What Works Best For Getting the Unemployed Back to Work: Employment Services or Small-Business Assistance Programmes? Evidence from Romania

Nuria Rodriguez-Planas[1]
Universitat Autònoma de Barcelona,
IZA and FEDEA

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Abstract

Recent empirical evidence has found that employment services and small-business assistance programmes are often successful at getting the unemployed back to work. One important concern of policy makers is to decide which of these two programmes is more effective and for whom. Using unusually rich (for transition economies) survey data and matching methods, I evaluate the relative effectiveness of these two programmes in Romania. While I find that employment services (ES) are, on average, more successful than a small-business assistance programme (SBA), estimation of heterogeneity effects reveals that, compared to non-participation, ES are effective for workers with little access to informal search channels, and SBA works for less-qualified workers and those living in rural areas. When comparing ES to SBA, I find that ES tend to be more efficient than SBA for workers without a high-school degree, and that the opposite holds for the more educated workers.

Key words: Active labour market programmes, evaluation, propensity score matching, transition economies, and treatment effects.

JEL classification: J21, J23, J31, J64, J65, J68

Contact e-mail: Nuria.Rodriguez@uab.es

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I. Introduction

In recent years, there has been a substantial increase in the empirical evidence on the effectiveness of active labour market programmes (ALMPs) in developed, developing and transition economies.[1] This improvement can be explained by the increased availability of data, the improvements on data quality, and recent developments on evaluation methodology. However, most of these studies focus on comparing the labour market outcomes of unemployed individuals who participate in an ALMP with other unemployed individuals who do not participate in an ALMP—at least during a pre-determined span of time.

While evaluating the effects of ALMPs relative to non-participation is an interesting question per se, policy makers may well be more interested on what are the relative effects of two different types of programmes, as well as the suitability of these programmes for different target groups.[2] More specifically, for the two ALMPs—that is, small-business assistance programmes and employment services—that recent empirical studies indicate are often successful at getting the unemployed back to work, a useful policy question may be: which one is more effective and for whom. This paper provides some evidence on which of these two types of programmes' works best for different population subgroups in Romania. The results offer interesting policy recommendations for implementing these programmes both in transition countries and countries with large informal sectors, such as developing countries. The focus is on the direct effects of the programmes; no attempt is made to assess the general equilibrium implications.

There are considerable differences in the design of these two types of programmes. On the one hand, small-business assistance programmes are usually intended to support the start-up and development of self-employment endeavours or micro-enterprises. They usually provide counselling and assistance in developing and implementing a business plan, and often include some form of financial assistance. Although the use of these programmes has been limited compared to other ALMPs, their popularity (as well as the number of empirical evaluations available) has recently increased (Kluve, 2006). Employment services, on the other hand, include different types of measures aimed at improving job search efficiency. They usually include the following types of services: job clubs, job-search courses, counselling, testing, and assessment. Moreover, because of their relatively low costs, employment services tend to be the most cost-effective (Martin, 1998, Dar and Tzannatos, 1999, and Kluve, 2006, among others).

Despite the institutional differences between these two programmes, recent empirical evidence highlights their success at getting the unemployed back to work. According to Kluve, 2006, a consistent result for both Europe and the US are the positive effects for employment services, and small-business assistance programmes. Martin and Grubb, 2001, also find that these programmes are successful at getting the unemployed back to work in developed countries. In addition, Dar and Tzannatos, 1999, and Betcherman, Olivas and Dar, 2004, find that both of these programmes tend to be successful in developing, and transition countries.

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[1] See Katz, 1994, Fay, 1996, Martin, 1998, Dar and Tzannatos, 1999, Martin and Grubb, 2001, Kluve and Schmidt, 2002, and Betcherman, Olivas, and Dar, 2004, for good reviews of the literature.

[2] A methodological question also arises since most of these studies use non-experimental approaches to estimate the impact of the programmes. While non-participants may well be intrinsically different to participants, the difference between participants of different types of ALMPs may not be as large (see discussion in Section IV).

To my knowledge, there is no theoretical and comparative empirical research devoted to analysing the relative effects of employment services and small-business assistance programmes.[3] The main reason for this is that the latter type of programmes has, until recently, been seldom used, and thus data allowing comparison of these two types of programmes in the same country are rarely available. While cross-country studies of these two types of programmes are possible, differences in labour market conditions, institutions, evaluation designs, availability of outcome variables, and time periods seriously complicate the analysis.

Romania can be used to study the differences between employment services and small-business assistance programmes because these two programmes were the first major ALMPs implemented in Romania on a large national scale after the 1989 Revolution. Moreover, these programmes were targeted at more or less the same population of unemployed. Furthermore, the experience of Romania ought to be of interest to policymakers of other countries, especially transition economies, which have suffered soaring labour surplus after social, economic, and political reforms, and developing countries that, like transition economies, tend to have large informal sectors. Finally, a rich data set (collected specifically for this evaluation) provided good quality data on key variables—such as earnings for both the employed and the self-employed, and allowed me to track individuals' earnings and employment status at different points in time over a four-year period.

The data, a random sample of 3,357 persons who registered at the Employment Bureau during 1999, was collected during January and February 2002. Thus, we observe individuals *at least* 24 months after the programmes started. About two fifth of this sample (1,408 individuals) were ALMP participants whose programme contract began in 1999. The rest of the sample—the potential comparison group—were 1,949 persons who were registered at the Employment Bureau around the same time and in the same county than participants but who had *not* participated in an ALMP. This database was previously used by Benus and Rodriguez-Planas, 2007, (BR, hereafter) for a microeconomic evaluation study of several active labour market policies. Their study focuses on the effects of ALMPs relative to non-participation, and finds that participants of both programmes improved participants' economic outcomes compared to non-participants. However, their paper does not address the relative effectiveness of these two programmes, nor does it discuss theoretical implications of both programmes and contrasts them with heterogeneity results.

Since the primary objective of both programmes is to get the unemployed individuals back into the primary labour market, and with a job, at least implicitly as good as the previous one, the current analysis focuses in outcomes that measure workers' reemployment probabilities (in paid or self-employed jobs), and their earnings at the new job. These outcomes are measured 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. I also include duration of the unemployment spell and months receiving unemployment benefits during the two-year period 2000-2001.

[3] Microeconometric studies looking at the relative effects of programmemes in one country include, among others, Bonnal, Fougère and Sérandon, 1997, Carling and Gustafson, 1999, Melkersson, 1999a,b, Brodaty, Crépon and Fougère, 2000, Frölich, Heshmati and Lechner, 2000, Johansson and Martinsson, 2000, Gerfin and Lechner, 2002, Larsson, 2003, Carling and Richardson, 2004, Gerfin, Lechner and Steiger, 2005, and Sianesi, 2005. However, none of these studies compares the relative effect of small business assistance programmes compared to employment services.

I base the analysis on the conditional independence assumption (CIA), and use matching methods to estimate the average treatment effect. A part of the paper is devoted to discussing the plausibility of the CIA in this context. One of the biggest challenges when evaluating ALMPs in transition economies is the quality and quantity of data—see Kluve, Lehmann and Schmidt, 1999, or Earle and Pauna, 1996, among others, for discussion on the poor quality of ALMPs' data in transition economies. I argue that the data used contains important baseline information—in particular, pre-treatment earnings, employment history and experience information—making the CIA assumption more plausible.

The analysis reveals that average effects for the population as a whole may hide statistically and economically significant differences across subgroups. While, I find that employment services (ES) are, on average, more successful than small-business assistance programmes (SBA), estimation of heterogeneity effects reveals that, compared to non-participation, ES are effective for workers with little access to informal search channels—such as young workers, and those living in rural areas, and SBA works for less-qualified workers or those living in rural areas. In addition, I find that while both programmes have positive effects for workers with and without a high-school degree, ES is superior to SBA for the latter, while the opposite is true for the former.

In summary, this paper contributes to the literature in several ways. First, by focussing on the two programmes, I am able to analyse their differences in participant selection and outcomes in a more profound and informative way than BR. Second, following Gerfin, Lechner and Steiger, 2005, I relate these differences to the different institutional set-ups and discuss theoretical implications, which are then empirically contrasted with the heterogeneity effects. This analysis offers evidence consistent with improved job matching theory for ES, and human capital and positive signalling theories for SBA. Finally, the findings in the paper suggest important policy implications since they provide some guidance on which populations would benefit from ES in economies with large informal sectors, and which type of workers are most likely to succeed when participating in SBA in transition economies.

This paper is organized as follows. The next section presents an overview of the Romanian labour market, focusing on self-employment, unemployment and the ALMPs under evaluation. Section three summarises previous empirical findings. Section four describes the data, sample selection and displays the descriptive statistics. Section five discusses the economic evaluation strategy and the empirical implementation. Section six displays the results from a multiple treatment evaluation framework using a 'matching propensity score' estimator. Section seven concludes with a discussion on policy implications and cost-effectiveness.

II. Economic and Institutional Background

II.1. The Economic Context

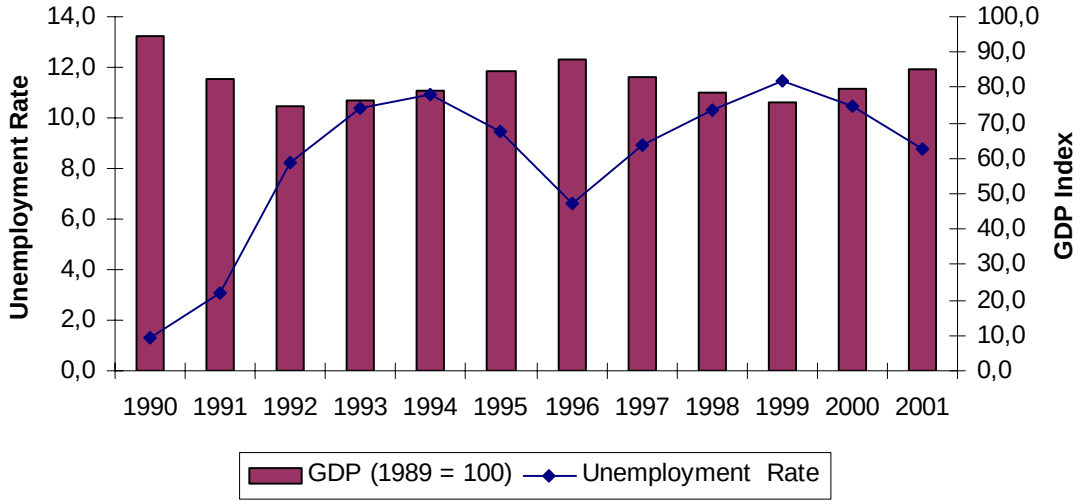
Since 1989, Romania has been in transition to a market economy. This transition has been slow partly as a result of Romania's stop-and-go approach to the restructuring and the reform process. Since the 1989 Revolution, successive governments have adopted a cautious approach to market-oriented reforms.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

unfavourable set of initial conditions inherited from the previous regime.

As shown in Figure 1, 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 (CMEA), Romania applied a macro-stabilization programme and experienced a partial economic recovery beginning in 1992, similar to the one observed in leading transition economies in Central Europe. However, 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 privatisation, 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.

Figure 1

Romania Economic Indicators, 1990-1999



The collapse in output at the beginning of the 1990s prompted an increase in unemployment. As seen in Figure 1, following the fall in output, registered unemployment soared and reached over 10 percent of the labour force in 1994. The unemployment rate then fell temporarily during 1995 and 1996, only to rise rapidly thereafter, reaching 11.5 percent in 1999. Since then, the registered unemployment rate has fallen gradually to 9 percent of the labour force in 2001.

Moreover, 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 programmes. Even though these two policies succeeded in limiting the increase in registered unemployment, it pushed workers out of the labour force and into low productivity jobs, primarily in agriculture.[4]

[4] Since this policy approach was abandoned in the late 1990s, some of the initial imbalances are in the process of

Second, a high share of Romania's employment was in subsistence agriculture—the share of agricultural employment in Romania in 2001 was 42 percent of total employment (up from 28 percent of total employment in 1989). This suggests the existence of a two-tier system consisting of a large number of people involved in low productivity jobs and subsistence agriculture coexisting with large, potentially profitable but unreformed, farms. And third, the existence of borderline 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 labour market performance.[5]

II.2. The Small and Medium Enterprise Sector[6]

Despite the slow restructuring process, the Romanian small and medium enterprise (SME) sector has been dynamic, and its contribution to employment growth has surged. While SMEs (enterprises with less than 250 employees) accounted for 24% of total employment in 1995, they represented about 46.9% of total employment and 55.9% of total turnover in the economy by 2000. The private sector contribution to GDP has also grown considerably. From contributing 16.4% of GDP in 1990, its contribution almost tripled by 1995 (at 45.3% of GDP) and reached 65.5% in the year 2000.

However, in spite of the positive contribution of the SME sector to economic transformation, its relative size in Romania is still small compared with the other transition economies. Many factors explain why Romanian SMEs' sector has been slow to develop. A brief summary of the most relevant ones follows.[7] First, Romania lacked of private initiative or small firms prior to 1990. In contrast with other socialist countries where some entrepreneurial activities existed during the last years of the communist years, private enterprise and entrepreneurship was first legalised in Romania during 1990. Second, Romania's legislative framework for setting up SMEs has been complex, cluttered, and volatile, adding an additional source of uncertainty to entrepreneurship. Third, the taxation level of private entrepreneurs has been high, and additional taxation has been imposed by the "special funds contributions"—ad-hoc special purpose taxes levied directly by ministries or other public institutions. Fourth, there have been considerable social and political pressures against restructuring of state firms, which has led to a slow privatisation process. Fifth, the entrepreneurial tradition has been very weak in Romania, since the communist regime forced the artisans to merge into the state productive structures, breaking the chain of familiar handicraft tradition. A consequence of this was that, by 1990, Romania lacked of sons of traditionally entrepreneurial families—the backbone of Western European entrepreneurs. Finally, SMEs' financing has been limited and expensive. Because of the high and variable inflation rates, interest rates on lending have been prohibitive for many SMEs. Moreover, access to bank loans was limited to SMEs and when available, they were generally short-term,

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being corrected.

[5] See Brown et al., 2006, for a careful study on nonstandard forms and measures of employment and unemploymen in Romania.

[6] In Romania, most enterprises in this sector were micro-firms (with less than 10 employees). For instance, in the year 2000, 93.6% of SMEs were micro-firms, 5% were small (with 10 to 49 employees), and 1.4% were medium firms (with 50 to 249 employees). Moreover, 97.4% of total SMEs was private, 0.3% was state-owned, and 2.3% were mixed firms.

[7] For thourough studies on the development of SMEs during the transition in Romania, see Ahrend and Martins, 2003, Dochia, 2000, Ferrari, 1999, Mitrut and Constantin, 2006, Nagarajan and Meyer, 1997a, among others.

small and required a collateral (Nagarajan and Meyer, 1997).

II.3. The Institutional Environment

To address the problem of soaring labour surplus, the Romanian government soon developed social safety programmes, including labour market programmes to help the unemployed during this transition period. The programmes combined social insurance and means-tested income support with active policies aimed at increasing labour demand and improving matching.

The Romanian Unemployment Programme

As early as 1991, the Ministry of Labour and Social Protection adopted the Romanian Unemployment Programme. This programme was not a pure social insurance programme since it contained provisions for means testing.[8] According to this programme, unemployed individuals were eligible for financial support through *unemployment benefits, allowance for vocational integration* and *support allowance*. To be eligible, an individual had to: be registered at the local Labour Office, be aged eighteen and over, have an income less than half of the indexed national minimum wage, be unemployed due to liquidation or a lay-off, be employed at least six months during the last twelve months, or be a recent graduate from school or university unable to find suitable employment. Unemployment benefits were paid for a maximum duration of nine months. The level of these benefits ranged from 50 to 60 percent of the average monthly salary during the last three months of employment for laid-off 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 percent of the indexed minimum wage) for a maximum period of 18 months.[9]

Active Labour Market Programmes

With the introduction of social insurance and means-tested income support, the Ministry of Labour and Social Protection also adopted several active policies aimed at increasing labour demand for youths, improving matching by providing retraining for unemployed individuals, and stimulating job creation through credits to businesses.[10] However, the extent of these active programmes remained very limited (as discussed in Earle et al., 1998). And the need for additional and more diversified measures to support employment emerged progressively and became particularly urgent after 1996-1997 when privatisation and restructuring accelerated and resulted in massive layoffs.

In the late 1990s, the Romanian government launched the real start of active labour programmes (ALMPs) on a large national scale. The two major programmes offered were (1)

[8] It contained two means-testing elements: a ceiling on land ownership, and a ceiling on personal income of half the minimum wage. However, according to officials from the Ministry of Labor and from local offices the ceiling on land ownership was seldomly binding as the inefficiency and corruption of the process of land privatization lead to many new owners without titles. Thus, there was no proof that the land was theirs, although they may have worked it unofficially. The manjor consequence of the ceiling on personal income was widespread income under-reporting.

[9] See Earle and Pauna (1998) for a detailed description and thourough analysis of the unemployment benefit and support allowance programme in Romania.

[10] The first active measure to be adopted was training and retraining for the unemployed in 1991. Then in 1992, a youth measure was initiated through the labor offices, the Wage Subsidy Programme for New Graduates. In 1995, a programme offering loans to small- and medium-sized enterprises that hired at least 50 percent of the new hires from the unemployment pool was also launched.

employment services (ES), and (2) small business assistance (SBA). These programmes offered counselling and services aiming to facilitate job placement, and job search skills (the former), and business start-ups (the latter) to unemployed workers. Altogether, these two programmes served more than 80 percent of the unemployed who received some kind of active labour market programme in Romania during that period.[11]

Implementation of ALMPs

Implementation of ALMPs began in 1997 by the National Agency for Employment and Vocational Training and the county agencies for Employment and Vocational Training. These services were not provided by the county agencies themselves, but were contracted out to public or private service providers. The county agencies were responsible for the public announcements of the tenders, conducting the tendering process, and contracting out the ALMPs.

Contracts to service providers were awarded with built-in incentives to improve labour market impact such as negotiated levels of job placement and business start-up. Thus, service providers were likely to select those unemployed individuals most likely to succeed in completing their programme and accessing employment. As we shall see in Section IV, this will cause selection bias due to a correlation of individual programme participation with the outcomes under investigation.

There were three requisites that prevented duplication of payment and services. First, individual clients could not receive income support payments if they were receiving other types of state financed income support, such as unemployment benefits. Second, individuals could not participate in more than one programme. And third, individuals were not allowed to participate more than once in a programme in a period of 24 months.

II.4. The Programmes

Table 1 summarises the key characteristics of the two programmes under evaluation. Below, I provide a more detailed description of the programmes.

Employment Services

Clients eligible for this service were offered a variety of employment services, including job and social counselling, labour market information, job search assistance, job placement services, and relocation assistance. The duration of these services was limited to 9 months per individual. In addition, those clients receiving relocation assistance could be reimbursed for expenses associated with moving to another community—up to $500 U.S. dollars equivalent in lei per family (based on submission of receipts). The programme also offered up to two months of salary at the minimum wage.

Service providers had to demonstrate minimum capabilities to be service providers, such as staff qualifications, facilities, financial viability, and placement capability. Moreover, they

[11] Even though two additional programmes (training services and public employment) were also offered at that time, their implementation was on a considerably smaller scale as they served less than 20 percent of the clients. Moreover, these two programmemes were targeted to more disadvantaged populations—such as the young, the low skilled workers and the long-term unemployed. An evaluation of these programmemes can be found in BR.

had to agree to a negotiated job placement rate of at least 10 percent. Eligible costs included staff and administrative personnel costs, rent and utilities, consumable materials, client transportation for job interviews, non-durable goods, and depreciation of capital equipment up to 20 percent per year.

Small Business Assistance Programmes

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 entrepreneurs who intended to start, or who had started businesses during the past 12 months. There were also provisions for short-term working capital loans of up to $25,000 U.S. dollars to programme participants.

Service providers had to agree to a negotiated business start-up rate of at least 5 percent of clients initially contacted. Eligible costs included personnel services, transportation costs, rent and utilities, consumable materials and non-durable goods, and capital depreciation up to 20 percent per year. Maximum length of initial contract was 12 months. Costs per client had to be specified in all contracts, however unit costs could be identified for different categories of services by each service provider based on the understanding that all clients did not need full services and some may drop out after initial contacts. Reimbursement to service providers was based on the contracted average cost per client, for each category services.

Utilization of the Programmes

As indicated in Table 2, between these two ALMPs, there were 180 contracts completed as of September 1, 2001, and close to fifty-two thousand clients served. The placement rate among these contracts was close to one fifth. ES provided assistance to 31,679 individuals at an average cost of only 123.74 thousand lei per client (about 12 US dollars per client). In contrast, SBA served a smaller number of clients (20,293 clients) and the cost per client for this programme was 179.15 thousand lei per client (about 17 US dollars per client).[12]

III. Previous Empirical Evidence[13]

III.1. Employment Services

Employment services include different types of measures aimed at improving job search efficiency. They usually include the following types of services: initial interviews at the employment offices, job clubs, job-search courses, counselling, testing, and assessment. Such services may be combined with increased monitoring and enforcement of the job-search requirement for receipt of unemployment benefits. Given that, in many countries, the private sector provides matching services successfully to some segments of the population, public employment services are often justified as being beneficial for the more disadvantaged segments of the population, such as the low-skilled and the long-term unemployed (Fretwell and Goldberg, 1994, Van der Berg and Van der Klaauw, 2006).

[12] All costs figures have been deflated using 1998 deflator.

[13] All of the reviewed studies in this section evaluate the impact of participation in either programme compared to non-participation.

In developed countries, most studies find that employment services were successful in helping the unemployed workers find better-paid jobs.[14] This is true for most experimental studies in Canada, Sweden, the United Kingdom, and the United States.[15] Two exceptions are: (1) the evaluation of three different employment service strategies in Washington D.C. and Florida during 1995 and 1996, which found uneven impacts on employment and earnings (Decker et al., 2000); and (2) the evaluation of job-search assistance in Holland, which did not find significant results for unemployment insurance recipients with relatively good labour market prospects (Van der Berg and Van der Klaauw, 2006). Quasi-experimental studies also find successful outcomes for employment services in Australia (Fay, 1996), Austria (Weber and Hofer, 2003 and 2004), France (Crépon, Dejemeppe and Gurgand, 2005, and Fougère, Pradel, and Roger, 2005), Germany (Caliendo, Hujer, and Thomsen, 2005), the Netherlands (OECD, 2003), New Zealand (NZ DOL, 1995), and the United Kingdom (Van Reenen, 2003, Blundell, Costas Dias, Meghir, and Van Reenen, 2004, and Dolton, and O'neil, 2002). However, no positive effect was found in Canada or Holland (Fay, 1996).

Even though the evidence is scarcer in transition countries, the results seem to indicate that employment services are also useful into helping the unemployed find work. To my knowledge, five non-experimental studies have evaluated self-employment assistance programmes in the following transition countries: Bulgaria (Walsh, Kotzeva, Dolle, and Dorenbos, 2001), Czech Republic (Terrel and Storm, 1999), Hungary and Poland (O'Leary, 1998a), Macedonia (World Bank, 2002), and Romania (BR, 2007). All of these programmes found positive impacts of this type of services on improving employment prospects of participants. However, of the three evaluations that estimated the impact of these services on earnings, only in Poland a positive effect was found.[16]

According to Betcherman, Olivas, and Dar, 2004, the two studies that evaluate employment services in developing countries (Brazil and Uruguay) show that in countries with large informal sectors, public employment services may have limited reach as workers may prefer other channels of job search (Woltermann, 2002). In addition, when positive results are found, they are linked to better-educated workers (Fawcett, 2001).

When targeted to specific groups, employment services seem to work best for women on welfare (in the United States) and the long-term unemployed (in New Zealand, the Netherlands, and the United Kingdom). In contrast, they do not seem to work well for young workers (in Canada, and Holland), or mass layoffs (in Canada). However, because these studies were evaluating employment services targeted to a specific population subgroup, they were unable to analyse the relative effectiveness of the programme across different subgroups.

To identify the population subgroups that are most likely to benefit from a specific active labour market programme, it is worthwhile estimating the relative impact of a programme across subgroups. This approach reduces the sensitivity of the results to differences in labour market conditions, institutions, evaluation designs, availability of outcome variables, and time periods.

[14] This is particularly true under favorable economic conditions and when sufficient public funds are available, as pointed out by Dar and Tzannatos, 1999.

[15] See Human Resources Development Canada, 1996, for a review of the Canadian results; Björklund and Régner, 1996, for a review of the Swedish evidence, Dolton and O'Neil, 1996, for U.K. evidence; and Meyer, 1995, and Heckman, Lalonde and Smith, 1999, for reviews of the U.S. studies. However, it is worthwhile noting that the Canadian evidence finds that the earnings gains eventually fade away.

[16] The evaluations conducted in Romania and Hungary found no impact on earnings.

When looking at studies that evaluate heterogeneity results, employment services are more beneficial for less educated males (in Australia), low skill and less educated workers (France), women (in Hungary and Poland), younger workers (in Romania), higher educated youth (in Uruguay), workers with histories of short-term unemployment (in Austria and Romania), and those living in rural areas (in Romania).

III.2. Small Business Assistance

Small-business assistance programmes are usually intended to support the start-up and development of self-employment endeavours or micro-enterprises. They usually provide counselling and assistance in developing and implementing a business plan, and often include some form of financial assistance. Although the use of these programmes has been limited compared to other ALMPs, their popularity (as well as the number of empirical evaluations available) has recently increased (Kluve, 2006).

When focusing on scientific evaluations (that is, evaluations with a control group), there are relatively few studies that evaluate the impact of participating in a small business assistance programme.[17] About half of these studies focus on business development of participants rather than future employment and earnings of participants, in which case either the business survival-rate indicator is not a net indicator (since it does not involve a comparison with a control group) or if compared to a control group, it is one constructed with businesses started by individuals who were not previously unemployed.

The evidence in industrialized countries suggests that small-business assistance programmes work, regardless of whether they focus on the business survival rate indicator or the employment indicator. On the one hand, the evaluations that assess whether these programmes can effectively support business start-up tend to find that the survival rate is high. For instance, studies in Canada (Graves and Gauthier, 1995), and Germany (Pfeiffer and Reize, 2000, and Reize, 2004) find that participants' business survival rate is high.[18] On the other hand, studies that evaluate the impact of these on employment and earnings find that positive results for employment and mixed results for earnings.[19] For instance, controlled experiments in the United States suggest that small business assistance is successful for a small group of unemployed individuals—such as, highly educated prime-aged men, indicating that this measure may be most appropriate for unemployed workers who have entrepreneurial skills and the motivation to survive in a competitive environment (Fay, 1996, and Wilson and Adams, 1994). Quasi-experimental studies find similar results: an evaluation of a self-employment programme offered to unemployment insurance claimants in Delaware, Maine, Maryland, New Jersey, New York, Oregon, and Pennsylvania in the late 1990s found that participants were four times more likely than non-participants to have obtained employment of any kind—either wage or salary job or self-employment, (Kosanovich, et al., 2001). In addition, studies in Finland (Nätti, Aho, and Halme, 2000), Germany (Baumgartner, and Caliendo, 2007), New Zealand (Perry, 2006), in the United Kingdom (Payne, 2000), and Spain (Cueto and Mato, 2006) also find successful results for small-business assistance combined with start-up grants.

[17] In their 2004 paper, Betcherman, Olivas, and Dar have identified 13 scientific evaluations of small business assistance programmes. Since then, I have identified about half a dozen more.

[18] There are a few exceptions among older studies in Denmark and France (Wilson and Adams, 1994). In addition, Meager, Bates and Cowling, 2003, evaluated business start-up subsidies to young people in the United Kingdom and did not find that they had a significant impact on subsequent employment or earnings chances.

[19] Some caution has been to made when evaluating earnings results for the self-employed as they tend to under-report earnings relative to wage and salary workers.

The results from scientific evaluations in transition countries are also consistent with small business assistance programmes increasing the probability of reemployment. These studies found that self-employment assistance programmes in Bulgaria (Walsh, Kotzeva, Dolle, and Dorenbos, 2001), Hungary and Poland (O'Leary, 1998a), and Romania (BR, 2007) were successful at getting the unemployed back to work. However, the evidence on earnings is mixed. While the Romanian study found no effect on earnings, the study from Hungary found a negative effect, and the study from Poland found a positive one. The Bulgarian programme did not estimate the impact of the programme on earnings.

The little empirical evidence found in developing countries seems to indicate that small business assistance improves the outcomes of its participants with entrepreneurial skills and motivation. For instance, Almeida and Galasso, 2007, find that a very small subset of former welfare beneficiaries in Argentina were attracted to a small-business assistance programme (mainly female household heads and highly educated workers), and that this programme was successful for only a subset of participants, namely younger and more educated workers, and those for whom self-employment was related to some ongoing activity. In addition, a business-training programme targeted, not to the unemployed, but to female entrepreneurs participating in a Peruvian village banking programme indicates that the treatment improved business knowledge, revenues, repayment, and client retention rates (Karlan and Valdivia, 2006).

As already mentioned, heterogeneity analysis suggests that these programmes work best for unemployed workers who have entrepreneurial skills and the motivation to survive in a competitive environment, such as, highly educated prime-aged males in developed countries or young and more educated workers in Argentina. The evidence in transition economies is more mixed and finds that these programmes are beneficial for a more diversified group of workers, such as women (in Hungary, Poland, and Romania), older workers (in Hungary, and Poland), workers without a high-school diploma (in Romania), and those living in rural areas (in Romania).

IV. The Data and Descriptive Statistics

IV.1. Data Source

This study uses data from a follow-up survey of registered unemployed specifically designed for this evaluation.[20] I shied away from using existing data from the Ministry of Labor and Social Protection for several reasons. First, I was concerned on the quantity and quality of the existing data. But, more importantly, data from the Ministry lacked several key variables needed for our analysis, such as earnings for both the employed and the self-employed.[21]

Compared with existing data from the Ministry, our follow-up survey data provided much more detailed characteristics of the unemployed individuals, and observed their earnings and employment status at different points in time over a four-year period. Moreover, since official

[20] Survey data of registered unemployed specifically collected for a study was also used in Earle and Pauna, 1996 and Earle and Pauna, 1998.

[21] The Ministry of Labor and Social Protection provides some aggregate classifications on the unemployed each month. These data, although useful, are limited to a few basic dimensions and allow few inferences concerning the origins, causes, and incidence of unemployment to be drawn.

data is collected in the local labour offices, I was concerned that respondents would be more reluctant to fully disclose their earnings to public authorities (as part of tax avoidance strategy) than to trained interviewers from an outside independent agency.[22]

For these reasons, a Romanian private survey firm, Institute of Marketing and Polls (IMAS), was contracted to conduct field surveys during January-February 2002. The study goal was to achieve over 4,000 respondents. Of the 4,839 individuals contacted for interviewing, about 70 percent responded, leaving us with a sample of 3,357 persons. As is common in these type of studies, response rate was slightly higher for participants (72 percent) than for non-participants (68 percent).

IV.2. Sample Selection

The data used in this study, a random sample of almost 3,357 persons who registered at the Employment Bureau during 1999, was collected during January and February 2002. About two fifth of this sample (1,408 individuals) were ALMP participants of either employment services (ES) or small-business assistance (SBA) whose ALMP contract began in 1999.[23]

To obtain a representative sample of ALMP participants, we randomly selected, for each of the programmes, 10% of clients served in the fifteen counties with the largest number of clients served in 1999. These fifteen counties represented 86% of all clients served in 1999. Furthermore, an analysis of the economies of these fifteen judets indicates that they represented a broad spectrum of the Romanian economy with many sectors represented, including heavy industry, mining, agriculture and other sectors. Moreover, these fifteen judets included some of the poorest judets in Romania (Botosoni and Vaslui—north-east region) as well as some judets with substantial natural resources and highly developed industries (Cluj and Maramures—north-west region).

The rest of the sample—the potential comparison group—were 1,949 persons who were registered at the Employment Bureau around the same time and in the same county than participants but who had *not* participated in an ALMP.[24] To select non-participants, we first determined, for each of the two ALMPs, the number of participants that were selected for the participant sample in each of the counties. Next, in each county and for each programme, we randomly selected a similar number of non-participants from the same Employment Bureau register list. Following recommendations from Heckman, Ichimura, and Todd, 1997, in addition to draw the comparison group from the same local labour market with respect to participants, the same questionnaire was used for both participants and non-participants.

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[22] Previous studies have found evidence of this. For instance, O'Leary et al., 1998, found evidence that participants of self-employment assistance programmes in Hungary could have been under-reporting earnings to interviewers who worked in local labor offices.

[23] Based on discussions with programme implementation staff, we determined that contracts that begun in 1999 most accurately reflect the operations of the programmemes. Prior to 1999, the programmemes were new and some of the procedures were not fully implemented. Contracts that begun after 1999 may not be suitable for the evaluation since some may still be in operation or recently finished at the time of the survey and impacts from these contracts may not yet be fully reflected in participants' outcomes. Thus, our sample was drawn from contracts that started during 1999.

[24] Non-participants did not receive employment services, nor did they participate in small-business assistance. In addition, non-participants did not participate in the two other ALMPs—training and public services, which were offered at the same time but in a much smaller scale (see footnote 11).

The timing of events (illustrated by Figure 2) goes as follows. Some of the workers who registered at the Employment Bureau during 1999 received services from one of the two programmes described in section II.4. The rest of the workers did not receive any of these services. Although it is possible that some of the programme participants may have continued to receive services during the year 2000 (since the maximum duration of the programmes varied between 9 and 12 months), this is quite unlikely since, in practice, the length of these programmes was considerably shorter. During January and February of 2002, we interviewed the selected sample of participants and non-participants. All interviewed persons were asked three types of questions: (1) questions on employment and earnings at the time of the survey, (2) retrospective questions on employment and earnings during the years 2000 and 2001, and (3) retrospective questions on employment and earnings during 1998, prior to participating in the ALMPs. Details regarding the outcome variables are given in Section VI.1.

Figure 2

TIMING OF EVENTS

During 1999	**2000**	**2001**	**January/February 2002**
Some displaced workers registered at Employment Bureau participate in one of the two ALMPs. The rest do not participate in any ALMP.	Workers work or look for work. During 2000, some of the ALMP participants may continue to receive services.		Workers are interviewed regarding current and passed employment outcomes, including outcomes during 1998.

Restriction that all data be available led to a sample of 2,610 individuals (1,109 participants and 1,501 non-participants). 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 I restricted our sample to have all data available.

IV.3. Descriptive Comparison of Baseline Characteristics of Programme Groups and Non-Participants

Tables 3 and 4 display descriptive statistics for socio-economic variables for participants of employment services and small-business assistance, and for non-participants. Despite the small differences observed between ES and SBA participants, SBA participants seem to be slightly more advantaged workers than those participating in ES. In addition, when comparing participants to non-participants, the main finding is that non-participants tend to have more stable employment histories despite living, on average, in less dynamic areas. The results are summarized below.

Participants in ES and SBA have similar and relatively stable employment histories during 1998. This is reflected by the fact that about three fourths of them reported working during 1998, of which more than four fifths did so for at least 7 months. There are four major differences between these two groups: (1) the level of education attainment, (2) the type of region where they live, (3) the length of their employment spell during 1998, and (4) their average monthly earnings in 1998. While participants in SBA tend to be more educated with two fifths of them holding a high-school degree and one fifth of them holding a university

degree, participants in ES are more likely to live in large urban areas. Moreover, SBA participants were employed for a longer share of the year 1998 than ES participants, since two thirds of the former worked for at least 9 months during that year, compared to only half of the latter. Finally, SBA participants worked in better-paid jobs during 1998 than ES participants (on average, SBA participants earned 16 percent higher monthly earnings than ES participants).

Even though non-participants are similar to ES and SBA participants, there are some baseline differences worth highlighting. First, non-participants have a higher share of men in their group. Second, non-participants are more likely to live in rural areas than participants of any of the other two programmes. Third, non-participants experienced relatively more stable employment during 1998, despite living in areas where, on average, the unemployment rate was higher. For example, over four fifths of them were employed during 1998, of which 95 percent did so during for at least 7 months. Fourth, non-participants worked in better paid jobs during 1998. And finally, they were much less likely to have received training during 1998.

Comparing the outcomes of participants in the programmes, the major difference is that participants of ES are more likely to have shorter cumulated unemployment spell than SBA participants—shown in Table 5 (see section VI for a detailed description of the outcomes of interest). Comparing the outcomes between participants and non-participants, the employment experiences of the former are more successful than those of the latter. These figures cannot be interpreted as the causal effects of the programmes.

V. Identification and Estimation

V.1. The Evaluation Problem

I analyse the effects of the K = 2 different ALMPs (ES and SBA) on employment outcomes at the individual level. In a situation where individuals have multiple treatment options, I estimate the average treatment effect on the treated (ATET) of one ALMP against non-participation in any ALMP and of pair wise comparisons of the two programmes. I also analyse the heterogeneity of the estimated ATET by various socio-economic characteristics of the treated individuals as explained in Section VII.

The empirical approach follows the framework suggested by Roy (1951), and Rubin (1974), and extended by Imbens (2000) and Lechner (2001) to multiple, mutually exclusive states. Let the potential outcome Y^k, k = 1, 2, denote the outcome when a person gets the *treatment* (in this case, participates in one of the two ALMPs described above), and Y^0 denote the outcome when a person does not participate in any ALMP. For any individual, only one component of $\{Y^0, Y^1, Y^2\}$ is observable.

Participation in a particular treatment k is indicated by the realization of the random variable S, $S \in \{1,2\}$. This notation allows under the usual assumptions (see Rubin, 1974) to define average treatment effect on the treated for pair-wise comparisons of the effect of different states:

$$\gamma^{k,l} = E(Y^k - Y^l) = EY^k - EY^l \qquad (1)$$

$$\theta^{k,l} = E(Y^k - Y^l | S = k) = E(Y^k | S = k) - E(Y^l | S = k) \quad (2)$$
$$k \neq l;\ k,\ l \in \{0,1,2\}$$

The shorthand notation $E(.|S = k)$ denotes the mean in the population of all individuals who participate in an ALMP, denoted by $S = k$.

$\gamma^{k,l}$ denotes the expected (average) effect of the treatment k relative to the treatment l for a participant drawn randomly from the population (average treatment effect, ATE). ATEs are symmetric. $\theta^{k,l}$ shows the expected effect of the programme for those persons who actually participated (average treatment on the treated, ATET). However, we cannot observe the counterfactual, $E(Y^l|S = k)$, i.e., the average outcome of those persons who participated in the programme had they not participated. Thus, without further assumptions, ATETs are not identified. Lechner, 2001, shows that if we can observe all factors that jointly influence outcomes and participation decision, then—conditional on those factors (call them X), the participation decision and the outcomes are independent. This property is exploited by the conditional independence assumption (CIA). Note that the ATETs are not symmetric, if participants in treatment k and l differ in a way that is related to the distribution of X and if the treatment effects vary with X.

V.2. Is it Plausible to Assume Conditional Independence?

Our approach for meeting the CIA was to include in the matching process: (1) characteristics influencing the decision to participate in ALMP, (2) baseline values of the outcomes of interest, (3) variables influencing the outcomes of interest, and (4) variables reflecting local labour market conditions, and regional differences in programme implementation or local offices' placement policies.

The characteristics, implementation, and utilization of the different ALMPs as well as the characteristics of their participants indicates that the level of education, experience, previous earnings, and pre-programme unemployment history are important factors in determining whether an individual will participate in any programme, as well as in which of the programmes. These factors are also likely to influence the future labour market outcomes, and thus, in order for CIA to be plausible, they should be included in the estimation of the propensities.

Demographic characteristics, such as age and gender are also important determinants of labour market prospects. Moreover, family composition and whether the person is the family's main wage earner are also likely to influence individual's decision to participate in a programme or not.

I also include variables that capture the local labour market conditions. These variables measure the different employment opportunities in the *judets*. In addition, since differences in labour market conditions may favour a different mix of programme and unemployment policies, these variables are also a proxy for different policy approaches across counties.

Finally, I include county dummies to capture unobserved local aspects that are likely to be correlated with programme implementation and utilization, or local offices' placement policies, and thus relevant for programme-joining decisions and individuals' potential labour market performance.

Although at first sight and relative to studies conducted in developed countries, these data

may not seem sufficiently rich to observe all relevant factors, I believe that these data are unusually rich for studies conducted in transition economies—see Kluve, Lehmann and Schmidt , 1999, or Earle and Pauna, 1996, among others, for discussion on the poor quality of ALMPs' data in transition economies. For instance, one could argue that, even though I control for employment history during 1998, I lack of information on employment history *prior* to 1998 (Heckman and Smith, 1999, point to the importance of controlling for employment dynamics prior to programme participation.) Rich data on employment dynamics prior to programme participation in studies on transition economies is unusual. Many of these studies do not have *any* employment information prior to participation (Lubyova and Van Ours, 1999, Puhani and Steiner, 1997, and Vodopic, 1999, among others). Others have limited information on employment history prior to participation. For instance, while O'Leary 1998, have information on prior employment status and whether the individual was a long-term unemployed, they do not control for months employed or unemployed prior to programme participation. A study that has information on the unemployment spell that took place right before programme participation is Terrel and Sorm's JCE 1999 paper. However, their information is limited to the year of participants' unemployment registration, which is, at most, the year prior to programme entrance. Finally, to our knowledge, Kluve, Lehman and Schmidt, 1999 and 2002, have the most thorough information on employment dynamics prior to participation in transition economies, and again, this information is limited to 12-month prior to entering the programme.

Moreover, I have information on 1998 earnings, which can be considered a proxy for both workers' pre-displacement job characteristics and workers' motivation, ability and soft skills. Again, while baseline earnings data may be frequently available in developed countries, they are less common in transition economies' studies. The only study that I identified that controlled for baseline earnings data was Terrel and Sorm's, 1999.[25]

Finally, although I lack information on the willingness of the Employment Bureau staff of the different local offices to assign people into different programmes, I control for several county characteristics that most likely capture most of these local differences. Thus, I believe that our unusually informative data allows us to capture the major effects of unobservable variables that are both correlated with potential outcomes and the decision to participation.[26]

Summarizing, the available data include much, but not all, information on factors, which affect the selection and the outcomes. The crucial question—that is left to the reader to decide—is whether there is sufficient information to justify the conditional independence assumption. However, I believe that our data frequently provides variables that contain some of this needed key information, and is at least qualitatively equal (if not superior) to data used in other evaluations of ALMPs in transition economies.

V.3. Empirical Implementation

I used propensity scores to select a group of participants for *each* treatment group, according to the following three steps. First, I fitted a probit model for the choice between the two programmes and non-participation. Table A.1. in the appendix displays the estimation

[25] O'Leary et al, 1998, had information on net monthly household earnings, although it is unclear whether this information was used to control for selection bias.

[26] To my knowledge, I did not find any study on ALMPs in transition economies that had information on local labor offices' willingness to assign people into different programmes.

results and provides a more exact description of the variables used in the analysis.

Second, I used the output from this selection model to estimate choice probabilities conditional on X (the so-called propensity scores) for each treatment comparison pair. I then imposed the common-support requirement to guarantee that there is an overlap between the propensity scores for each pair (see column 9 of Table 6 for number of treated observations lost due to this requirement).

Third, for each treatment group member, I selected potential comparison group members based on their propensity scores and their *judet*. The selection process was done with replacement, so that a potential comparison group member could have been matched to more than one treatment group member.[27] 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 0.01").[28] When there were multiple matches, each non-participant received a weight that reflects the number of successful matches within the caliper range.[29] 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 resamples.[30]

V.4. Matching Quality

The results in Table 6 show indicators on the quality of the match for each of the two ALMPs and for each programme compared to non-participation. Overall, matching on the estimated propensity score balances the X's in the matched samples extremely well (and better than the other versions of matching I experienced with).

First, to test if the matching procedure is able to balance all the covariates, I estimated the median absolute standardized bias before and after the matching (Rosembaum and Rubin, 1985). This indicator assesses the distance in marginal distributions of the X-variables, and is commonly used to evaluate the validity of the match (Sianesi, 2004, Caliendo, Hujer and Thomsen, 2005, among others). Columns 7 and 8 of Table 6 show the median standardized difference over all covariates before and after the matching took place. The matching procedure balances the distribution of covariates very well since the median absolute standardized bias drops from a range between 9.36% and 18.56% before the matching to a range between 2.29% to 4.19% after the match.

Second, comparison of the pseudo-R^2's before and after the matching indicates that after the matching there are no systematic differences in the distribution of the covariates between the two groups (columns 4 and 5 of Table 6). Similarly, the P-value of the likelihood test

[27] Matching with replacement minimizes the propensity-score distance between the matched comparison units and the treatment unit: each treatment unit can be matched to the nearest comparison unit, even if a comparison unit is matched more than once. This 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 (Rosembaum, 1995).

[28] I tried alternative matching methods and caliper sizes of 0.05, 0.02, and 0.01. To ensure a good quality match, I implemented a caliper of 0.01.

[29] By using more comparison units, one increases the precision of the estimates, but at the cost of increased bias.

[30] Heckman, Ichimura, and Todd, 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 neighbour matching, since it allows to circumvent the issues regarding nearest neighbour matching raised by Abadie and Imbens, 2006.

after the matching rejects joint significance of the regressors, while the opposite was true before the matching (column 6 of Table 6).

VI. Programme Impacts

VI.1. Measurement of Labour 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.[31] Moreover, since our survey included retrospective questions, I 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.

In addition to measuring employment experience with employment and average usual monthly earnings at the time of the survey, I compute two variables that measure the reemployment probability for a period of at least 6 and 12 months, respectively, during the years 2000 and 2001. These two variables provide additional information on workers' reemployment experiences over the two-year period prior to the survey, and inform us on the workers' employment attachment over that period. I also include average usual monthly earnings during the two-year period prior to the survey as a proxy for worker's productivity.

Finally, I include accumulated months of unemployment within the two-year period 2000-2001 to get a measure on how many months of unemployment programme participation could save. I also computed accumulated months receiving unemployment benefits (UB) during the two-year period 2000-2001. Table 5 summarizes these outcomes by participation status and before the matching procedure. Table A.2 in the Appendix describes the outcomes of interest.

VI.2. Average Results

VI.2.1. Treatment versus Non-Participation

Impacts were estimated as the difference in average outcomes between the treatment and the comparison groups. The estimated ATET and their bootstrapped 95 percent confidence intervals are shown in Table 7. The last two columns of Table 7 show the impacts of participation in ES and SBA, respectively, compared to non-participation. Overall, participation in either programme is successful into getting the unemployed back to work compared to non-participation. Moreover, participation in ES improves the average realized earnings of its participants compared to non-participants. Below, I summarize the results.

First, I find that ES was successful in improving participants' economic outcomes compared to non-participants in all dimensions. ES had a positive impact both on employment at the time of the survey and on employment during the two-year period 2000-

[31] 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. Recently, Lechner and Melly, 2007, have proposed consistent nonparametric estimators of individuals' earnings capacity as an alternative measure to realized earnings.

2001. For instance, it increased the probability of being employed at the time of the survey by 8.45 percentage points, which represents a 19.73% increase in the likelihood of being employed at the time of the survey—this employment effect is explained by higher likelihood of being employed in a wage and salary job.[32] Similarly, ES improved by 6.22 percentage points (or 8.56%) and 7.65 percentage points (or 13.72%) the likelihood of being employed for at least 6 and 12 months during the two-year period 2000-2001, respectively. The programme also reduced the accumulated number of months participants were, on average, unemployed compared to non-participants by almost two months (or 16.74%), and the number of months receiving UB payments by almost one month (or 48.37%). Finally, ES had a positive impact on earnings: it increased average current monthly earnings by 57 thousand lei (or 22.49%) and average monthly earnings during 2000-2001 by 87 thousand lei (or 28.44%) compared to the earnings of non-participants.

I also find that SBA improved its participants' employment prospects. More specifically, SBA increased by 8.38 percentage points (or 11.89%) the likelihood of being employed for 6 months during the two-year period 2000-2001. This programme also reduced the accumulated number of months participants were unemployed and receiving UB payments by 14.94% and 34.25%, respectively. However, I did not find that SBA increased the average monthly earnings of its participants relative to non-participants. This lack of result could be explained by the fact that entrepreneurs are more likely to under-report their earnings than wage and salary workers.

Finding that participation in ES or SBA increases the employment prospects of its participants compared to attending no-programme, does not address the question of which of these two programmes is the most effective for getting unemployed workers back to work. Below, I proceed to answer this question.

VI.2.2. Pair wise Evaluation of Employment Services and Small-Business Assistance Services

The first two columns of Table 7 show the pair-wise average outcome differences between ES and SBA. The estimated effects show that in terms of the accumulated employment effects, ES was superior to SBA.

The first column of Table 7 shows that ES was more effective for individuals receiving this type of service than if they had participated in a SBA programme instead. For instance, I find that participating in ES increased by 17.28 percentage points (or 34.18%) the likelihood of being employed for at least 12 months in the two-year period 2000-2001, and reduced by over 3 months (or 27.48%) the spell of unemployment during the same period compared to participating in SBA.

Moreover, SBA participants would have been better off had they participated in ES instead. For example, when the treatment was the SBA programme (column 2 of Table 7), its participants had 9.86 percentage points (or 11.53%) and 17.02 percentage points (or 24.65%) lower probability of being employed for at least 6 and 12 months, respectively, within the years 2000 and 2001. Moreover, although not statistically significant, the impacts on

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

outcomes measured at the time of the survey suggest that SBA participants were less likely to be employed at wage and salary jobs than if they had participated in ES instead.

The main difference that appears when comparing the results between different populations defined by treatment status (that is, comparing the first two columns of Table 7) concerns the impact on average realized earnings over the period 2000-2001. I find the earnings estimate to be 64 thousand lei (or 14.16%) lower for SBA participants than for ES participants, when the treatment was SBA (column 2). The impact was also negative, although not statistically significant, when the treatment status was participation in ES instead of SBA (column 1). This difference could be explained by the fact that I am using a measure of realized earnings as opposed to individuals' true earnings capacity, and thus, the earnings measures are a crude measure of productivity, as explained by Lechner and Melly, 2007.

VI. 3. Heterogeneity among Individuals

So far, I have considered the average effects for the participants in the different programmes. In summary, I find that ES was superior to SBA, and that both ES and SBA were superior to non-participation (this latter result was already found by BR). However, this average analysis does not provide any guidance on why ES might work better than SBA, nor does it explore whether the impacts vary with the socio-economic characteristics of its participants. In this section, I discuss competing theories explaining why the different programmes may have different effects, and then explore the compatibility of the estimated effect heterogeneity with the discussed theories.[33]

VI.3.1. Theoretical Considerations

There are at least three main reasons that could explain why the programmes may have different effects: (1) improved job matching; (2) human capital; and (3) signalling.

Improved Job Matching

Both programmes may improve job matching for different reasons. On the one hand, the main objective of ES is to improve job search efficiency by increasing the information available to potential employers on the amount and quality of the applicants, and by improving unemployed workers' knowledge about potential new employers and new occupations. On the other hand, SBA offers a network and contacts to unemployed workers that could (and sometimes does) result in wage and salary job offers. For instance, more than 45% of participants of a self-employment assistance programme in New Jersey ended up working in wage and salary jobs. Similar results were found in Maine and New York, where close to 60% of participants of self-employment programmes ended up working in wage and salary jobs. The study also reports that most of these wage and salary jobs were full-time jobs and, often, in the same industries as those in which participants initially became self-employed (Kosanovich, et al., 2001).

It is unclear which of these two programmes would work best at improving job matching. A priori, one would think that ES should be more efficient at improving job search then SBA since the job matching mechanism for the latter programme is the result of an indirect, and thus, secondary effect of the programme. However, theoretical findings on search channels find that the efficiency of employment counselling services alone (that is, without monitoring)

[33] Our approach is similar to that of Gerfin, Lechner, and Steiger, 2005.

is seriously questionable. For instance, Van der Berg and Van der Klaauvw, 2006, developed a job search model with two search channels—the formal and informal one—and endogenous job search, and found that low-intensity job search programmes were useless. Moreover, empirical evidence on employment services in countries with large informal sectors, such as transition economies and developing countries, have shown that public employment services may have limited reach as workers may prefer other channels of job search (Woltermann, 2002).

Human Capital

The impact of ES on human capital is likely to be small since the programme does not incorporate explicit training. In contrast, SBA offers some training through the form of advising on legal, accounting, financial, marketing and sales services issues, and some short-term entrepreneurial courses. Moreover, there is some empirical evidence that business-training programmes improved participants' business knowledge and productivity, measured by revenues, repayment, and client retention rates (Karlan and Valdivia, 2006).

Signalling

Finally, participating in SBA may have a signalling value to prospective clients and contractors. Given the little entrepreneurial tradition in Romania, it is likely that prospective clients and contractors conclude that individuals who have participated in SBA are better entrepreneurs, and are more reliable since they have institutional support than those who did not participate in SBA. Moreover, in order to be a credible signal, participating in SBA must be more costly for less productive workers than ES. Given that SBA involves entrepreneurial courses, and writing a business plan, it is likely that participating in SBA is more costly for less capable workers than ES.

Discussion of the Resulting Effects

As Gerfin et al., 2005, discuss in their paper, it is not possible to derive strict tests for the relative importance of these explanations. However, systematic heterogeneity of the effects between different groups of unemployed will provide evidence consistent with one theory but not with another. I follow their strategy and use non-participation in any programme as a benchmark because non-participation will neither have job matching, human capital, or signalling effects. In addition, subgroup estimates comparing the impacts of ES versus SBA participation are also provided and discussed.

Let's first consider the effect of heterogeneity with respect to age. I expect ES to work better for young workers since they are likely to have less access to informal search channels. The empirical literature on the use of different search channels by different types of workers indicates that workers with characteristics such that their chances to find a job are low because of little access to informal search channels rely to a relatively large extent on formal search (Van der Berg and Van der Klaauw, 2006, Abbring, Van der Berg and Van Ours, 2005, Keeley and Robins, 1985). Because ES facilitate job finding through the formal channel, I would expect it to have strong effects for young workers. In contrast, for the older workers, I would expect a weaker job-matching effect of ES compared to non-participation since these workers are likely to have more access to the large informal sector existing in Romania. On the other hand, it is difficult to determine whether the human capital generated by SBA will be more beneficial for younger or older workers given that the skill formation of the former might have strongly differed to that of latter because of the social, political and economic reforms. Finally, according to the signalling theory, SBA should be more efficient for those

workers for whom the costs of participating in SBA would be lower. Because older workers are likely to have more networks and contacts than younger ones, this should lower their costs of starting a business compared to those of younger workers. Thus, if signalling is important, I expect SBA to have more of an impact compared to non-participation for older than younger workers.

A similar argument to the one done with respect to age and the use of search channels can be done in the case of workers in a labour market with unfavourable circumstances. In this case, the empirical literature also indicates that these workers are more likely to rely on formal search channels because access to their informal search channels has dried up. Given that rural areas in Romania in the late 1990s tended to be more economically depressed than urban areas, I suspect that ES will have a stronger job-matching effect compared to non-participation on workers living in rural than urban areas. On the other hand, if I assume that SBA generates human capital by providing knowledge on the possibility and scope of new micro-entrepreneurial activities to unemployed workers who have little outside opportunities, then individuals living in depressed rural areas are likely to benefit the more from SBA than individuals living in urban areas. This occurs because of the little outside options existent in rural versus urban areas and is consistent with the empirical evidence from developed countries that finds that facilitating micro-credits in depressed rural areas is an efficient way to generate self-employment and economic activity (Karlan and Valdivia, 2006). Finally, because rural areas were more depressed than the urban ones, having access to contacts and networks was likely to be more difficult in the former, thus increasing the costs of starting a new business. Therefore if signalling is important, I ought to find more of an impact of SBA compared to non-participation in urban than in rural areas.

Finally, I will consider the effect of heterogeneity with respect to the skill level of the unemployed. It is unclear whether skilled workers in Romania would have more or less access to informal channels than unskilled workers, thus the job matching mechanism is uninformative on the relative impact of either programme with respect to skill level. On the other hand, the human capital theory does predict a stronger effect of the SBA programme compared to non-participation for the less skilled workers if we are willing to assume that: (1) the human capital generated by SBA has a certain component of general human capital, which will be partly redundant for high skilled workers, and that (2) it is not necessarily increasing with productivity. In contrast, the signalling theory would predict the opposite: SBA signalling effects would be stronger for more qualified workers since they have lower costs of acquiring entrepreneurial skills. For instance, according to Costariol, 1993, in the case of Romania, where two generations of artisan tradition were lost during communism, *the typical private entrepreneur is a first-generation person, middle-aged, mainly with previous experience in a managerial position with large scale state-owned companies or, if he is young, usually with a University education*. This description of the typical Romanian entrepreneur indicates that being more educated facilitates access to entrepreneurial activities.

VI.3.2. Empirical evidence

The methodology to estimate the impacts follows the steps described in Section V, however, I have previously stratified the sample along the dimensions age, type of region, education, unemployment duration, and gender, and subsequently, match within strata.

Heterogeneity with Respect to Age

I find that, with respect to non-participation, ES improved the economic outcomes of younger workers compared to older ones (estimates shown in Table 8). These results are consistent with the improved matching theory since the effects of ES with respect to non-participation were significantly larger for the subpopulation that is likely to have less access to informal search channels. For instance, I find that younger workers had 26.20 percentage points (or 61.02%) higher likelihood of being employed for at least 12 months within the two-year period 2000-2001 than non-participants. In the case of older ES participants, this likelihood was increased by only 4.12 percentage points (or 7.16%) and the estimate was not statistically significant. This finding is explained by considerably higher likelihood of employment for older non-participants (57.58%) compared to younger ones (42.94%), suggesting that the latter may find it more difficult to find work through alternative job search channels.

When comparing SBA participants to non-participants, I find that the impact estimates on current employment and earnings are larger for older than younger workers (although the differences between the two subgroups are not statistically significant). This result would be compatible with the signalling hypothesis.

Finally, pair-wise comparison of current employment outcomes between ES and SBA shows that, with respect to SBA, ES are more effective for younger workers than for older ones. I find that participating in ES increases by 26.52 percentage points (or 46.86%) the likelihood of being employed at the time of the survey for younger workers, and that it increases their current earnings by 129 thousand lei (or 43.37%). The estimates for older workers are negative although not statistically significant.

Heterogeneity with Respect to Type of Region

Above I have argued that ES should also work better for individuals living in rural areas compared to those living in urban ones, because the former live in more depressed areas where informal search channels have most likely dried up. The evidence found in Table 9 is consistent with this theory. For instance, I find that ES increases the average wage of its participants by 144.24 thousand lei, and reduces the unemployment spell by almost 5 months over the period 2000-2001.

In addition, I find that, compared to non-participation, SBA is more successful for workers living in rural areas than those living in urban ones. For instance, participating in SBA increased the likelihood of employment for at least 12 months of the two-year period 2000-2001 by 19.06 percentage points (or 49.16%) and reduced the accumulated spell of UB receipt by 3.61 months (or 86.57%) for workers living in rural areas. No statistically significant effects were found for SBA participants living in urban area. These results are compatible with the human capital hypothesis in the sense that, by offering human capital in entrepreneurship, SBA widens the scope of activities that seem possible in deprived areas. In addition, they are not compatible with the signalling theory.

Heterogeneity with Respect to Education

According to the human capital hypothesis presented above, SBA should have stronger effects compared to non-participation for the less skilled workers relative to more skilled ones. However, the opposite is true for the signalling hypothesis.

As a measure of skill I have used whether the worker has a high-school degree or not. When comparing SBA to non-participation, the results are consistent with the human capital hypothesis. The estimates in Table 10 show that SBA is beneficial with respect to non-participation for workers without a high-school diploma. For instance, I find that, with respect to non-participation, SBA increased the probability of being employed for at least 12 months within the two-year period 2000-2001 by 19.35 percentage points (or 48.51%) for the lower educated subgroup—compared to a non-statistically significant increase of 1.45 percentage points (or 2.44%) for the higher educated one. This large difference seems to be explained by the scarce employment chances among the group of less educated workers, as illustrated by a considerably lower average employment likelihood for the lower educated workers' comparison group (of 40%) as compared to the one for the higher educated group of non-participants (60%).

However, when comparing ES and SBA, I find that ES works best for workers with less than a high-school degree. This results suggest that for the less educated workers, participating in any programme is better than not participating, but that ES tend to be more efficient for them than SBA. In contrast, for workers with at least a high school degree, I find that SBA was superior to ES. This result is compatible with the signalling theory and indicates that for more educated workers SBA may be more efficient than ES.

Additional Heterogeneity Analysis

In addition to the above results, I have estimated the effects of ES and SBA with respect to non-participation for the following sub-populations: males, females, with prior unemployment spell below 6 months, and with prior unemployment spell longer than 5 months (shown in the Appendix Tables A.3. and A.4.).

I find that, with respect to non-participation, ES improves economic outcomes of participating workers with histories of short-term unemployment compared to those with histories of long-term unemployment. In the case of short-term unemployed workers, ES reduced the likelihood of being employed 6 months during the 2000-2001 period by 7.55 percentage points (or 10.25%). In contrast, ES were not beneficial for the long-term unemployed since it did not affect the employment likelihood (although the impact was a negative 5.02 percentage points, it was not statistically significant).

Finally, I also find that the impact estimates on accumulated employment are larger for females than for males (although the differences between the two subgroups are not statistically significant).

VI.4. 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. I compared the results obtained by matching to some alternative estimators. Tables 11, 12, and 13 present average impact estimates on various employment outcomes and earnings in Romania using four alternative estimators. The first set of results (first column of Tables 11 through 13) is gross impact estimates, which were *not* adjusted for observable differences between the participant and non-participants, that is, I use the whole sample of non-participants regardless of whether their baseline characteristics resembled to those of participants. The second set of results (second column of Tables 11 through 13) 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 to estimates the propensity scores in Tables 7 through 10, and column 4 of Tables 11 through 13. 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 IV, however, I 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 is the estimators presented in section VI.2. and Table 7.

Below, I summarize the main findings from analysing the sensitivity of the results of participating in either ES or SBA compared to non-participation (Tables 12 and 13, respectively). The most obvious overall result in Table 13 is that the unadjusted impact estimates (column 1) are generally different from the other estimates (columns 2 through 4). In general, the unadjusted impact estimates of SBA were better than the other ones, suggesting that operators "cream off" the most qualified candidates among the unemployed for this programme. This finding is consistent with other analyses of ALMP in transition economies (O'Leary, 1998 and Kluve, Lehmann, and Schmidt, 2001, among others). Moreover, I find that comparing the gross impact estimates with the regression-adjusted estimates (column 1 versus column 2 of Table 13) clearly reduces the positive impact of most of SBA estimates, reflecting that there is an over-representation of individuals with "better" observable characteristics in this group. Comparing columns 2 and 4 provides us with a comparison between results obtained by matching with the standard OLS regression for the continuous dependent variables, and a probit model for the discrete dependent variables. I observe that the results obtained by matching reduce the significance of the SBA estimates. These differences are presumably explained by the parametric restrictions underlying the OLS and probit estimations. Matching allows for heterogeneity in the treatment effect in a more flexible way. In the case of ES participants, I find that although some "cream off" also seems to take place for ES participants, the results are not as strong as those observed for the SBA participants (see Table 12). For instance, regression-adjusting the estimates has little effect on ES findings, leaving most estimates unchanged (columns 1 and 2 of Table 12). Finally, comparing estimates from column 3 and 4 enable us to explore the importance of controlling for pre-earnings, pre-employment, and pre-unemployment history. I find that these variables are important when measuring the effect of the different programmes, as reflected by the fact that excluding them changes the size of impact estimates of both programmes.

I now proceed to summarize the main findings from the sensitivity analysis of the relative impacts of participating in ES compared to SBA (Table 11). Again I find that the unadjusted impact estimates (column 1) are generally different from the other estimates. The unadjusted impact estimates indicate very few differences in the outcomes of ES and SBA participants. The only statistically significant difference is that ES reduced the length of UB receipt by a bit more than half a month. Since the other estimates (columns 2 through 4) reflect a superiority of ES, this result suggests that programme operators are more selective when choosing SBA participants than ES participants. This implies that there is an over-representation of individuals with "better" observable characteristics in the group of SBA participants, which is consistent with earlier findings from Tables 12 and 13. In addition, the differences between the OLS and probit estimators on the one hand (column 2), and matching

on the other (column 4) are not too large, and overall, both types of estimates are consistent with ES being more successful than SBA. Finally, comparing estimates from the last two columns indicates that controlling for employment and earnings baseline characteristics is relevant.

VII. Conclusion

Recent empirical evidence has found that employment services and small-business assistance programmes are useful active labour market programmes to help get the unemployed back to work. In this paper, I investigate which of these two programmes works best and for whom. To do so, I exploit the fact that these two programmes were the first large scale programmes ever implemented in Romania after the 1989 Revolution, that they were targeted at more or less the same population of unemployed, and that I have rich informative survey data specially designed and collected for the evaluation of these two programmes. By concentrating in one country, I have the advantage that the institutional environment is held constant. Finally, this paper's findings can be helpful to policy makers from transition economies, as well as those from other economies with large informal sectors, such as those in the developing world.

When estimating the average effects for participants in the programmes, I find ES to be more successful than SBA in getting the unemployed back to work. For instance, I find that participating in ES increased by 17.28 percentage points (or 34.18%) the likelihood of being employed for at least 12 months in the two-year period 2000-2001, and reduced by over 3 months (or 27.48%) the spell of unemployment during the same period compared to participating in SBA. Moreover, I also find that even for participants in SBA, ES would have been a better option since it would have increased their likelihood of being employed for at least 6 and 12 months in the period 2000-2001 by 9.86 percentage points (or 11.53%) and 17.02 percentage points (or 24.65%), respectively.

I have explored three alternative theoretical explanations for these findings, and have found empirical evidence from the heterogeneity analysis compatible with: (1) improved job matching theory for ES (based on the results for the younger workers and those living in rural areas); (2) positive human capital effects for SBA (based on the results for the low-educated workers and rural workers); and (3) positive signalling effects of SBA (based on the results for the high-educated workers.)

These results suggest the following policy implications. First, I find that offering ES to unemployed workers with good access to the informal job search channel is not a good idea. This finding is consistent with earlier findings (Van der Berg and Van der Klaauv, 2006, among others). However, the novelty of this paper is to provide some guidance on which populations would benefit from ES in economies with large informal sectors. In such countries, ES ought to be targeted to displaced workers with little access to the informal job search channel (such as young workers) or those for whom the informal channel has dried up (such as those living in depressed areas.) Another policy implication is that, in transition economies, SBA seems to be an efficient programme for workers living in rural areas. The reason for this is that by improving worker's capabilities, SBA widens the scope of opportunities for unemployed workers in these often depressed areas. Finally, while, compared with non-participation, both ES and SBA are beneficial for workers with and without a high-school degree, the findings in this paper suggest that SBA is more appropriate for the more educated workers, and ES more helpful for the less educated ones.

To address the question of whether these two programmes were cost-effective from society's perspective, I can compare the costs per client of each programme with the economic benefits, as reflected in predicted earnings.[34] I estimated the average cost per client served by dividing the total amount spent in each ALMP by the number of clients served (shown in Table 2). The cost per client served was 123.74 thousand lei for ES and 179.15 thousand lei for SBA. To estimate the benefits of the policy, I used the estimated impact of these ALMPs on the usual average monthly earnings of their participants. I preferred using the earnings estimates over the 2000-2001 period because they are more likely to represent individuals' earnings than those observed at one point in time. This amounts to an annual sum of 1,047.84 thousand lei for ES, and 4,783.20 thousand lei for SBA (although this estimate was not statistically significant).[35] In both cases, benefits cover by far the cost per client served, indicating that both programmes were cost-effective.[36] Given that the cost differences between the two programmes were small, and that the heterogeneity analysis suggested that SBA was superior for some subgroups and ES for others, the policy recommendation is to target each programme to those sub-populations most likely to benefit the most from participation.

A caveat in my cost-benefit analysis is that I did not include among potential benefits: (1) possible effects on labour market behaviour of the unemployed prior to participation, such as, intensifying job search before entering the programmes in order to avoid participation, or leaving the labour force and stop collecting UB; (2) reduced criminal activity due to improved employment prospects; (3) improvements in the quality of life for participants and their families, (4) savings in the deadweight losses due to reduced taxes required to pay participants' future unemployment benefits. Another caveat is that I did not considered in this analysis the following potentially important costs: (1) the deadweight loss of taxation to finance benefits, subsidies, and operation of programmes; (2) the cost of the leisure forgone while participants are in the programme or employed; and (3) possible displacement effects of non-subsidized workers. Given that I ignore the above mentioned benefits and costs, the cost-effectiveness results have to be taken with some caution. This is especially true for the SBA programme, since this type of programmes can presumably have significant deadweight and displacement costs (although, according to Betcherman, Olivas and Dar, 2004, very few evaluations tend to provide any empirical evidence of them). Unfortunately, I cannot address this question with the data at hand. Future research ought to be directed towards this end.

[34] When measuring cost-effectiveness from society's perspective, I measure whether aggregate benefits from implementing the policy are greater than the aggregate resources spent by the policy, abstracting from who enjoys its benefits and who bears its costs. Thus, under this perspective, increases in taxes paid due to the increased employment of participants or reductions in public assistance of participants are not counted as they are transfers from participants to the rest of society.

[35] Estimates were calculated with respect to non-participation.

[36] Given that benefits that accrue within the observation period are above the costs, I did not use a long-term perspective to estimate cost-effectiveness.

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Table 1
Characteristics of the Two Programmes

	Employment and Relocation Services	Small Business Assistance
Content	Job and social counseling, labor market information, job search assistance, job placement services, and relocation assistance	Initial assessment of the aptitude and skills of unemployed persons to start businesses, developing business plans, advising on accounting, financial, legal, marketing and sales services issues, assistance in the dialogue with local authorities, short-term entrepreneurial courses and training and other consulting services
Duration[1]	Up to nine months	Up to 12 months
Participants' stipend	Up to two months of salary at the minimum wage. In addition, those clients receiving relocation assistance could be reimbursed for expenses associated with moving to another community—up to $500 U.S. dollars equivalent in lei per family—based on submission of receipts.	There were provisions for short-term working capital loans of up to $25,000 U.S. dollars to programme participants
Targeted group	Unemployed workers	Unemployed workers who intended to start, or who had started businesses during the past 12 months
Negotiated placement rate	Of at least 10 percent	Of at least 5 percent of clients initially contacted

[1]In practice, the length of these programmes was considerably shorter than the established maximum duration.

Table 2
Completed ALMP contracts as of September 1, 2001

	Number of contracts	Clients served	Clients placed	Placement rate	Total cost (Lei)	Cost per client (Lei)	Cost per placement (Lei)
Employment and relocation services	88	31,679	6,610	20.87%	3,920,060,312.43	123,743.18	593,049.97
Small business assistance	92	20,293	3,568	17.58%	3,635,562,636.30	179,153.53	1,018,935.72

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

Table 3
Baseline Demographic and Regional Characteristics of ALMP Participants and Non-Participants, 1998
(Percentages except where noted)

Characteristics	Employment Services (1)	Small-Business Assistance (2)	Non-Participants (3)
	Personal characteristics		
Male	45.92	50.69	63.82
Age			
Less than 31 years old	7.50	4.99	8.93
Between 31 and 35 years old	14.59	22.71	16.46
Between 36 and 45 years old	40.16	40.44	36.58
Between 45 and 50 years old	20.62	17.73	19.79
More than 50 years old	17.14	14.13	18.25
Education completed			
Primary school	13.25	9.97	14.86
Secondary school	45.92	32.41	44.30
High school	28.65	37.67	29.31
University	12.82	19.45	11.26
	Family characteristics		
Family size	3.64 (0.05)	3.59 (0.62)	3.65 (0.03)
Main family earner	44.31	42.38	46.04
	Regional information		
Region			
Rural	11.24	5.82	17.92
Urban with less than 20 thousand inhabitants	18.34	35.46	18.45
Urban with 20 - 79 thousand inhabitants	20.08	14.13	28.11
Urban with 80 - 199 thousand inhabitants	39.89	27.15	25.98
Urban with 200 thousand inhabitants	10.44	17.45	9.53
Judet's unemployment rate	11.86	11.37	13.12
Sample size	747	362	1,501

Standard deviation in parenthesis for continuous variables.

Table 4
Baseline Employment Characteristics of
ALMP Participants and Non-Participants, 1998
(Percentages except where noted)

Characteristics	Employment Services (1)	Self-Employment Assistance (2)	Non-Participants (3)
	Work Experience		
Work experience (years)	23.99 (0.30)	22.99 (0.42)	23.63 (0.23)
	1998 Employment status		
Not employed in 1998	22.36	23.82	19.19
Employed in 1998	77.64	76.18	80.81
Employed between 1 and 3 months in 1998	4.42	1.39	2.53
Employed between 4 and 6 months in 1998	8.70	6.37	7.40
Employed between 7 and 9 months in 1998	10.71	3.05	5.53
Employed between 9 and 12 months in 1998	53.82	65.37	65.36
Not employed in 1998	22.36	23.82	19.19
	1998 usual monthly earnings		
Earnings per month			
Under 500 thousand lei	5.22	4.43	3.00
500 - 600 thousand lei	5.22	3.05	4.46
601 - 700 thousand lei	9.64	5.82	7.13
701 - 850 thousand lei	14.19	13.02	12.26
851 - 1,000 thousand lei	15.66	10.80	14.72
1,001 - 1,200 thousand lei	13.79	13.30	14.06
1,201 - 1,500 thousand lei	7.36	13.30	10.79
1,501 - 1,900 thousand lei	3.88	5.54	6.79
1,901 - 2,500 thousand lei	1.20	4.16	5.40
More than 2,500 thousand lei	1.47	2.77	2.20
Average monthly earnings (in thousand lei)	758.07 (22.51)	881.72 (39.38)	926.60 (17.88)
	Unemployment experience in 1998		
Average unemployment length during 1998 (months)	3.90 (0.17)	3.38 (0.25)	2.99 (0.11)
Unemployed at least 9 months during 1998	23.56	23.27	18.85
	Training experience in 1998		
Received training during 1998	6.69	8.86	3.13
Average training length during 1998 (months)	0.26 (0.05)	0.29 (0.06)	0.10 (0.02)
Sample size	747	362	1,501

Standard deviation in parenthesis for continuous variables. Monthly earnings have been deflated using 1998 deflator.

Table 5
Outcomes for ALMP Participants and Non-Participants
(Percentages except where noted)

	Employment Services	Self-Employment Assistance	Non-Matched Non-Participants
OUTCOMES			
Current experience			
Employed or self-employed	51.28	50.86	39.24
Employed	48.99	44.73	35.38
Self-employed	2.28	6.35	3.40
Average monthly earnings (in thousand lei)	309.64	303.28	232.62
During the two year period 2000-2001			
Employed for at least 6 months	78.87	78.86	68.22
Employed for at least 12 months	63.39	59.71	51.97
Average monthly earnings (in thousand lei)	394.34	398.60	322.42
Months unemployed	9.45	10.36	12.14
Months receiving UB payments	0.79	1.44	1.79
Sample size	747	362	1,501

Monthly earnings have been deflated using 1998 deflator.

Table 6
Indicators on the quality of the match, by ALMP

	Number of treated before (1)	Number of nontreated before (2)	Treated as a percentage of nontreated before (3)	Probit pseudo-R^2 before (4)	Probit pseudo-R^2 after (5)	Pr > X^2 After (6)	Median bias before (7)	Median bias after (8)	Number of treated lost to common support after (9)
ES versus SBA	438	247	177.33%	0.200	0.019	0.998	18.56	2.99	37
SBA versus ES	247	438	56.39%	0.200	0.035	0.743	18.56	4.19	5
ES vs. No participation[a]	747	1,028	72.67%	0.174	0.017	0.533	9.36	2.88	4
SBA vs. No participation[a]	362	964	37.55%	0.162	0.013	0.985	11.31	2.29	11

[a]The difference in the number of non-participants in the last two rows is explained by the fact that I restricted the sample to have treated and non-treated units come from the same local area (judet) and the two ALMPs under study were not implemented in all of the same judets.

(1) Number of treated, that is, joining an ALMP programme in 1999.

(2) Number of potential comparisons, that is, persons who had registered at the Employment Bureau in 1999 but did not participate in an ALMP.

(3) Treated as a percentage of potential comparisons.

(4) Pseudo-R^2 from probit estimation of the joining probability on X, giving an indication of how well the regressors X explain the participants probability.

(5), (6), (7), and (10) are postmatching indicators on kernel-based matching (1 % caliper).

(5) Pseudo-R^2 from probit estimation of the joining probability on X on the matched samples.

(6) 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.

(7), and (8) 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 nontreated subsamples as a percentage of the square root of the average of the sample variances in the full treated and nontreated groups. The standardized difference *after* matching is the difference of the sample means in the matched treated, that is, the common support, and matched nontreated subsamples as a percentage of the square root of the average of the sample variances in the full nontreated groups:

$$B_{before}(X) \equiv 100.\frac{\overline{X}_1 - \overline{X_0}}{\sqrt{\left[V_1(X) + V_0(X)\right]/2}} \text{ and } B_{after}(X) \equiv 100.\frac{\overline{X}_{1M} - \overline{X_{0M}}}{\sqrt{\left[V_1(X) + V_0(X)\right]/2}}$$

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

(9) Number of treated individuals falling outside of the common support (based on a caliper of 1 %).

Table 7
Average Treatment Effects of Programmes on the Employment Experience of their Participants
(Percentage points except where noted)

	Employment services vs. Small-business assistance (1)	Small-business assistance vs. Employment services (2)	Employment services vs. No participation (3)	Small-business assistance vs. No participation (4)
OUTCOMES				
Current experience				
Employed or self-employed	-1.02 (-10.77; 11.52)	-5.05 (-9.92; 2.95)	**8.45** **(3.19; 13.90)**	6.14 (-0.44 12.29)
Employed	2.30 (-8.11; 13.46)	-8.34 (-18.07; 0.38)	**9.72** **(4.17 ; 15.12)**	2.8 (-3.93 ; 9.55)
Self-employed	-2.74 (-5.38; 0.08)	2.93 (-0.88; 0.67)	-1.17 (-3.75 ; 0.65)	2.37 (-1.01 ; 5.30)
Average monthly earnings (in thousand lei)	-37.56 (-133.27; 40.26)	-25.32 (-98.78; 36.73)	**56.86** **(1 0.49; 109.51)**	37.58 (-13.25; 80.12)
During the two year period 2000-2001				
Employed for at least 6 months	10.70 (-0.86; 20.86)	**-9.86** **(-19.79; -3.07)**	**6.22** **(2.35 ; 13.52)**	**8.38** **(2.29; 14.13)**
Employed for at least 12 months	**17.28** **(0.38; 26.70)**	**-17.02** **(-26.02; -10.18)**	**7.65** **(2.11 ; 13.73)**	7.97 (-0.20; 14.40)
Average monthly earnings (in thousand lei)	-69.99 (-148.74; 15.99)	**-63.94** **(-140.56; -9.45)**	**87.32** **(56.99; 130.21)**	43.08 (-9.48; 87.58)
Months unemployed	**-3.10** **(-4.70; -0.32)**	**3.41** **(1.66; 6.10)**	**-1.90** **(-3.15 ; -0.9 2)**	**-1.82** **(-3.00 -0.54)**
Months receiving UB payments	-0.45 (-1.17; 0.87)	0.74 (-0.22; 1.47)	**-0.74** **(-1.18 ; -0.29)**	**-0.75** **(-1.50; -0.05)**
Sample size	643	643	1,748	1,311
Size of treatment group	401	242	743	350
Size of comparison group	242	401	1,005	961

Monthly earnings have been deflated using 1998 deflator. Bold numbers indicate significance at the 5% level (two-sided test).

Table 8
Average Treatment Effects according to Age
(Percentage points except where noted)

	Employment services vs. Small-business assistance (1)		Employment services vs. No participation (2)		Small-business assistance vs. No participation (3)	
OUTCOMES	<36 years	>35 years	<36 years	>35 years	<36 years	>35 years
Current experience						
Employed or self-employed	**26.25**✓	-1.98✓	**16.89**	**6.73**	-2.83	**9.01**
Employed	**27.30**✓	1.48✓	19.28	6.96	-1.14	5.04
Self-employed	-1.05	-3.18	-2.39	-0.19	0.24	2.87
Average wage (in tousand lei)	**129.18**✓	-71.31✓	65.73	**60.67**	-51.40	**58.01**
During the two year period 2000-2001						
Employed for at least 6 months	9.52	**10.11**	**17.78**✓	3.96✓	9.35	8.31
Employed for at least 12 months	15.63	11.54	**26.20**✓	4.12✓	12.89	**10.76**
Average wage (in thousand lei)	-43.76	-82.91	**116.62**	**82.81**	5.11	43.27
Months unemployment	-2.25	-2.27	**-4.62**✓	-1.21✓	-2.50	**-2.22**
Months receiving UB payments	-0.64	-0.46	-0.66	**-0.76**	-0.71	-0.75
Sample size	124	473	362	1,365	273	955
Size of treatment group	71	304	159	577	97	254
Size of comparison group	53	169	203	788	176	701

Monthly earnings have been deflated using 1998 deflator. Bold numbers indicate significance at the 5% level (two-sided test).
✓ indicates that the difference of the two estimated effects is significant at the 5% level.

Table 9
Average Treatment Effects according to Type of Region
(Percentage points except where noted)

	Employment services vs. Small-business assistance (1)		Employment services vs. No participation (2)		Small-business assistance vs. No participation (3)	
OUTCOMES	Rural areas	Urban areas	Rural areas	Urban areas	Rural areas	Urban areas
Current experience						
Employed or self-employed	7.82	-1.49	**17.93**	**6.13**	9.90	4.00
Employed	9.77	0.37	17.60	8.19	6.82	0.27
Self-employed	-1.96	-1.55	0.33	-1.65	3.30	2.31
Average wage (in tousand lei)	33.64	-64.86	**91.54**	47.19	36.90	42.54
During the two year period 2000-2001						
Employed for at least 6 months	6.16	16.65	7.73	**3.68**	**19.89**✓	0.06✓
Employed for at least 12 months	13.85	**18.55**	**17.25**	5.09	**19.06**✓	5.38✓
Average wage (in thousand lei)	**81.89**	-110.59	**144.24**✓	50.42✓	10.28	34.48
Months unemployment	-2.52	-3.28	**-4.87**✓	-0.96✓	**-3.64**✓	-1.20✓
Months receiving UB payments	0.60	-0.96	**-1.57**	**-0.50**	**-3.61**✓	0.36✓
Sample size	229	384	454	1,177	427	774
Size of treatment group	135	268	189	531	142	210
Size of comparison group	94	116	265	646	285	564

Monthly earnings have been deflated using 1998 deflator. Bold numbers indicate significance at the 5% level (two-sided test).
✓ indicates that the difference of the two estimated effects is significant at the 5% level.

Table 10
Average Treatment Effects according to Education Achievement
(Percentage points except where noted)

	Employment services vs. Small-business assistance (1)		Employment services vs. No participation (2)		Small-business assistance vs. No participation (3)	
OUTCOMES	No HS diploma	HS diploma or more	No HS diploma	HS diploma or more	No HS diploma	HS diploma or more
Current experience						
Employed or self-employed	7.50✓	**-11.61**✓	5.86	**11.28**	5.48	5.15
Employed	**10.43**	-2.25	8.52	11.09	3.47	0.70
Self-employed	-1.51	**-9.36**	-1.92	-0.04	1.00	3.44
Average wage (in tousand lei)	1.69✓	**-168.77**✓	73.48	**55.11**	20.34	41.30
During the two year period 2000-2001						
Employed for at least 6 months	13.17	9.70	3.87	6.47	**13.45**	4.89
Employed for at least 12 months	**17.34**	**15.26**	5.39	**9.13**	**19.35**✓	1.45✓
Average wage (in thousand lei)	-55.14	-65.56	60.08	**97.01**	47.95	14.68
Months unemployment	-3.26	**-2.26**	-1.40	**-1.96**	**-3.61**✓	-0.57✓
Months receiving UB payments	-13.81	**-1.27**	**-0.83**	-0.76	**-1.93**	0.61
Sample size	293	294	977	725	595	687
Size of treatment group	204	158	438	296	200	150
Size of comparison group	89	136	539	429	395	537

Monthly earnings have been deflated using 1998 deflator. Bold numbers indicate significance at the 5% level (two-sided test).
✓ indicates that the difference of the two estimated effects is significant at the 5% level.

Table 11
Sensitivity Analysis
Impacts of Employment Services Compared to Small Business Assistance
(Percentage points except where noted)

	ES vs. SBA		ES VS. MATCHED SBA	
	Difference of Means	Regression Adjusted (using all observable variables)	Difference of Means (using all observable variables, with the exception of pre-employment history, to match participants to non-participants)	Difference of Means (using all observable variables to match participants to non-participants)
OUTCOMES				
Current experience				
Employed	-0.25	4.12	1.28	-1.02
Employed	4.80	**7.67**	5.43	2.30
Self-employed	**-4.08**	**-3.06**	-3.70	**-2.74**
Average monthly earnings (in thousand lei)	-2.60	12.72	-25.38	-37.56
During the two year period 2000-2001				
Employed for at least 6 months	-0.43	**8.63**	**8.95**	10.70
Employed for at least 12 months	2.96	**14.48**	13.89	**17.28**
Average monthly earnings (in thousand lei)	-8.47	8.83	-53.74	-69.99
Months unemployed	-0.73	**-2.75**	**-2.43**	**-3.10**
Months receiving UB payments	**-0.67**	**-0.90**	**-0.65**	-0.45
Sample size	1,109	1,109	631	643
Sample size of the treatment group	747	747	414	401
Sample size of the comparison group	362	362	217	242

Monthly earnings have been deflated using 1998 deflator. Bold numbers indicate significance at the 5% level (two-sided test).

Table 12
Sensitivity Analysis
Impacts of Employment Services Compared to Non-Participation
(Percentage points except where noted)

	PARTICIPANTS VS. NON-PARTICIPANTS		PARTICIPANTS VS. MATCHED NON-PARTICIPANTS	
	Difference of Means	Regression Adjusted (using all observable variables)	Difference of Means (using all observable variables, with the exception of pre-employment history, to match participants to non-participants)	Difference of Means (using all observable variables to match participants to non-participants)
OUTCOMES				
Current experience				
Employed	**12.17**	**12.16**	**9.81**	**8.45**
Employed	**13.61**	**10.67**	**11.81**	**9.72**
Self-employed	-1.12	-0.85	-1.17	-1.17
Average monthly earnings (in thousand lei)	**77.44**	**78.43**	**56.32**	**56.86**
During the two year period 2000-2001				
Employed for at least 6 months	**10.63**	**9.83**	**9.07**	**6.22**
Employed for at least 12 months	**11.49**	**11.93**	**11.24**	**7.65**
Average monthly earnings (in thousand lei)	**71.97**	**84.19**	**62.37**	**87.32**
Months unemployed	**-0.68**	**-2.40**	**-2.57**	**-1.90**
Months receiving UB payments	**-0.25**	**-1.17**	**-1.42**	**-0.74**
Sample size	2,248	2,248	1,724	1,748
Sample size of the treatment group	747	747	746	743
Sample size of the comparison group	1,501	1,501	978	1,005

Monthly earnings have been deflated using 1998 deflator. Bold numbers indicate significance at the 5% level (two-sided test).

Table 13
Sensitivity Analysis
Impacts of Small-Business Assistance Programme Compared to Non-Participation
(Percentage points except where noted)

	PARTICIPANTS VS. NON-PARTICIPANTS		PARTICIPANTS VS. MATCHED NON-PARTICIPANTS	
	Difference of Means	Regression Adjusted (using all observable variables)	Difference of Means (using all observable variables, with the exception of pre-employment history, to match participants to non-participants)	Difference of Means (using all observable variables to match participants to non-participants)
OUTCOMES				
Current experience				
Employed	**12.42**	**7.68**	**7.52**	6.14
Employed	**8.82**	3.25	4.30	2.80
Self-employed	**2.96**	2.07	2.18	2.39
Average monthly earnings (in thousand lei)	**80.04**	40.50	40.67	37.58
During the two year period 2000-2001				
Employed for at least 6 months	**11.06**	**9.92**	**10.27**	**8.38**
Employed for at least 12 months	**8.53**	**8.77**	**9.11**	7.97
Average monthly earnings (in thousand lei)	**80.43**	21.09	40.29	43.08
Months unemployed	**-0.99**	**-1.74**	**-2.30**	**-1.82**
Months receiving UB payments	-0.17	**-0.75**	**-1.11**	**-0.75**
Sample size	1,863	1,863	1,318	1,311
Sample size of the treatment group	362	362	358	350
Sample size of the comparison group	1,501	1,501	960	961

Monthly earnings have been deflated using 1998 deflator. Bold numbers indicate significance at the 5% level (two-sided test).

Table A.1. Estimation of the Propensity Score (marginal effects)

	Coefficient and standard errors		
	ES vs. SBA	ES vs. Non-Participation	SBA vs. Non-Participation
Characteristics			
Male	-.0925456	-.1427264*	-.2015284*
	(.1161069)	(.0725004)	(.0926006)
Age	-.1656625	.0140676	.0284343
	(.1535611)	(.0929445)	(.1061328)
Age squared	.0020078	-.0001519	-.0004043
	(.0018123)	(.0010719)	(.0012505)
Education completed			
Secondary school	-.2818174	.0801002	.0398253
	(.1904824)	(.1099728)	(.1420994)
High school	-.7095729**	-.0840283	.3389603*
	(.2003055)	(.1175862)	(.1468737)
University	-.9529029**	-.0083351	.6136505**
	(.2453969)	(.1411292)	(.1687934)
Persons in the household			
Three	-.0822973	.0232715	.1021722
	(.1813362)	(.1042423)	(.1271709)
Four	.052221	.133011	.0459635
	(.1731464)	(.1018456)	(.1259283)
>four	-.1073348	.0280627	.0726954
	(.190981)	(.1143186)	(.1431552)
Respondent is the main earner	.3417435	.0962171	-.1547861
	(.1809325)	(.1111627)	(.1348952)
Respondent is spouse of main earner	.0569422	-.0487241	-.3095629*
	(.1847614)	(.1115485)	(.1379943)
Region			
Urban <20 thousand inhabitants	-1.191507**	-.1270346	.4965981**
	(.2948134)	(.1306713)	(.1689958)
Urban (20-79 thousand inhabitants)	-.9827035**	.2316202	.2525536
	(.3221553)	(.124284)	(.1768784)
Urban (80-199 thousand inhabitants)	-.1594149	.3309776	.0461624
	(.2952032)	(.119047)	(.1719474)
Urban (200 thousand inhabitants)	-4.656347**	-.0189794	.7366886**
	(.8770943)	(.1976237)	(.2738287)
Counties' unemployment rate	-1.263342**	.0894544	-.1610341**
	(.1876125)	(.0627584)	(.0342555)
Work experience (years)	.0605245	.0307314	.0356114
	(.0801103)	(.0490692)	(.0539121)
Experience squared	-.0011081	-.0007828	-.0007137
	(.0016136)	(.0009607)	(.001081)
1998 employment spell			
1-3 months	.7215252	-.6807008*	-.9830641*
	(.4825013)	(.3418347)	(.499512)
4-6 months	-.114007	-.6466339	-.1562037
	(.5612538)	(.3363872)	(.4336655)
7-9 months	.7544596	-.3247323	-.2502013
	(.5603387)	(.3236533)	(.4274598)
9-12 month	.0857637	-.123323	.9910766*
	(.484743)	(.2971646)	(.4134734)

Table A.1. ***(Continued)***
Estimation of the Propensity Score (marginal effects)

	Coefficient and standard errors		
	ES vs. SBA	ES vs. Non-Participation	SBA vs. Non-Participation
Characteristics			
Average earnings per month in 1998 (in thousand lei)	.0000781 (.0001593)	-.0001 (.0000854)	-.0000 (.0000943)
500-600	.0676088 (.3362484)	-.1813 (.2095827)	-.2457 (.2942938)
601-700	-.0890722 (.2849194)	-.2447 (.1841415)	-.1330 (.249114)
701-850	-.2301096 (.2499098)	-.1748 (.1698717)	-.0327 (.2145763)
851-1,000	.1270668 (.2534562)	-.2043 (.1625509)	-.2962 (.2074279)
1,001-1,200	-.0594705 (.2571654)	-.1763 (.1622569)	-.3793 (.1984934)
1,201-1,500	-.090362 (.2910924)	-.3851* (.1724099)	-.1055 (.1972956)
1,501-1,900	.0037922 (.3443563)	-.4094* (.1938586)	-.3607 (.2262893)
1,901-2,500	-.5107524 (.8254375)	-.9456** (.2595758)	-.3758 (.2408035)
1998 average unemployment spell (months)	.1290935 (.1080122)	.5042** (.0673983)	.3975** (.0973285)
Avg. unemployment spell squared	-.0031777 (.0090996)	-.0387** (.0071279)	-.0289** (.009252)
1998 unemployed at least 9 months	-.8385808 (.7934063)	.2608 (.5406227)	.6637 (.7353178)
Received training during 1998	-.1745353 (.6501546)	-.2614 (.42072)	.5994 (.5026792)
1998 average training length (months)	-.6527418 (.4194368)	.1144 (.1907319)	-.0084 (.2404551)
Sample size	685	1,775	1,326

Standard errors in parentheses. Pseudo-R^2 for all 4 specifications are presented in Table 6, column 4.
***** significant at 5% level; ** significant at 1% level.

Table A.2.

Description of outcome variables

Variables	Definition
At the time of the survey	
Employed or self-employed	Person was employed at the time of the survey (dummy variable)
Employed	Person was employed at a wage or salary job at the time of the survey (dummy variable)
Self-employed	Person was self-employed at the time of the survey (dummy variable)
Average monthly earnings	Average monthly earnings at the time of the survey
During the two year period 2000-2001	
Employed at least 6 months	Person has been employed for at least 6 months during the period 2000-2001 (dummy variable)
Employed at least 12 months	Person has been employed for at least 12 months during the period 2000-2001 (dummy variable)
Months unemployed	Number of months the person has been unemployed during the period 2000-2001
Months receiving UB payments	Number of months the person has been registered with the Public Employment Services and receiving unemployment benefits payment during the period 2000-2001
Average monthly earnings	Average monthly earnings during the two-year period 2000-2001

Note: Earnings are deflated by gross domestic product (base=1998). Earnings are coded as zero if person reported not working at the time of the survey.

Table A.3.
Average Treatment Effects according to Pre-Unemployment History
(Percentage points except where noted)

	Employment services vs. Small-business assistance (1)		Employment services vs. No participation (2)		Small-business assistance vs. No participation (3)	
OUTCOMES	<6 months	>5 months	<6 months	>5 months	<6 months	>5 months
Current experience						
Employed or self-employed	1.66	4.49	**12.25**✓	-3.83✓	4.29	18.98
Employed	5.49	6.76	13.14	-3.49	-0.83	14.93
Self-employed	**-3.43**	-2.27	-0.52	-0.78	3.55	4.09
Average wage (in tousand lei)	-52.03	27.21	**102.01**✓	**-70.20**✓	31.46	**204.01**
During the two year period 2000-2001						
Employed for at least 6 months	**15.19**	13.29	**7.55**✓	-5.02✓	5.64	3.15
Employed for at least 12 months	**22.31**	20.80	**7.33**	-1.15	3.65	4.35
Average wage (in thousand lei)	-86.44✓	55.02✓	**91.47**	18.83	19.68	123.90
Months unemployment	**-3.85**	-4.10	**-2.04**	-0.20	-1.02	-1.55
Months receiving UB payments	-0.95	0.20	**-1.00**	-0.21	-0.70	-0.01
Sample size	388	189	1,282	324	966	208
Size of treatment group	256	99	482	213	244	45
Size of comparison group	132	90	1,282	111	722	163

Monthly earnings have been deflated using 1998 deflator. Bold numbers indicate significance at the 5% level (two-sided test).
✓ indicates that the difference of the two estimated effects is significant at the 5% level.

Table A.4.
Average Treatment Effects according to Gender
(Percentage points except where noted)

	Employment and relocation services vs. Small business assistance (1)		Employment and relocation services vs. No participation (2)		Small business assistance vs. No participation (3)	
OUTCOMES	MALES	FEMALES	MALES	FEMALES	MALES	FEMALES
Current experience						
Employed or self-employed	11.46	-0.92	**8.95**	**8.24**	1.18	2.83
Employed	**13.25**	3.36	11.45	8.20	0.21	-4.32
Self-employed	-1.79	-3.24	-2.32	0.15	0.18	6.01
Average wage (in tousand lei)	**59.51**	-21.86	**85.24**	44.19	8.59	23.63
During the two year period 2000-2001	**18.38**	**11.61**				
Employed for at least 6 months	21.07	15.12	**6.65**	6.83	1.47	**13.15**
Employed for at least 12 months	39.78	-80.88	**8.18**	**9.64**	3.68	9.04
Average wage (in thousand lei)	**-4.75**	-2.25	**109.04**	**59.27**	-21.72	46.86
Months unemployment	-0.60	-0.73	**-2.42**	**-1.79**	-1.03	-1.55
Months receiving UB payments	265	314	-0.33✓	**-1.22✓**	-0.68	-1.16
Sample size	156	197	901	804	790	463
Size of treatment group	109	117	338	400	181	175
Size of comparison group	57	80	563	404	609	288

Monthly earnings have been deflated using 1998 deflator. Bold numbers indicate significance at the 5% level (two-sided test).
✓ indicates that the difference of the two estimated effects is significant at the 5% level.