steps.

First, we estimated binary conditional probabilities for each of the programs vs. non-participation. The results of the four probit estimations can be found in the Appendix. Second, we used the output from these selection models to estimate choice probabilities conditional on X –the so-called propensity scores, $P(X)$– for each treatment and potential comparison group member. We then imposed the common-support requirement to guarantee that there is an overlap between the propensity scores for each pair. Third, for each treatment group member, we selected potential comparison group members based on their propensity scores and their county. The selection process was

---

[9] While it is true that we only have information on unemployment history for the year prior to becoming unemployed, Fitzenberger and Speckesser (2007) have recently found that the omission of the employment history beyond 12 months before the beginning of the unemployment spell does not invalidate their main results for West Germany.

done with replacement.[10] In addition, the selection method used was kernel-based matching, which uses all of the comparison units within a predefined propensity score radius (or "caliper of 1%").[11]

One major advantage of kernel-based matching is the lower variance which is achieved compared to other possible alternatives, such as, the nearest-neighbor matching, because more information is used for constructing counterfactual outcomes. As our treatment and comparison groups are rather small, we prefer this method over the nearest-neighbor matching.[12] When there were multiple matches, each non-participant received a weight that reflects the number of successful matches within the caliper range. To adjust for the additional sources of variability introduced by the estimation of the propensity score as well as by the matching process itself, bootstrapped confidence intervals have been calculated based on 1,000 re-samples.[13]

Our goal was to select, for each of the four groups, a well-matched comparison group. A comparison group is well matched to a treatment if the estimated propensity score and the collection of available baseline characteristics are not significantly different across the two groups. Overall, results in Table 3 show that matching on the estimated propensity score balances the X's in the matched samples extremely well (and better than the other versions of matching we experimented with).

5 Empirical results

5.1 Measurement of labor market outcomes

Because the primary objective of these policies is to get displaced workers back to work in jobs, at least implicitly, as good as the previous one, the analysis focuses in two types of outcomes: those that measure workers' reemployment probabilities (in paid or self-employed jobs), and those that measure workers' earnings at the new job.[14] Moreover, since our survey included retrospective questions, we measure these outcomes at two different points in time: at the time of the survey, and during the two-year period prior to the survey, that is, during the years 2000 and 2001. Measuring employment experience with employment for a period of at least 6 and 12 months, respectively,

---

[10] Matching with replacement is beneficial in terms of bias reduction, but may reduce the precision of the estimates. An additional advantage of matching with replacement instead of without replacement is that the results are not sensitive to the order in which the treatment units are matched (Rosenbaum 1995).

[11] We used Epanechnikov kernel.

[12] In addition, Plesca and Smith (2007) have found results that highlight the relatively poor performance of the widely used single nearest-neighbor matching estimator.

[13] Heckman et al. (1997) derive the asymptotic distribution of kernel-based matching estimators and show that bootstrapping is valid to draw inference. This is an additional advantage of this matching method compared to alternative methods, such as nearest-neighbor matching, since it allows to circumvent the issues regarding nearest-neighbor matching raised by Abadie and Imbens (2006). Estimations are done using the PSMATCH2 Stata ado-package by Leuven and Sianesi (2003).

[14] All earnings variables are deflated by gross domestic product (base = 1998), and coded as zero if the person is reported not working. This measure of earnings is one of realized earnings and is frequently used in the literature, despite being a crude measure of productivity—since earnings are only observed for employed individuals.

Table 3 Indicators on the quality of the match, by ALMP

ALMP	Number of treated before[a]	Number of nontreated before[b]	Treated as a percentage before[c]	Probit pseudo-R^2 of nontreated[d]	Probit pseudo-R^2 before[e]	$Pr > X^2$ After[f]	Median bias before[g]	Median bias after[h]	Number of treated lost common to support[i]
Training and retraining	72	696	10.34	0.368	0.035	0.850	27.24	5.69	11
Self-Employment Assistance	362	964	37.55	0.162	0.013	0.985	11.31	2.29	12
Public service employment	445	1,384	32.15	0.359	0.013	0.996	24.64	1.87	6
Employment and relocation services	747	1,028	72.67	0.174	0.017	0.533	9.36	2.88	4

[a] Number of treated, that is, joining an ALMP program in 1999

[b] Number of potential comparisons, that is, persons who had registered at the Employment Bureau in 1999 but did not participate in an ALMP

[c] Treated as a percentage of potential comparisons

[d] Pseudo-$R2$ from probit estimation of the joining probability on X, giving an indication of how well the regressors X explain the participants probability

[e], [f], and [h] are post-matching indicators on kernel-based matching (1 % caliper)

[e] Pseudo-$R2$ from probit estimation of the joining probability on X on the matched samples

[f] P value of the likelihood ratio test after matching. After matching, the joint significance of the regressors is always rejected. Before matching, the joint significance of the regressors was never rejected at any significance level, with $Pr > X^2 = 0.0000$

[g], and [h] Median absolute standardized bias before and after matching, median taken over all regressors X. Following Rosembaum and Rubin (1985), for a given covariate X, the standardized difference before matching is the difference of the sample means in the full treated and non-treated subsamples as a percentage of the square root of the average of the sample variances in the full treated and non-treated groups. The standardized difference *after* matching is the difference of the sample means in the matched treated, that is, the common support, and matched non-treated subsamples as a percentage of the square root of the average of the sample variances in the full non-treated groups:

$$B_{before}(X) \equiv 100 \cdot \frac{\overline{X}_1 - \overline{X}_0}{\sqrt{[V_1(X) + V_0(X)]/2}} \quad \text{and} \quad B_{after}(X) \equiv 100 \cdot \frac{\overline{X}_{1M} - \overline{X}_{0M}}{\sqrt{[V_1(X) + V_0(X)]/2}}$$

Note that the standardization allows comparisons between variables X and, for a given X, comparisons before and after matching

[i] Number of treated individuals falling outside of the common support (based on a caliper of 1 %)

Table 4 Average treatment effects of programs on the employment experience of their participants, by ALMPs (percentage points except where noted)

OUTCOMES	Training and retraining	Self-employment assistance	Public employment	Employment and relocation
Current experience				
Employed	12.47 (−7..00; 29.54)	6.14 (−0.44 12.29)	0.61 (−6.07; 6.29)	8.45 (3.19; 13.90)
Average monthly earnings (in thousand lei)	65.67 (−76.45; 177.64)	37.58 (−13.25; 80.12)	3.10 (−33.87; 33.44)	56.86 (1 0.49; 109.51)
During the two year period 2000–2001				
Employed for at least 6 months	2.53 (−10.55; 27.28)	8.38 (2.29; 14.13)	−7.36 (−14.98; −0.75)	6.22 (2.35 ; 13.52)
Employed for at least 12 months	8.06 (−10.76; 26.91)	7.97 (−0.20; 14.40)	−8.45 (−15.41–1.40)	7.65 (2.11 ; 13.73)
Average monthly earnings (in thousand lei)	164.81 (63.09; 362.20)	43.08 (−9.48; 87.58)	−6.65 (−47.29; 30.33)	87.32 (56.99; 130.21)
Months unemployed	−1.66 (−4.91; 2.79)	−1.82 (−3.00–0.54)	1.95 (0.66; 3.21)	−1.90 (−3.15 ; −0.92)
Months receiving UB payments	−1.01 (−2.24; −0.53)	−0.75 (−1.50; −0.05)	0.21 (−0.60; 0.93)	−0.74 (−1.18 ; −0.29)
Sample size	768	1,311	1,445	1,748

Monthly earnings have been deflated using 1998 deflator. 95% bootstrap confidence intervals in parenthesis

during the years 2000 and 2001 provides additional information on workers' reemployment experiences, and informs us on the workers' employment attachment over that period. We also include average usual monthly earnings during the two-year period prior to the survey as a proxy for worker's productivity. Finally, we include accumulated months of unemployment within the two-year period 2000–2001 to get a measure on how many months of unemployment program participation could save. We also computed accumulated months receiving unemployment benefits (UB) during the two-year period 2000–2001. The outcomes by treatment status are summarized at the bottom of Table 2.

5.2 Mean effects of the programs for their participants

Impacts were estimated as the difference in average outcomes between the treatment and the comparison group. ATT estimates and their bootstrapped 95% confidence intervals are shown in Table 4. The main results are summarized below.

First, we find that ER was successful in improving participants' economic outcomes compared to non-participants in all dimensions. ER had a positive impact both on current employment and on employment during the years 2000–2001. For instance, it increase the probability of being employed at the time of the survey by 8.45 percentage points, which represents a 20% increase in the likelihood of being employed

at the time of the survey.[15] Partly as a result of its positive impacts on employment, the program reduced the number of months unemployed and receiving UB during the 2000–2001. Finally, ER had a positive impact on earnings: it increased average current monthly earnings by 57 thousand lei (or 22%) and average monthly earnings during 2000–2001 by 87 thousand lei (or 28%) compared to the earnings of non-participants.

We also find that SE improved its participants' employment prospects. More specifically, SE increased by 8.38 percentage points (or 12%) the likelihood of being employed for 6 months during the two-year period 2000–2001. This program also reduced the number of months participants were on average unemployed compared to non-participants by almost two months, and the number of months receiving UB payments by almost one month. However, we did not find that SE increased the average monthly earnings of its participants relative to non-participants. This lack of result could be explained by entrepreneurs under-reporting their earnings.

We find that TR has a positive and large impact on the average usual monthly earnings perceived during 2000–2001: it increased the earnings of participants by 165 thousand lei relative to the earnings of non-participants. This is equivalent to 58% higher earnings than non-participants. TR also reduced the length of UB receipt. Unfortunately, due to the small sample size of our sample of TR participants, we lack precision for the other estimates. However, the size of these estimates is consistent with TR being successful in improving participants' economic outcomes compared to non-participants.

In contrast, we find that the PE had a negative impact on employment, and length of unemployment spell during the past two years. These detrimental effects are usually explained by one or a combination of the following two explanations. First, participating in PE may be ineffective insofar as it does not rebuild human capital, boost search efforts or improve the image of the long-term unemployed individual. Second, participation in PE may be a negative signal to the employer (Lehmann 1995).

5.3 Sensitivity analysis

One way to check the robustness of the results is to apply various estimators to the same problem to see whether the results differ. We compared the results obtained by matching to some alternative estimators, which are displayed in Tables A.5 through A.8 in the appendix. The first set of results is gross impact estimates, which were *not* adjusted for observable differences between the participant and non-participants, that is, we use our whole sample of non-participants regardless of whether their baseline characteristics resembled to those of participants. The second set of results is net impact estimates, which were adjusted for demographic and regional differences, and earnings, employment, unemployment and training experiences in 1998 using multivariate ordinary least squares regression (when the dependent variable was continuous) or probit regression (when the dependent variable was a binary variable). The covariates included in the OLS and the probit estimations are the same as those used

---

[15] This result is calculated by dividing the ATT estimate (in this case, 8.45) by the percent of matched non-participants employed at the time of the survey, which is 42.83%.

to estimate the propensity scores in Table 4. The third set of results are net impact estimates that were computed as simple differences between the mean outcome of interest for the participant group and the mean outcome for a non-experimentally matched comparison group selected by the same propensity score method described in section 4, however, we did not use any of the pre-earnings, pre-employment, and pre-unemployment history to match participants to non-participants. The fourth set of results are the estimators presented in Sect. 5.2 and Table 4.

The most obvious overall result is that the unadjusted impact estimates are generally different from the other estimates. For TR, SB and ER, the unadjusted impact estimates were better than the other ones, suggesting that operators "cream off" the most qualified candidates among the unemployed for these particular types of programs. This finding is consistent with other analyses of ALMP in transition economies (O'Leary et al. 1998; Lubyova and van Ours 1999; Kluve et al. 2008, among others). In contrast, adjusting for observable characteristics reduces the detrimental employment and unemployment impact estimates on PE. In particular, the sensitivity of the results to the availability of different covariates indicates that information on the regional location of participants and non-participants was essential when measuring this program's impacts, suggesting that programs like public employment may be used as a regional policy by "bringing work to the workers", that is, creating jobs in high unemployment regions.

In addition, comparing matching estimates calculated including and excluding pre-earnings, pre-employment, and pre-unemployment history reveals that these variables are very essential when measuring the effect of the different programs, as reflected by the fact that excluding them raises the size of impact estimates of all programs. This is particularly true in the case of TR and PE. For example, having information on earnings and unemployment history before participation considerably reduces the positive effect of TR on the employment estimates (sometimes by more than half) and makes them no longer significant. The positive impact of TR on unemployment spell and UB receipt remains significant but its size is reduced by about half. When estimating the impact of PE, we find that having pre-earnings and unemployment history information before participation considerably reduces the negative effect of this ALMP implying that the most disadvantaged workers are selected into the program, and that having information on pre-unemployment history and earnings is crucial to correct for the sample selection.

6 Conclusion

Despite the growing body of ALMPs' evaluations in transition countries, our knowledge is still limited, and based on a reduced sample of evaluations. Moreover, most of these evaluations have concentrated in Central European countries, and although their findings may well apply broadly to South-East European countries, the slower transition process of these countries may limit what some programs can achieve in terms of creating formal employment or increasing wages. By providing estimates of the effects of four ALMPs implemented in Romania in the late 1990s, this paper aims to increase our knowledge on what experiences work in post-communist countries, in general, and, South-East European countries, in particular.

Using a follow-up survey with unusually rich data (for transition economies) and matching methods to control for potential selection bias, we find that three of the four programs—TR, SE, and ER—had success in improving participants' economic outcomes. In contrast, our analysis reveals that PE was found detrimental for the employment prospects of its participants.

Overall, our results are consistent with earlier findings in transition countries.[16] This is particularly true for studies that have evaluated training programs, job search assistance and related employment services, or self-employment assistance programs.[17] These studies find that participating in one of these three ALMPs improves the employment prospects of their participants. For studies evaluating the effectiveness of public employment, the results are mixed.[18] While two evaluations in Poland concluded, as in our study, that PE had a significant negative effect on exiting unemployment and future employment, evaluations in Macedonia, Slovakia, Slovenia, and Ukraine found that a positive employment effect disappeared if the worker did not find a job immediately after the program ended, suggesting that employers may be using PE as a screening device before committing to formal employment. Finally, a small positive impact on employment was found in Bulgaria. However, this positive result was small compared to other ALMPs, and was achieved at a high unit cost.

References

Abadie A, Imbens G. (2006) On the Failure of the Bootstrap for Matching Estimators. Working Paper. Harvard University, Cambridge

---

[16] Many rigorous evaluations have also studied the impact of ALMPs in East Germany. Despite their interest, we do not discuss results from East Germany because of the extremely unique process this country experienced.

[17] We identified four studies that evaluated ER and three studies that evaluated SE in transition economies. Studies evaluating ER took place in the Czeck Republic (Terrell and Sorm 1999), Macedonia (World Bank 2002), Hungary and Poland (O'Leary et al. 1998), whereas studies that evaluated SE occurred in Bulgaria (Walsh et al. 2001), and Hungary and Poland (O'Leary et al. 1998). In the case of TR, we identified eleven different studies, including countries such as, Bosnia and Herzegovina (Benus et al. 2001), Bulgaria (Walsh et al. 2001), Estonia (Leetmaa and Võrk 2004), Hungary (Gill and Dar 1995; O'Leary et al. 1998), Macedonia (World Bank 2002), Poland (Kluve et al. 1999; 2008; Puhani 1998; O'Leary et al. 1998), and Slovak Republic (Lubyova and van Ours 1999.

[18] The studies we identified as evaluating PE in transition economies include: Bulgaria (Walsh et al. 2001), Hungary (O'Leary et al. 1998) Macedonia (World Bank 2002), Poland (Kluve et al. 1999, 2008; Puhani 1998; O'Leary et al. 1998), Slovak Republic (Lubyova and van Ours 1999), Slovenia (Vodopivec 1999), and Ukraine (Kupets 2000).

Bechterman G, Olivar K, Dar A (2004) Impact of Active Labor Market Programs: New Evidence from Evaluations with Particular Attention to Developing and Transition Countries. Social Protection Discussion Paper Series 0402

Bender S, Bergemann A, Fitzenberger B, Lechner M, Miquel R, Speckesser S, Wunsch C (2005) Uber dieWirksamkeit von Fortbildungs- und Umschulungsmaßnahmen. Beiträge zur Arbeitsmarkt-und Berufsforschung. IAB, Nürnberg

Bender S, Klose C (2000) Berufliche Weiterbildung für Arbeitslose – ein Weg zurück in die Beschäftigung? Analyse einer Abgängerkohorte des Jahres 1986 aus Maßnahmen der Fortbildung und Umschulung mit der ergänzten IAB–Beschäftigtenstichprobe 1975–1990. Mitteilungen aus der Arbeitsmarkt– und Berufsforschung, 33(3):421–444

Benus J, Rude J, Patrabansh S (2001) Impact of the Emergency Demobilization and Reintegration Project in Bosnia & Herzegovina. Abt Associates

Boeri T, Burda M (1996) Active labor market programs, job matching, and the Czech miracle. Eur Econ Rev 40:805–817

Burda M , Lubyova M (1995) The Impact of Active Labor Market Policies: a Closer Look at the Czech and Slovak Republics. Discussion Paper No. 1102, Centre for Economic Policy Research, London

Brown D, Earle J, Gimpelson V, Kapeliushnikov R, Lehmann H, Telegdy A, Vantu I, Visan R, Voicu A (2006) Nonstandard Forms and Measures of Employment and Unemployment in Transition: A Comparative Study of Estonia, Romania, and Russia. Budapest Working Papers on the Labour Market from Institute of Economics 602, Hungarian Academy of Sciences

Dar A, Tzannatos Z (1999) Active Labor Market Programs: A Review of the Evidence from Evaluations. Social Protection Discussion Paper 9901. The World Bank, Washington, DC

Earle J, Pauna C (1996) Incidence and duration of unemployment in Romania. Eur Econ Rev 49:829–837

Earle J, Pauna C (1998) Long-term unemployment, social assistance and labor market policies in Romania. Empir Econ 23:203–235

Fay R (1996) Enhancing the Effectiveness of Active Labor Market Policies: Evidence from Program Evaluations in OECD countries. Labor Market and Social Policy Occasional Papers 18, Paris

Fitzenberger B, Speckesser S (2007) Employment effects of the provision of specific professional skills and techniques in Germany. Empir Econ 32:529–573

Gill I, Dar A (1995) Cost and Effectiveness of Retraining in Hungary. Internal Discussion Paper IP-155, Europe and Central Asia Region, The World Bank

Heckman J, Ichimura H, Todd P (1997) Matching as an econometric evaluation estimator: evidence from evaluating a job training program. Rev Econ Stud 64:605–654

Heckman J, Smith J (1999) The preprogram earnings dip and the determinants of participation in a social program: implications for simple program evaluation strategies. Econ J 108:313–348

Heckman J, Smith J (2004) The determinants of participation in a social program: evidence from a proto-typical job training program. J Labor Econ 22:243–298

Irac D, Minoiu C (2006) Risk Insurance in a Transition Economy: Evidence from Rural Romania. Department of Economics, University of Columbia

Imbens G (2004) Nonparametric estimation of average treatment effects under exogeneity: a review. Rev Econ Stat 86:4–29

Jürges H (2005) Objects in the mirror are closer than they appear: unemployment, retrospective error, and life satisfaction. Schmollers Jahrb 125:157–165

Katz L (1994) Active labor market policies to expand employment and opportunity. In: Proceedings, Federal Reserve Bank of Kansas City, issue January, pp 239–322

Kluve J (2006) The Effectiveness of European Active Labor Market Policy. IZA Discussion Paper 2018

Kluve J, Lehmann H, Schmidt C (1999) Active Labor market policies in Poland: human capital enhancement, stigmatization, or benefit churning? J Comp Econ 27:61–89

Kluve J, Lehmann H, Schmidt C (2008) Disentangling treatment effects of polish active labor market policies: the role of labor force status sequences. Labour Econ 15:1270–1295

Kupets O (2000) The impact of active labor market policies on the outflows from unemployment to regular jobs in Ukraine Thesis. Nation University. Kiev-Mohyla Academy

Lehmann H (1995) Active Labor Market Policies in the OECD and in Selected Transition Economies. Policy Research Working Paper 1502, World Bank

Lechner M, Miquel R, Wunsch C (2005a) Long-Run effects of public sector sponsored training in West Germany. IZA Discussion Paper 1443

Lechner M, Miquel R, Wunsch C (2005b) The curse and blessing of training the unemployed in a changing economy: The case of East Germany after Unification. Discussion Paper, University of St. Gallen

Leetmaa R, Võrk A (2004) Evaluation of active labour market programmes in Estonia. Mimeo

Leuven E, Sianesi B (2003) Psmatch2: Stata module to perform Mahalanobis and propensity score matching, common support graphing, and covariate imbalance testing. Available at http://ideas.repec.org/c/boc/bocode/s432001.html

Lubyova M, van Ours JC (1999) Effects of active labor market programs on the transition rate from unemployment into regular jobs in the Slovak Republic. J Comp Econ 27:805–837

Martin JP (1998) What Works among Active Labor Market Policies: Evidence form OECD Countries' Experiences. Labor Market and Social Policy Occasional Papers No. 35. Paris

Martin J, Grubb D (2001) What Works and For Whom: A Review of OECD Countries' Experiences with Active Labor Market Policies. Office of Labor Market Policy Evaluation. Working Paper 2001/14

Nesporova A (2002) Why Unemployment Remains so High in Central and Eastern Europe. International Labor Office, Employment paper 2002/43

O'Leary CJ, Kolodziejczyk P, Lazar G (1998) The net impact of active labor programs in Hungary and Poland. Int Labor Rev 137:321–346

Oyer P (2004) Recall bias among displaced workers. Econ Lett 82:392–397

Plesca M, Smith J (2007) Evaluating multi-treatment programs: theory and evidence from the U.S. Job training partnership act experiment. Empir Econ 32:491–528

Puhani P (1998) Advantage through Training? A Microeconometric Evaluation of the Employment Effects of Active Labor Market Programs in Poland. ZEW Discussion Paper no. 98–25, Manheim

Romania National Institute of Statistics (2006) Quality Report on 2006 Structure of Household Labor Survey

Rosenbaum P (1995) Observational Studies. Springer Series in Statistics, New York

Rosenbaum P, Rubin D (1983) The central role of the propensity score in observational studies for causal effects. Biom 27:33–60

Roy A (1951) Some thoughts on the distribution of earnings. Oxf Econ Pap 3:135–145

Rubin D (1974) Estimating causal effects of treatments in randomized and nonrandomized studies. J Educ Psychol 66:688–701

Rubin D (1977) Assignment to treatment group on the basis of a covariate. J Educ Stat 2:1–26

Sianesi B (2004) An evaluation of the Swedish system of active labor market programs in the 1990 s. Rev Econ Stat 86:133–155

Smith J, Todd P (2005) Does matching overcome LaLonde's critique of nonexperimental estimators? J Econom 125:305–353

Song Y (2007) Recall bias in the displaced workers survey: are layoffs really lemons? Labour Econ 14:335–345

Terrell K, Munich D (1995) An Overview of Labor Market Policies in the Czech Republic. Paper presented at the OECD Technical Workshop, "What can we learn from the experience of transition countries with labour market policies," Vienna

Terrell K, Sorm V (1999) Labor market policies and unemployment in the Czech Republic. J Comp Econ 27:33–60

Vidovic H (2004) Labour Markets and Employment Development in South East Europe in Survey of Economic and Social Challenges in South-East Europe

Vodopivec M (1999) Does the Slovenian public work program increase participants' chances to find a job? J Comp Econ 27:113–130

Walsh K, Kotzeva M, Dolle E, Dorenbos R (2001) Evaluation of the Net Impact of Active Labor Market Programs in Bulgaria. Ministry of Labor and Social Policy, Bulgaria

World Bank (2002) Evaluation of Active Labor Programs in Macedonia. Final Report. Social Reform and Technical Assistance Project. IDA-2722-MK