

Understanding Why Public Employment Services and Small-Business Assistance Programs Work at Getting the Unemployed Back to Work Evidence from Romania

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Abstract

Recent empirical evidence has found that public employment services (PES) and small-business assistance programs (SBA) are often successful at getting the unemployed back to work. One important concern of policy makers is to understand the suitability of these programs for different population subgroups, as well as the channels through which these programs work. Using unusually rich (for transition economies) survey data and matching methods, this evaluation study enhances our understanding of the potential of PES and SBA in transition economies, in general, and Romania, in particular. The main result of the paper is that heterogeneity matters and that ALMPs need to be tailored to the problem at hand, rather than following a one-size-fits-all approach. Our results show that PES are effective for workers with little access to informal job-search channels, and that SBA works for workers with less access to the primary labor market sector (that is, high productivity wage and salary jobs). The paper concludes with interesting policy recommendations for implementing these programs both in transition countries and in countries with large informal sectors, such as developing countries.

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

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

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I. Introduction

In recent years, there has been a substantial increase in the empirical evidence on the effectiveness of active labor market programs (ALMPs) in developed, developing and transition economies.² This improvement can be explained by the increased availability of data, the improvements on data quality, the recent developments on evaluation methodology, and policy makers' increased interest in information on policy effectiveness. In this literature, two ALMPs have emerged as being particularly successful for getting the unemployed back to work: public employment services (PES), and small-business assistance programs (SBA). According to Kluve, 2006, a consistent result for both Europe and the U.S. are the positive effects of PES and SBA. Martin and Grubb, 2001, also find that these programs 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 programs tend to be successful in developing and transition countries.

There are, however, considerable differences in the design of these two types of programs. On the one hand, PES include different types of measures aimed at improving job search efficiency. Among the different services offered, PES include: job clubs, job-search courses, counseling, testing, and assessment. In addition, their relative low costs make them one of the most cost-effective ALMP (Martin, 1998, Dar and Tzannatos, 1999, and Kluve, 2006, among others). On the other hand, SBA programs are usually intended to support the start-up and development of self-employment endeavors or micro-enterprises. They usually provide counseling and assistance in developing and implementing a business plan, and often include some form of financial assistance. Although the use of these programs has been limited compared to other ALMPs, their popularity—as well as the number of empirical evaluations available—has recently increased (Kluve, 2006; and Baumgartner and Caliendo, 2008).

Most previous evaluation studies focus on analyzing the average effects of these two ALMPs for the population of unemployed or for the specific population subgroups for which the program is targeted. However, few studies analyze the relative effectiveness of these programs across different subgroups of unemployed. While evaluating the average effects of ALMPs relative to non-participation is an interesting question per se, just as important is to understand the suitability of these programs for different population subgroups as well as the channels through which these programs work. Drawing on a rich individual-level data set and analyzing these data with matching methods, this evaluation study enhances our understanding of the potential of PES and SBA in transition economies, in general, and Romania, in particular. The main result of the paper is that heterogeneity matters. The paper carries forward the important insight that ALMPs need to be tailored to the problem at hand, rather than following a one-size-fits-all approach. The paper concludes with interesting policy recommendations for implementing these programs both in transition countries and in countries with large informal sectors, such as developing countries.

When targeted to specific groups, PES seem to work best for women on welfare in the United States (Meyer, 1995, and Heckman et al., 1999) and the long-term unemployed in New Zealand (NZ DOL, 1995), Holland (OECD, 2003), and the United Kingdom (Dolton and O'Neil, 1996, and Van Reenen, 2003). In contrast, PES do not seem to be successful for young workers in Canada (HRDC, 1996) and Holland (OECD, 2003), or mass layoffs in Canada (HRDC, 1996). However, because these studies are evaluating PES targeted to a specific population subgroup, they are unable to analyze the relative effectiveness of the program across different subgroups. To identify the population subgroups that are most likely to benefit from a specific program, it is worthwhile estimating the relative impact of a program across subgroups. This approach reduces the sensitivity of the results to differences in labor

² See Katz, 1994, Fay, 1996, Martin, 1998, Dar and Tzannatos, 1999, Martin and Grubb, 2001, Betcherman, Olivas, and Dar, 2004, and Kluve, 2006, for good reviews of the literature.

market conditions, institutions, evaluation designs, availability of outcome variables, and time periods. When looking at studies that evaluate heterogeneity results, the results for PES are mixed. While there is some indication that PES mainly work for individuals with sufficient skills and better labor market prospects (Dar and Tzannatos, 1999, Weber and Hofer, 2004, Fawcett, 2001, and Kluge, 2006), some studies conclude that the opposite is the case (Fay, 1996, Fougère et al., 2005, Van der Berg and Van der Klaauw, 2006). In the case of SBA, the amount of evaluation studies that evaluate their average effectiveness is considerably smaller.³ Of those evaluations that conduct some heterogeneity analysis, their results suggest that these programs 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 (Fay, 1996, Wilson and Adams, 1994, Kosanovich and Fleck, 2001).

Despite the recent increase in the number of evaluation studies in transition countries, the evidence continues to be limited and has tended to concentrate on evaluations of training programs or public employment (or public works) programs, which have been the most popular programs to be implemented during early transition from central planning to market economy. Although many findings on PES and SBA effectiveness from OECD countries may well apply broadly to transition economies, there are common underlying specificities—such as, the important underground economy, the frequently used informal job-search channels, the administration’s weak capacity and reduced resources to implement ALMPs, the major economic restructuring, and the surge of the small- and medium-enterprise sector—that may limit what some programs can achieve in terms of creating formal employment or increasing wages (see OECD, 2000, Nesporova, 2002; Irac and Minoiu, 2006; and Vidovic, 2004, among others, for thorough discussion on the economic and institutional specificities of central and south-east European countries).

We argue that the analysis presented in this paper, although focusing on the Romanian case, is relevant to a larger set of transition countries, especially south-east European countries, as these countries experienced a slow transition process, and shared an underlying specificity of the rural sector and high poverty levels. Using unusually rich (for transition economies) survey data and matching methods, the analysis reveals that average effects for the population as a whole may hide statistically and economically significant differences across subgroups. We relate these differences to the different institutional set-ups and discuss theoretical implications, which are then empirically contrasted with the heterogeneity effects. The estimation of heterogeneity effects reveals that, compared to non-participation, PES are effective for workers with little access to informal job-search channels—such as young workers and those living in rural areas—and that SBA work best for workers with less access to the primary labor market (that is, less access to high-productivity wage and salary jobs)—such as the less-qualified workers or those living in rural areas. These findings are compatible with improved job matching theory for PES, and segmented labor market theory for SBA. Finally, the findings in the paper suggest important policy implications since they provide some guidance on which populations would benefit from PES in economies with large informal job-search sectors, and which type of workers are most likely to succeed when participating in SBA in transition economies.

The research presented in this paper is closest to Benus and Rodriguez-Planas, 2008, as it uses the same data set. The data used is drawn from a follow-up survey specifically designed and collected for this evaluation. The most important reasons for using survey data instead of administrative data were that the former allowed us to track individuals’ earnings and employment status at different points in time over a four-year period, and provided us with good quality data on key variables—such as earnings for both the employed and the self-employed.⁴ However, it should be noted that there are

³ In their 2004 paper, Betcherman, Olivas, and Dar have identified 13 scientific evaluations of SBA. Since then, I have identified about half a dozen more.

⁴ While administrative data frequently report earnings for wage and salary workers, they usually lack of information on earnings for the self-employed. In the case of Romania, register data containing individuals’ employment and earnings history was unavailable (see section III for more detailed information on the data used and the reasons for using these data).

also drawbacks with the survey data used such as, the limited sample size, the imperfect recall of the interviewed individuals for events that have occurred some time ago, and the possible misleading replies. Benus and Rodriguez-Planas, 2008, study focuses on the average effects of ALMPs relative to non-participation, and tests the sensitivity of their results to the availability of information on employment history and earnings before the treatment. Their paper documents that pre-treatment labor force and earnings information contain indispensable information regarding selection into treatment and that controlling for these variables can eliminate a large part of the overt bias between treated and comparison units. However, their paper does not address the heterogeneity effects of the different programs, and it does not discuss the programs' theoretical implications and empirically contrasts them with heterogeneity results.⁵

This paper is organized as follows. The next section presents an overview of the Romanian labor market, focusing on self-employment, unemployment and the ALMPs under evaluation. Section 3 provides a theoretical discussion of the different channels through which these two programs might work, and relates these alternative theoretical explanations with previous empirical findings. Section 4 explains the methodological approach to estimate the treatment effects and presents the data. Section 5 analyzes the results, and section 6 concludes with a discussion on policy implications. An additional appendix provides further information on the data and detailed empirical results.

II. Economic and Institutional Background

II.1. The Economic Context

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

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

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

However, data on registered unemployment in Romania understate the real problem with dislocated workers for at least the following two reasons. First, during the 1990s the increase in open unemployment was contained by Romania's policy approach of limiting job destruction by adjusting through real wages, combined with a series of early retirement programs. However, these two policies pushed workers out of the labor force and into low productivity jobs, primarily in subsistence agriculture and the urban underground economy—for instance, the share of agricultural employment (much of it being subsistence

⁵ Although the 2007 working paper version contains some brief heterogeneity analysis, this has been eliminated from the most updated version (available from the author upon request).

agriculture) in Romania rose from 28 % in 1989 to 42 % in 2001. And second, 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 labor market performance.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 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 and reached 65.5% in the year 2000.

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. Below, we list the most relevant factors explaining the slow development of Romanian SMEs’ sector.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 was first legalized 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 privatization 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. Finally, SMEs’ financing has been expensive due to the high and variable inflation rates, and access to bank loans has been limited to SMEs and when available, they were generally short-term, small and required collateral.

II.2. The Institutional Environment

As early as 1991, Romania adopted passive labor programs, including *unemployment benefits*, *allowance for vocational integration* and *support allowance*. To be eligible for these benefits, unemployed individuals had to: be registered at the local employment office; be aged eighteen and over; have an income less than half of the indexed national minimum wage; and be in one of the following two covered groups: (1) employees having worked for at least 6 months during the last 12 months; or (2) be a recent graduate from school or university unable to find suitable employment. Unemployment benefits were paid for a maximum duration of 9 months. The level of these benefits ranged from 50 to 60 % of the average monthly salary during the last 3 months of employment for displaced workers. For new entrants, benefits varied by the level of education and years of experience for those with prior work experience. After exhausting unemployment benefits, those who remained unemployed received a support allowance (of 60 % of the indexed minimum wage) for a maximum period of 18 months.⁸

Like in other countries with large rural population and important underground economy, the administration of unemployment benefits has been a relatively difficult task in Romania because the employment status of individuals in rural areas is often unclear, and there is frequently no reliable documentation of incomes for workers in subsistence agriculture or the urban informal sector. A consequence of this is that effective targeting of benefits on the unemployed is not very precise in

⁶ See Brown et al., 2006, for a careful study on nonstandard forms and measures of employment and unemployment in Romania.

⁷ For thorough 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.

⁸ See Earle and Pauna, 1998, for a detailed description and thorough analysis of this program in Romania.

Romania, as reflected by the fact that approximately one-third of all persons whom the Labor Force Survey (LFS) counts as unemployed are usually not registered as unemployed at the local employment office, while close to half of the registered unemployed are not LFS unemployed (OECD, 2000).

In addition, while in many countries unemployed workers receive some form of public employment services, during most of the 1990s, the Romanian Ministry of Labor and Social Protection devoted most of its staff and resources to strengthen the capacity of local employment offices to administer the claims for unemployment benefits. Thus, even though registration in the local office was compulsory to receive unemployment benefits and had to be renewed monthly by a personal visit to the employment office, it only involved a simple signing-up procedure, that is, there was no initial interview before being registered as unemployed for the first time, nor were there regular counseling interviews.

In 1997, the Romanian government launched the real start of active programs on a significant scale by signing a loan agreement with the World Bank. The two major programs offered were (1) PES, and (2) SBA. Altogether, these two programs served more than 80% of the unemployed who received some kind of ALMP in Romania during that period. Although these programs were designed and implemented by county level Agencies for Employment and Vocational Training, the services were not provided by the county agencies themselves, but were contracted out to public or private service providers. Contracts to service providers were awarded with built-in incentives to improve labor market impact such as negotiated levels of job placement and business start-up, with financial incentives to meet objectives and disincentives if objectives were not met. Thus, service providers were likely to select those unemployed individuals most likely to succeed in completing their program and accessing employment.

The two programs were clearly different program types as evident from the description of their key characteristics described below:

- **Public Employment Services (PES).** Clients eligible for this service were offered a variety of employment services, including job and social counseling, labor market information, job search assistance, job placement services, and relocation assistance. Notice that PES focused on counseling but did not include monitoring or sanctioning eligible workers who did not comply with the job search planning. The duration of these services was limited to 9 months per individual. The program also offered up to 2 months of salary at the minimum wage. Service providers had to agree to a negotiated job placement rate of at least 10 %.
- **Small-Business Assistance (SBA).** Provision of these services included initial assessment of the aptitude and skills of unemployed persons to start businesses, developing business plans, advising on legal, accounting, financial, marketing and sales services issues, assistance in the dialogue with local authorities, short-term entrepreneurial courses and training and other consulting services to unemployed workers who intended to start a business. There were also provisions for short-term working capital loans of up to \$25,000 U.S. dollars to program participants. Service providers had to agree to a negotiated business start-up rate of at least 5 % of clients initially contacted. Maximum length of initial contract was 12 months.

As indicated in Table 1, between PES and SBA, there were 180 contracts completed as of September 1st, 2001, and close to 52,000 clients served. The placement rate among these contracts was close to one fifth. PES provided assistance to 31,679 individuals at an average cost of only 123.74 thousand lei per client (about 12 U.S. dollars per client). In contrast, SBA served a smaller number of clients (20,293 clients) and the cost per client for this program was 179.15 thousand lei per client (about 17 U.S. dollars per client).

Table 1
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)
SBA	92	20,293.00	3,568	17.58%	3,635,562,636.30	179,153.53	1,018,935.72
PES	88	31,679.00	6,610	20.87%	3,920,060,312.43	123,743.18	593,049.97

Costs figures have been deflated using 1998 deflator.

Source: USDOL Technical Assistance Support Team.

III. Theoretical Considerations and Previous Empirical Findings

This section presents a theoretical discussion of the different channels through which these two programs might work, and discusses previous empirical findings consistent with these theoretical explanations.⁹ The competing theories discussed are: (1) improved job matching; (2) segmented labor markets; (3) human capital; and (4) signaling. Although it is not possible to derive strict tests for the relative importance of these explanations, systematic heterogeneity of the effects between different groups of unemployed will provide evidence consistent with one theory but not with another (this follows the strategy used by Gerfin et al., 2005). Section V presents separate treatment effects for PES and SBA compared to non-participation in any program (that is, non-participation is considered a benchmark given that the theoretical effects of the programs discussed below will not take place.)

Improved Job Matching

Both PES and SBA may improve job matching for different reasons. On the one hand, the main objective of PES 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 networks and contacts to unemployed workers that could (and sometimes does) result in wage and salary job offers. For instance, Kosanovich and Fleck, 2001, find that more than 45% of participants of a self-employment assistance program in New Jersey ended up working in wage and salary jobs. They find similar results in Maine and New York, where close to 60% of participants of self-employment programs ended up working in wage and salary jobs. Their 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.

A priori, one would think that PES should be more efficient at improving job search than SBA since the job matching mechanism for the latter program is the result of an indirect, and thus, secondary effect of the program. However, theoretical models on the effect of PES find that the efficiency of PES alone (that is, without monitoring) is seriously questionable. More specifically, the theoretical discussions highlight the importance of distinguishing between two search channels: the *formal* one, which refers to formalized search methods, such as PES, and the *informal* channel, which occurs when workers receive job offers through referrals by an employed worker, a family member, or a friend. For instance, Van der Berg and Van der Klaauw, 2006, show that in an endogenous job search model with formal and informal search channels, job search programs without monitoring are useless. In their model, only job search programs with monitoring increase unemployed workers' exit rate to work. This increase is merely explained by the effect the program has on individuals with unfavorable labor market prospects (individual or macro-economic). Since these individuals have

⁹ See Betcherman, Olivas, and Dar, 2004, and Kluge, 2006, for recent good reviews of the literature on PES and SBA in developed, developing and transition countries. In addition, Baumgartner and Caliendo, 2008, present a thorough review of previous empirical findings on self-employment programs.

little access to the informal search channel, monitoring of their formal search leads to an increase in their exit rate to work. In contrast, individuals with relatively favorable prospects, and thus, with access to informal search channels, monitoring is inefficient since it only leads to substitution of informal search channels to formal ones.

Consistent with this theoretical discussion, empirical evidence on PES in countries with large informal search sectors, such as developing countries, have shown that these services may have limited reach as workers may prefer other channels of job search (Woltermann, 2002). In addition, the empirical literature on the use of different search channels indicates that workers with characteristics such that their chances to find a job are low because of little access to informal search channels (such as young workers) or workers for whom their informal search channels have dried up (such as workers living in depressed areas) rely to a relatively large extent on formal search channels (Van der Berg and Van der Klaauw, 2006, Abbring et al., 2005, Keeley and Robins, 1985). Because PES facilitate job finding through the formal channel, we would expect it to have stronger effects for these type of workers than for those with access to informal search channels (such as older workers and workers living in more dynamic areas).

Segmented Labor Markets

The dualistic view perceives the labor market in developing and transition countries as segmented by two sectors: the *primary* (or modern) labor market, characterized by jobs with high-productivity growth and good benefits, and the *secondary* labor market, where more traditional and less productive jobs are found. This dualistic view easily applies to Romanian labor market, as reflected by OECD's conclusion that *Romania's central labor market problem is not so much unemployment as a prevalence of employment in low-productive activities, which often correspond badly to the needs of the households and to those of their country* (OECD, 2000). According to the OECD report, many of those who cannot find employment in the primary labor market do not become unemployed because they engage in alternative types of activity in the subsistence agricultural sector or the urban underground economy.

This view argues that individuals working in the unregulated self-employed sectors are frequently seen as the disadvantaged segment of the labor force rationed out of salaried employment (Fajnzylber et al., 2006). In other words, workers with little access to the primary labor market enter self-employment while queuing for wage and salary jobs. This phenomenon is seen as particularly tied to transition countries, as indicated by Köllö and Vincze, 1999: *Many interpret the substantial rise in the number of entrepreneurs in Central and Eastern Europe as a temporary response to the 'transformational recession': during the hard times many people started a business temporarily and 'unwillingly' because it was difficult to find wage work.*

This segmented labor market view predicts that SBA ought to have stronger effects on individuals with little access to the primary labor market (such as, younger workers, less educated workers, and those living in more distressed areas), because it will give its participants a comparative advantage relative to other individuals who enter self-employment without any assistance. In contrast, individuals with access to the primary labor market—and thus, to high-productivity wage and salary jobs with work benefits—are likely to be little interested in entering self-employment, implying that offering them SBA ought to be quite ineffective.

Human Capital Theory

The impact of PES on human capital is likely to be small since the program 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. Consistent with this, there is some empirical evidence that business-training programs improved participants' business knowledge and productivity, measured by revenues, repayment, and client retention rates (see for example, Karlan and Valdivia, 2006).

Assuming that human capital may be a complement to managerial activity (as argued by Rees and Shah, 1986, and Cressy, 1996), SBA will work best for more educated workers. This is consistent with empirical evidence from the U.S. that finds that the probability of survival of SME is positively related to the level of education of their owners (Bates, 1999). In addition, if individuals acquire more capital, knowledge of business opportunities, and managerial ability while working, SBA ought to also have more of an impact with older and therefore more experienced workers. Not surprisingly, the empirical evidence suggests that SBA programs work best for unemployed workers who have entrepreneurial skills and the motivation to survive in a competitive environment, such as, highly educated prime-aged men (Fay, 1996, and Wilson and Adams, 1994), or young educated workers and those for whom self-employment was related to some ongoing activity (Almeida and Galasso, 2007).

Signaling Theory

Participating in SBA may also have a signaling 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 and more reliable entrepreneurs since they have institutional support compared to 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 PES. 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 PES.

According to this view, SBA should be more effective 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. A similar prediction would hold for more educated workers since they have lower costs of acquiring entrepreneurial skills than less educated ones. 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 experienced and educated facilitates access to entrepreneurial activities. Thus, if signaling is important, I expect SBA to have more of an impact compared to non-participation for older and more qualified workers.

IV. Methodological Approach and Data

IV.1. Identification

We follow the potential–outcome–approach to causality (Roy 1951; Rubin 1974) and base our analysis on comparing the outcomes of two alternative strategies available to displaced workers: to participate in a particular ALMP, or to continue searching for a job as openly unemployed.¹⁰

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

¹⁰ We considered basing our analysis on the “multiple treatments” model. However, the socio-economic differences across the different treatments combined with the relative modest samples, lead to large losses of observations due to the common support requirement, and poor matching.

the treatment shows for those individuals who actually participate in a program. The ATT is given by:

$$\Delta = E(Y^1 | D = 1) - E(Y^0 | D = 1) \quad (1)$$

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

To correct for possible selection bias, we use matching, which intends to mimic a randomized experiment *ex post* by balancing the distribution of covariates in the treatment group and the matched comparison group.¹¹ This strategy is feasible if there is only “overt bias” (Rosenbaum 1995), that is, treatment and comparison group differ prior to treatment only in observable variables that matter for the outcome under study. Let X denote the vector of observed pre-treatment variables, or covariates. Then the concept of “selection on observables” is formalized in the following *conditional identifying assumption (CIA)*: The assignment mechanism D is independent of the potential outcomes (Y^1, Y^0) conditional on X (Rubin 1974, 1977). This assumption is commonly referred to as *unconfoundedness*. As we are interested in ATT only, we only need to assume that Y^0 is independent of D conditional on X (that is, $Y^0 \perp D | X$, where \perp denotes independence) because the moments of the distribution of Y^1 for the treatment group are directly estimable. Clearly, this assumption may be a very strong one and has to be justified on a case-by-case basis, as the researcher needs to observe all variables that simultaneously influence participation and outcomes. In section IV.3., we discuss the plausibility of this assumption for our evaluation. Additionally, it has to be assumed that there is *weak overlap*: $\Pr(D = 1 | X) < 1$, for all X . This implies that there is a positive probability for all X of not participating, that is, that there are no perfect predictors which determine participation. These assumptions are sufficient for identification of the ATT, which can be written as:

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

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

¹¹ See Imbens (2004) or Smith and Todd (2005) for recent overviews regarding matching methods.

IV.2. Data and Descriptive Statistics

One of the biggest challenges when evaluating ALMPs in transition economies is the quality and quantity of data—see Kluge et al., 1999, or Earle and Pauna, 1996, among others, for discussion on the poor quality of ALMPs' data in transition economies. In the case of Romania, we were unable to use the Labor Force Survey because it is cross-section and it is missing several important variables, such as earnings, ALMPs participation, or retrospective questions on labor market history. Unfortunately, we were also unable to use the official unemployment data because the available information reports total number of unemployed and the number in a few basic categories (such as sex, age groups, counties (*judets*), broad educational groups, and ALMPs), but does not report individual characteristics and experiences of the unemployed.

For these reasons, we collected survey data from computer-assisted telephone interview.¹² To do so, from each program, we randomly drew participants whose ALMP contract began in 1999. As we wanted to compare them with non-participants, we had to choose a potential comparison group.¹³ We restricted this comparison group to those who were registered at the Employment Bureau around the same time and in the same *judet* (county) than participants but who did *not* participate in an ALMP during 1999. Additionally, participants and non-participants received the same questionnaire.

To select non-participants, we first determined, the number of participants that were selected for the participant sample in each of the *judet*. Next, in each *judet*, we selected an equal number of non-participants from the same Employment Bureau register list. Participants and non-participants were interviewed during January and February of 2002, and asked questions on employment and earnings: (1) at the time of the survey, (2) during the years 2000 and 2001, and (3) during the year 1998, that is, the year before participating in the ALMPs. Restriction that all data be available led to a sample of 2,610 individuals. Of these 1,109 had participated in either in PES or SBA during 1999, and 1,501 were non-participants during the same year. We refer to the Benus and Rodriguez-Planas 2007 working paper for a thorough discussion of the sample design and the survey effort. In addition, Table A.1. in the appendix displays information on survey and item non-response, which, as is common in this type of studies, were slightly lower for participants than for 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 we restricted our sample to having all data available. Table A.2. in the appendix provides sample mean values for the completed interviews—columns (1)—and for the final sample, that is, after applying the restriction that all data be available—columns (2)—, by treatment status. The differences between sample means in columns (1) and (2) are minor for all treatment status. In addition, we could not reject the null hypothesis that each of the sample means from columns (1) and (2) were equal at the 95% confidence level.

Compared with available administrative data, our survey provides detailed information on: (1) individual labor market histories and earnings prior to 1999 unemployment spell (such as, the level of experience, usual monthly earnings, unemployment history, and any prior participation in a training program); (2) individual socio-demographic information (such as, age, gender, family composition and

¹² The practice of collecting survey data specifically designed for the evaluation of ALMPs in transition countries is quite common (see, Terrell and Sorm, 1999; Earle and Pauna, 1996 and 1998; Benus et al., 2001, O'Leary et al., 1998, Baumgartner and Caliendo, 2008, among others). Some exceptions include, Kluge et al., 2008, which were able to exploit retrospective individual labor market behavior from a supplementary questionnaire to the Polish Labor Force Survey, and evaluations of ALMPs in East Germany, which used unique recently available integrated data set from various administrative sources.

¹³ The recent evaluation literature has thoroughly discussed how this potential comparison group should be chosen. For instance, comparing participants with individuals who never participate is inadequate, as it can be assumed that the latter group is particularly selective (see discussion in Sianesi, 2004). In addition, it may invalidate the conditional independence assumption due to conditioning on future outcomes (see discussion in Fredriksson and Johansson, 2004).

whether the person is the family's main wage earner); and (3) information on individuals' employments and earnings at least 24 months after the program started. We combined these data with data capturing the local labor market conditions. Table 2 contains sample mean values of the most relevant variables (a full list of the sample mean values for all available variables can be found in Table A.3. in the Appendix).

There are drawbacks with the survey data used. One of the biggest concerns with these data is the possibility that recall bias for events that have occurred some time ago may affect our estimates. Empirical evidence has shown that unemployment that occurred some time ago tends to be underreported in relation to more recent unemployment (see, for example, Jürges (2005) for a thorough study on retrospective errors and inconsistencies in the unemployment information using survey data.). Similarly, there is evidence that unemployed workers tend to comparatively underreport unemployment and wage losses when these are associated with less salient events (Oyer, 2004; and Song, 2006, among others.) That said, for recall bias to be affecting our estimates, the size and direction of the recall bias would need to differ between the treatment and control groups. This could easily occur if there are compositional differences between the two groups. However, the matching methodology used to calculate our estimates balances the distribution of covariates in the treatment group and the matched comparison group and thus aims to reduce the compositional differences between the two groups—a thorough discussion on the methodology used and the quality of the match can be found in the section IV.3. below. We argue that although recall bias is likely to occur with our survey data, the odds that it affects our estimates are considerably reduced by the methodology and the rich data used.

It is also important to highlight that our sample contains information only on individuals who are registered unemployed, excluding all of those who might be unemployed according to a standard labor force survey definition, but who, for whatever reasons, have failed to register at the local office.¹⁴ In addition, we need to be kept in mind that the sample of non-participants selected does not represent a random sample of registered unemployed, as they were selected to match the distribution of age, gender, education and region of our sample of participants. Although this does not affect our estimation and interpretation strategy, it should be kept in mind when interpreting the differences between groups.

Table 2 displays selected descriptive statistics for socio-economic variables for participants of PES or SBA, and for non-participants. We restricted the data to the 25 to 55 years old in order to rule out periods of formal education or vocational training as well as early retirement. The descriptive statistics conform to our expectations that different types of displaced workers participated in the different ALMPs. The results are summarized below.

As we saw in Section II, PES and SBA are quite different program types, and not surprisingly, they do not naturally serve the same population among the unemployed. Whereas one would think , for instance, that PES are rather broadly applicable and useful for all unemployed individuals, perhaps even more so the more disadvantaged, SBA programs conceivably seem to cater the more advantaged among the unemployed, who a priori would be thought to have better prospects of succeeding with their own business. This is reflected by the distribution of educational attainment among the two groups, as well as by their employment history during 1998: SBA participants were more educated, employed for a longer share of the year and worked in better paid jobs than PES participants. Another difference is that PES participants were more likely to live in large urban areas than SBA participants.

¹⁴ Registered unemployment in Romania is measured according to national legislation and differs considerably from unemployment as recorded by the Labor Force Survey. For instance, part of the registered unemployed are classified as employed in the Labor Force Survey and vice-versa (Earle and Pauna, 1996, OECD, 2000; and Romania National Institute of Statistics, 2006, among others).

Table 2
Selected Characteristics of ALMP Participants and Non-Participants
(Percentages except where noted)

Characteristics	PES (1)	SBA (2)	Non-Participants (3)
<i>Pre-program Characteristics</i>			
Male	45.92	50.69	63.82
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
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
Work experience (years)	23.99 (8.28)	22.99 (8.04)	23.63 (8.91)
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
1998 usual monthly earnings (in thousand lei)	758.07 (618.68)	881.72 (756.83)	926.60 (719.77)
Average unemployment length during 1998 (months)	3.90 (4.70)	3.38 (4.87)	2.99 (4.56)
Received training during 1998	6.69	8.86	3.13
<i>Post-program Outcomes</i>			
Current experience (January or February 2002)			
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 (485.19)	303.28 (384.02)	232.62 (389.45)
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 (426.58)	398.60 (475.21)	322.42 (357.41)
Months unemployed	9.45 (9.39)	10.36 (9.43)	12.14 (9.78)
Months receiving UB payments	0.79 (3.03)	1.44 (4.70)	1.79 (5.05)
Sample size	747	362	1,501

Standard deviation in parenthesis for continuous variables

IV.3. Validity of the CIA

The CIA is in general a very strong assumption and the applicability of the matching estimator depends crucially on its plausibility. Only variables that influence the participation decision and the outcome variable simultaneously should be included in the matching procedure (Caliendo and Kopeinig, 2008). Hence, economic theory, a sound knowledge of previous research, and information

about the institutional setting should guide the researcher in specifying the model (see Smith and Todd, 2005 or Sianesi, 2004, among others). In this section we discuss the plausibility of such an assumption for the current evaluation study.

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

Many have argued that program participation probability depends upon the variables determining re-employment prospects once unemployment began (Heckman and Smith, 1999, Sianesi, 2004, and Fitzenberger and Speckesser, 2006, among others). Following these authors, we argue that the level of previous earnings, and some information about pre-program unemployment history are important factors in determining whether an individual will participate in any program, as well as in which of the programs. In addition, these variables are also likely to influence the future labor market outcomes, and thus, in order for CIA to be plausible, they should be included in the estimation of the propensities.¹⁵ Finally, previous earnings are also a good proxy for workers' pre-displacement job characteristics and workers' motivation, ability and soft skills.

We also included in the propensity score estimation socio-demographic and human capital variables. Among the first group of variables, we have information on age, and gender, as well as family composition and whether the person is the family's main wage earner. Among the second group of variables, we included information on the individual's education level, her experience prior to participation, and whether she had participated in any training program during 1998 and the duration of that program. Both economic theory and previous empirical evaluation studies have highlighted the importance of these human capital variables in determining the program participation as well as future labor market prospects.

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

IV.4. Estimation of the Propensity Score and Matching Details

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

First, we estimated binary conditional probabilities for each of the programs vs. non-participation. The results of the two probit estimations can be found in Table A.4 in the Appendix, which also provides the reader with a more exact description of the variables used in the analysis.¹⁶ Second, we used the output from these selection models to estimate choice probabilities conditional on X –the so-called propensity scores, $P(X)$ –for each treatment and potential comparison group member. We then imposed the common-support requirement to guarantee that there is an overlap between the propensity

¹⁵ While it is true that we only have information on unemployment history for the year prior to becoming unemployed, Fitzenberger and Speckesser, 2007, have recently find that the omission of the employment history beyond 12 months before the beginning of the unemployment spell does not invalidate their main results for West Germany (no data beyond 12 months was available for East Germany).

¹⁶ To ensure comparability between the estimates we chose the same covariates for each combination.

scores for each pair (few treated observations were dropped overall, as illustrated in column 9 of Table 3). Third, for each treatment group member, we 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.¹⁷ In addition, the selection method used was kernel-based matching, which uses all of the comparison units within a predefined propensity score radius (or “caliper of 1%”). Kernel matching assigns positive weight to comparison observations with propensity scores similar to that of each treated observation, where the weights decrease with the propensity score distance. Formally, the matching estimators have the general form:

$$\Delta^{MATCHING} = \frac{1}{N_1} \sum_{i \in \{D_i=1\}} \left[Y_i^1 - \sum_{j \in \{D_j=0\}} w_{N_0}(i, j) Y_j^0 \right]$$

where N_1 denotes the number of participants i in the treatment ($D = 1$ observations), and N_0 denotes the number of non-participants j ($D = 0$ observations). In the case of kernel-based matching, the weight function $w_{N_0}(i, j)$ is calculated as follows:

$$w_{N_0}(i, j) = \frac{G\left(\frac{P_i(X) - P_j(X)}{a_n}\right)}{\sum_{k \in \{D_k=0\}} G\left(\frac{P_i(X) - P_k(X)}{a_n}\right)}$$

where G denotes a kernel function and a_n denotes an appropriately chosen bandwidth.¹⁸

One major advantage of kernel-based matching is the lower variance which is achieved compared to other possible alternatives, such as, the nearest-neighbor matching, because more information is used for constructing counterfactual outcomes. As our treatment and comparison groups are rather small, we prefer this method over the nearest-neighbor matching.¹⁹ When there were multiple matches, each non-participant received a weight that reflects the number of successful matches within the caliper range.²⁰ To adjust for the additional sources of variability introduced by the estimation of the propensity score as well as by the matching process itself, bootstrapped confidence intervals have been calculated based on 1,000 re-samples.^{21 22}

¹⁷ 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 (Rosenbaum, 1995).

¹⁸ We used Epanechnikov kernel.

¹⁹ In addition, Plesca and Smith, 2007, have found results that highlight the relatively poor performance of the widely used single nearest neighbor matching estimator.

²⁰ By using more comparison units, one increases the precision of the estimates, but at the cost of increased bias.

²¹ Heckman et al., 1997, derive the asymptotic distribution of kernel-based matching estimators and show that bootstrapping is valid to draw inference. This is an additional advantage of this matching method compared to alternative methods, such as nearest-neighbor matching, since it allows circumventing the issues regarding nearest-neighbor matching raised by Abadie and Imbens, 2006.

²² Estimations are done using the PSMATCH2 Stata ado-package by Leuven and Sianesi (2003).

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

	Number of treated before ^a	Number of nontreated before ^a	Treated as a percentage of nontreated before	Probit pseudo- R^2 before	Probit pseudo- R^2 after	Pr > X^2 After	Median bias before	Median bias after	Number of treated lost to common support after
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ES vs. No participation	747	1,028	72.67%	0.174	0.017	0.533	9.36	2.88	4
SBA vs. No participation	362	964	37.55%	0.162	0.013	0.985	11.31	2.29	12

^aThe difference in the number of treated and non-treated in the different 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 program 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, 1983, 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:

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

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

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

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

To test if the matching procedure is able to balance all the covariates, we 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 et al., 2005, among others). Columns 7 and 8 of Table 3 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: for PES, the median absolute standardized bias dropped from 9.36 % before the matching to 2.88 % after the match; and for SBA, the median absolute standardized bias dropped from 11.31 % before the matching to 2.29 % after the match. In addition, following Sianesi's (2004) suggestion, we re-estimated the propensity score on the matched sample (that is, on the participants and matched non-participants) and compared the pseudo- R^2 values before and after matching (columns 4 and 5 of Table 3). We find fairly low pseudo- R^2 values after matching, indicating that, after matching, there are no systematic differences in the distribution of the covariates between the two groups. Finally, the P-value of the likelihood test after the matching rejects joint

V. Empirical Results

V.1. Measurement of Labor Market Outcomes

Because the primary objective of these policies is to get displaced workers back to work in jobs, at least implicitly, as good as the previous one, the analysis focuses in two types of outcomes: those that measure workers' reemployment probabilities (in paid or self-employed jobs), and those that measure workers' earnings at the new job.²³ 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. Measuring employment experience with employment for a period of at least 6 and 12 months, respectively, during the years 2000 and 2001 provides additional information on workers' reemployment experiences over the two-year period prior to the survey, and informs 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 program participation could save. I also computed accumulated months receiving unemployment benefits (UB) during the two-year period 2000-2001. The outcomes by treatment status are summarized at the bottom of Table 2.

V.2. Average Results

Impacts were estimated as the difference in average outcomes between the treatment and the comparison group. ATT estimates and their bootstrapped 95 percent confidence intervals are

²³ 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.

shown in Table 3. The main results are summarized below.

Overall PES were found to be successful in improving participants' economic outcomes compared to non-participants in all dimensions. PES had a positive impact both on employment at the time of the survey and on employment during the two-year period 2000-2001. 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.²⁴ Similarly, PES 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 program 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, PES 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 program 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.

Table 3
Average Treatment Effects of PES and SBA
(Percentage points except where noted)

OUTCOMES	PES vs. No participation	SBA vs. No participation
Current experience		
Employed or self-employed	8.45 (3.19; 13.90)	6.14 (-0.44 ; 12.29)
Employed	9.72 (4.17 ; 15.12)	2.8 (-3.93 ; 9.55)
Self-employed	-1.17 (-3.75 ; 0.65)	2.37 (-1.01 ; 5.30)
Average monthly earnings (in thousand lei)	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	6.22 (2.35 ; 13.52)	8.38 (2.29; 14.13)
Employed for at least 12 months	7.65 (2.11 ; 13.73)	7.97 (-0.20; 14.40)
Average monthly earnings (in thousand lei)	87.32 (56.99; 130.21)	43.08 (-9.48; 87.58)

²⁴ 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.

Months unemployed	-1.90	-1.82
	(-3.15 ; -0.9 2)	(-3.00 -0.54)
Months receiving UB payments	-0.74	-0.75
	(-1.18 ; -0.29)	(-1.50; -0.05)
Sample size	1,748	1,311
Size of treatment group	743	350
Size of comparison group	1,005	961

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

V.3. Heterogeneity among Individuals

So far, we have considered the average effects for the participants in the different programs. However, this average analysis does not provide any guidance on why the two ALMPs work, nor does it explore whether the impacts vary with the socio-economic characteristics of its participants. Section III provided some guidance on the different channels through which these two programs might work. In this section, we explore the compatibility of the estimated heterogeneity effects with the discussed theories.

The theoretical discussion in section III indicates that alternative theories may be consistent with differential ATT effects between different subgroups. Two of the theories—job search theory, and segmented labor markets—point to a higher effectiveness of ALMPs for the most disadvantaged workers. In the case of job search theory, workers with little access to informal search channels (such as young workers or those living in distressed areas) are more likely to benefit from PES than those with access to informal search channels. In the case of the dualistic view, unemployed workers crowded out of the primary labor market will have a comparative advantage in the secondary market if they participate in SBA. In contrast, the other two theories—human capital theory and signaling—are consistent with SBA being more effective for more qualified and experienced workers.

Although it is not possible to derive strict tests for the relative importance of these explanations, systematic heterogeneity of the effects between different groups of unemployed will provide evidence consistent with one theory but not with another. Tables 4 through 6 show subgroup impact estimates by age, type of region and education. These estimates have been estimated following the methodology described in section IV. However, we have previously stratified the sample along the dimensions age, type of region, and education, and subsequently matched within strata. Additional heterogeneity analysis by gender and prior-unemployment duration can be found in the Appendix (Tables A.5 and A.6)

Table 4 considers the effect of heterogeneity with respect to age. These estimates show that PES are more effective for younger workers than for older ones, and thus provide evidence that PES improve job matching. For instance, we find that younger PES participants 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 workers, the effectiveness of PES was more modest and not statistically significant. This finding is partly 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, we 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 consistent with the human capital and signaling view.

Table 4
Average Treatment Effects according to Age
(Percentage points except where noted)

OUTCOMES	PES vs. No participation		SBA vs. No participation	
	<36 years	>35 years	<36 years	>35 years
Current experience				
Employed or self-employed	16.89	6.73	-2.83	9.01
Employed	19.28	6.96	-1.14	5.04
Self-employed	-2.39	-0.19	0.24	2.87
Average wage (in thousand lei)	65.73	60.67	-51.40	58.01
During the two year period 2000-2001				
Employed for at least 6 months	17.78 ✓	3.96✓	9.35	8.31
Employed for at least 12 months	26.20 ✓	4.12✓	12.89	10.76
Average wage (in thousand lei)	116.62	82.81	5.11	43.27
Months unemployment	-4.62 ✓	-1.21✓	-2.50	-2.22
Months receiving UB payments	-0.66	-0.76	-0.71	-0.75
Sample size	362	1,365	273	955
Size of treatment group	159	577	97	254
Size of comparison group	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.

In Romania in the late 1990s, rural areas tended to be more economically depressed than urban areas. Therefore to test the predictions on more versus less distressed areas we compare the impact of both programs in rural versus urban areas (see Table 5). The evidence is, again, consistent with PES being more effective at improving job matching for those participants living in more distressed areas. For example, PES significantly increased average wages of participants living in rural areas by 144.24 thousand lei over the period 2000-2001, and reduced their unemployment spell by almost 5 months over the period 2000-2001 (these effects for participants living in urban areas were considerably smaller and not statistically significant).

Table 5 also shows results compatible with the segmented labor markets theory for SBA. We find that SBA is more successful for workers living in rural areas where individuals have less access to high-productivity wage and salary jobs 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 significant effects were found for SBA participants living in urban areas).

Table 5
Average Treatment Effects according to Type of Region
(Percentage points except where noted)

OUTCOMES	PES vs. No participation		SBA vs. No participation	
	Rural areas	Urban areas	Rural areas	Urban areas
Current experience				
Employed or self-employed	17.93	6.13	9.90	4.00
Employed	17.60	8.19	6.82	0.27
Self-employed	0.33	-1.65	3.30	2.31
Average wage (in thousand lei)	91.54	47.19	36.90	42.54
During the two year period 2000-2001				
Employed for at least 6 months	7.73	3.68	19.89 ✓	0.06✓
Employed for at least 12 months	17.25	5.09	19.06 ✓	5.38✓
Average wage (in thousand lei)	144.24 ✓	50.42✓	10.28	34.48
Months unemployment	-4.87 ✓	-0.96✓	-3.64 ✓	-1.20✓
Months receiving UB payments	-1.57	-0.50	-3.61 ✓	0.36✓
Sample size	454	1,177	427	774
Size of treatment group	189	531	142	210
Size of comparison group	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.

The estimates in Table 6 show ATT effects according to education level. As a measure of skill we have used whether the worker has a high-school degree or not.²⁵ The results are consistent with the segmented labor market view and contrast with the human capital and signaling hypothesis, as reflected by the findings that SBA is beneficial for workers without a high-school diploma (no significant results are obtained for high-school graduates). For instance, we find that 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%), and indicative of less skilled workers having less chances in the primary labor market.

²⁵ Unfortunately, limited sample sizes does not allow us to do the analysis for multiple education groups (such as, high-school dropouts, high-school graduates, and college graduates or more).

Table 6
Average Treatment Effects according to Education Achievement
(Percentage points except where noted)

OUTCOMES	PES vs. No participation		SBA vs. No participation	
	No HS diploma	HS diploma or more	No HS diploma	HS diploma or more
Current experience				
Employed or self-employed	5.86	11.28	5.48	5.15
Employed	8.52	11.09	3.47	0.70
Self-employed	-1.92	-0.04	1.00	3.44
Average wage (in thousand lei)	73.48	55.11	20.34	41.30
During the two year period 2000-2001				
Employed for at least 6 months	3.87	6.47	13.45	4.89
Employed for at least 12 months	5.39	9.13	19.35 ✓	1.45✓
Average wage (in thousand lei)	60.08	97.01	47.95	14.68
Months unemployment	-1.40	-1.96	-3.61 ✓	-0.57✓
Months receiving UB payments	-0.83	-0.76	-1.93	0.61
Sample size	977	725	595	687
Size of treatment group	438	296	200	150
Size of comparison group	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.

VI. Conclusion

Recent empirical evidence has found that PES and SBA are useful active labor market programs to help get the unemployed back to work. Most of these evaluation studies have focused in studying the effectiveness of these programs in developed countries, and in analyzing the average effects of these programs on the population of unemployed or on the population subgroups for which the programs have been targeted. However, relatively few studies have studied the relative effectiveness of these programs across population subgroups and the channels through which these programs operate (especially in transition economies).

Using unusually rich (for transition economies) survey data and matching methods, this paper broadens our knowledge of the effectiveness of PES and SBA in transition countries, in general, and in Romania, in particular. More specifically, this evaluation study aims to understand the suitability of these programs for different population subgroups, as well as the channels through which these programs work. The analysis reveals that average effects for the population as a whole may hide statistically and economically significant differences across subgroups. We relate these differences to the different institutional set-ups and discuss theoretical implications, which are then empirically contrasted with the heterogeneity effects.

These results suggest the following policy implications. First, we find that offering PES to unemployed workers with good access to the informal job-search channel is not a good idea. This finding is consistent with earlier findings (see Van der Berg and Van der Klaauw, 2006, among others). However, the novelty of this paper is to provide some guidance on which populations would benefit most from PES in economies with large informal job-search channels. In such countries, PES 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 distressed areas.) Another policy implication is that, in economies with segmented labor markets, offering SBA to unemployed individuals is better than offering no program since participation in SBA improves their success chances in the secondary labor market. Most importantly, this study reveals that in view of Romania's relatively difficult administrative and economic conditions, it is pertinent to conduct ALMPs on a moderate scale, targeting individuals with particular labor-market needs to job counselors that can help them access the programs most suitable to their needs. Identifying these needs requires careful analysis of the situation of individual job seekers, employers and local labor markets. More generally, in order to avoid expensive policy mistakes, the government will need to ensure that its decisions are based on a realistic analysis of the likelihood that a proposed policy could be implemented as intended. In other words, ALMPs cannot and should not be conceived as a quantitatively sufficient response to the shortage of formal-sector jobs, a problem which can only be remedied by improvements in the general conditions for business and job creation.

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APPENDIX

Table A.1.
Survey and item non-response

	Participants		Non-participants	
	Sample size	Response rate	Sample size	Response rate
Initial sample	1,934	--	2,905	--
Completed interviews	1,398	72.3 %	1,949	67.1 %
Final sample	1,109	79.3 %	1,501	77 %

Table A.2
Selected Characteristics for Completed Interviews and Final Sample, by Treatment Status
(Percentages except where noted)

Sample Characteristics	SBA		ER		Non-participants	
	(1)	(2)	(1)	(2)	(1)	(2)
Pre-program Characteristics						
Male	51.68	50.69	45.22	45.92	64.50	63.82
Education completed						
Primary school	9.62	9.97	13.04	13.25	15.15	14.86
Secondary school	31.77	32.41	46.48	45.92	44.32	44.30
High school	38.26	37.67	28.71	28.65	28.56	29.31
University	20.36	19.45	11.78	12.82	11.96	11.26
Region						
Rural	6.71	5.82	11.25	11.24	16.88	17.92
Urban with less than 20 thousand inhabitants	32.66	35.46	18.72	18.34	19.24	18.45
Urban with 20 - 79 thousand inhabitants	15.88	14.13	21.03	20.08	26.94	28.11
Urban with 80 - 199 thousand inhabitants	27.96	27.15	38.70	39.89	26.53	25.98
Urban with 200 thousand inhabitants	16.77	17.45	10.30	10.44	10.42	9.53
Judet's unemployment rate	11.41	11.37	11.79	11.86	12.97	13.12
Work experience (years)	22.83	22.99	24.03	23.99	23.68	23.63
	(7.88)	(8.04)	(8.33)	(8.28)	(8.91)	(8.91)
Not employed in 1998	20.81	23.82	21.25	22.36	17.81	19.19
Employed in 1998	79.19	76.18	78.81	77.64	82.37	80.81
Employed between 1 and 3 months in 1998	2.23	1.39	4.57	4.42	2.80	2.53
Employed between 4 and 6 months in 1998	6.04	6.37	8.50	8.70	7.22	7.40
Employed between 7 and 9 months in 1998	2.68	3.05	9.88	10.71	5.76	5.53
Employed between 9 and 12 months in 1998	68.23	65.37	55.79	53.82	66.41	65.36
1998 average monthly earnings (in thousand lei)	887.5	881.72	732.06	758.07	908.90	926.60
	(768.57)	(765.83)	(615.11)	(618.68)	(712.02)	(719.77)
Average unemployment length during 1998 (months)	2.96	3.38	3.73	3.90	2.84	2.99
	(4.72)	(4.87)	(4.66)	(4.70)	(4.49)	(4.59)
Received training during 1998	8.50	8.86	7.16	6.69	3.14	3.13
Sample size	450	362	948	747	1,949	1,501

Columns (1) are calculated with the sample of completed interviews, whereas columns (2) are calculated with the final sample. The difference in size between columns (1) and (2) are due to item non-response. Standard deviation in parenthesis for continuous variables. We could not reject the null hypothesis that each of the sample means from columns (1) and (2) were equal at the 95% confidence level.

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

	SBA	PES	Non-participants
Characteristics			
Male	50.69	45.92	63.82
Age			
Less than 31 years old	4.99	7.50	8.93
Between 31 and 35 years old	22.71	14.59	16.46
Between 36 and 45 years old	40.44	40.16	36.58
Between 45 and 50 years old	17.73	20.62	19.79
More than 50 years old	14.13	17.14	18.25
Education completed			
Primary school	9.97	13.25	14.86
Secondary school	32.41	45.92	44.30
High school	37.67	28.65	29.31
University	19.45	12.82	11.26
Family size	3.59 (1.18)	3.64 (1.28)	3.65 (1.29)
Main family earner	42.38	44.31	46.04
Region			
Rural	5.82	11.24	17.92
Urban with less than 20 thousand inhabitants	35.46	18.34	18.45
Urban with 20 - 79 thousand inhabitants	14.13	20.08	28.11
Urban with 80 - 199 thousand inhabitants	27.15	39.89	25.98
Urban with 200 thousand inhabitants	17.45	10.44	9.53
Judet's unemployment rate	11.37	11.86	13.12
Sample size	362	747	1,501

Standard deviation in parenthesis for continuous variables.

Table A.3 (Continued)
Baseline Employment Characteristics of
ALMP Participants and Non-Participants, 1998
(Percentages except where noted)

	SBA	PES	Non-participants
<i>Characteristics</i>			
Work experience (years)	22.99 (8.04)	23.99 (8.28)	23.63 (23.63)
Not employed in 1998	23.82	22.36	19.19
Employed in 1998	76.18	77.64	80.81
Employed between 1 and 3 months in 1998	1.39	4.42	2.53
Employed between 4 and 6 months in 1998	6.37	8.70	7.40
Employed between 7 and 9 months in 1998	3.05	10.71	5.53
Employed between 9 and 12 months in 1998	65.37	53.82	65.36
Not employed in 1998	23.82	22.36	19.19
Earnings per month			
Under 500 thousand lei	4.43	5.22	3.00
500 - 600 thousand lei	3.05	5.22	4.46
601 - 700 thousand lei	5.82	9.64	7.13
701 - 850 thousand lei	13.02	14.19	12.26
851 - 1,000 thousand lei	10.80	15.66	14.72
1,001 - 1,200 thousand lei	13.30	13.79	14.06
1,201 - 1,500 thousand lei	13.30	7.36	10.79
1,501 - 1,900 thousand lei	5.54	3.88	6.79
1,901 - 2,500 thousand lei	4.16	1.20	5.40
More than 2,500 thousand lei	2.77	1.47	2.20
Average monthly earnings (in thousand lei)	881.72 (39.38)	758.07 (22.51)	926.60 (17.88)
Average unemployment length during 1998 (months)	3.38 (4.87)	3.90 (4.70)	2.99 (4.59)
Unemployed at least 9 months during 1998	23.27	23.56	18.85
Received training during 1998	8.86	6.69	3.13
Average training length during 1998 (months)	0.29 (1.18)	0.26 (1.35)	0.10 (0.70)
Sample size	362	747	1,501

Standard deviation in parenthesis for continuous variables.

Table A.4
Results from the binomial probit estimations

	SBA vs. No participation	PES vs. No participation
Characteristics		
Male	-.2015284 (.0926006)	-.1427264 (.0725004)
Age	.0284343 (.1061328)	.0140676 (.0929445)
Age squared	-.0004043 (.0012505)	-.0001519 (.0010719)
Education completed		
Secondary school	.0398253 (.1420994)	.0801002 (.1099728)
High school	.3389603 (.1468737)	-.0840283 (.1175862)
University	.6136505 (.1687934)	-.0083351 (.1411292)
Persons in the household		
Three	.1021722 (.1271709)	.0232715 (.1042423)
Four	.0459635 (.1259283)	.133011 (.1018456)
>four	.0726954 (.1431552)	.0280627 (.1143186)
Respondent is the main earner	-.1547861 (.1348952)	.0962171 (.1111627)
Respondent is spouse of main earner	-.3095629 (.1379943)	-.0487241 (.1115485)
Region		
Urban <20 thousand inhabitants	.4965981 (.1689958)	-.1270346 (.1306713)
Urban (20-79 thousand inhabitants)	.2525536 (.1768784)	.2316202 (.124284)
Urban (80-199 thousand inhabitants)	.0461624 (.1719474)	.3309776 (.119047)
Urban (200 thousand inhabitants)	.7366886 (.2738287)	-.0189794 (.1976237)
Counties' unemployment rate	-.1610341 (.0342555)	.0894544 (.0627584)
Work experience (years)	.0356114 (.0539121)	.0307314 (.0490692)
Experience squared	-.0007137 (.001081)	-.0007828 (.0009607)

In addition, all regressions include *judet* dummies. Pseudo R^2 for all four specifications are presented in Table 3.

Table A.4 (Continued)
Results from the binomial probit estimations

	SBA vs. No participation	PES vs. No participation
Characteristics		
1998 employment spell		
1-3 months	-.9830641 (.499512)	-.6807008 (.3418347)
4-6 months	-.1562037 (.4336655)	-.6466339 (.3363872)
7-9 months	-.2502013 (.4274598)	-.3247323 (.3236533)
9-12 month	.9910766 (.4134734)	-.123323 (.2971646)
Average earnings per month in 1998 (in thousand lei) (wage98)	-.0000 (.0000943)	-.0001 (.0000854)
500-600	-.2457 (.2942938)	-.1813 (.2095827)
601-700	-.1330 (.249114)	-.2447 (.1841415)
701-850	-.0327 (.2145763)	-.1748 (.1698717)
851-1,000	-.2962 (.2074279)	-.2043 (.1625509)
1,001-1,200	-.3793 (.1984934)	-.1763 (.1622569)
1,201-1,500	-.1055 (.1972956)	-.3851 (.1724099)
1,501-1,900	-.3607 (.2262893)	-.4094 (.1938586)
1,901-2,500	-.3758 (.2408035)	-.9456 (.2595758)
1998 average unemployment spell (months)	.3975 (.0973285)	.5042 (.0673983)
Avg. unemployment spell squared	-.0289 (.009252)	-.0387 (.0071279)
1998 unemployed at least 9 months	.6637 (.7353178)	.2608 (.5406227)
Received training during 1998	.5994 (.5026792)	-.2614 (.42072)
1998 average training length (months)	-.0084 (.2404551)	.1144 (.1907319)
Sample size	1,326	1,775

In addition, all regressions include *judet* dummies. Pseudo R^2 for all four specifications are presented in Table 3.

Table A.5
Average Treatment Effects according to Pre-Unemployment History
(Percentage points except where noted)

OUTCOMES	PES vs. No participation		SBA vs. No participation	
	<6 months	>5 months	<6 months	>5 months
Current experience				
Employed or self-employed	12.25✓	-3.83✓	4.29	18.98
Employed	13.14	-3.49	-0.83	14.93
Self-employed	-0.52	-0.78	3.55	4.09
Average wage (in thousand lei)	102.01✓	-70.20✓	31.46	204.01
During the two year period 2000-2001				
Employed for at least 6 months	7.55✓	-5.02✓	5.64	3.15
Employed for at least 12 months	7.33	-1.15	3.65	4.35
Average wage (in thousand lei)	91.47	18.83	19.68	123.90
Months unemployment	-2.04	-0.20	-1.02	-1.55
Months receiving UB payments	-1.00	-0.21	-0.70	-0.01
Sample size	1,282	324	966	208
Size of treatment group	482	213	244	45
Size of comparison group	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.6
Average Treatment Effects according to Gender
(Percentage points except where noted)

OUTCOMES	PES vs. No participation		SBA vs. No participation	
	MALES	FEMALES	MALES	FEMALES
Current experience				
Employed or self-employed	8.95	8.24	1.18	2.83
Employed	11.45	8.20	0.21	-4.32
Self-employed	-2.32	0.15	0.18	6.01
Average wage (in thousand lei)	85.24	44.19	8.59	23.63
During the two year period 2000-2001				
Employed for at least 6 months	6.65	6.83	1.47	13.15
Employed for at least 12 months	8.18	9.64	3.68	9.04
Average wage (in thousand lei)	109.04	59.27	-21.72	46.86
Months unemployment	-2.42	-1.79	-1.03	-1.55
Months receiving UB payments	-0.33✓	-1.22 ✓	-0.68	-1.16
Sample size	901	804	790	463
Size of treatment group	338	400	181	175
Size of comparison group	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.