WHAT KIND OF TRAINING WORKS FOR THE UNEMPLOYED AND FIRST-TIME JOBSEEKERS? DIFFERENTIAL EFFECTS OF A REGIONAL PROGRAM

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ABSTRACT

The paper presents the results of an evaluation study on an occupational training program carried out in Tuscany between July 2007 and June 2008 through Measure A2 of Regional Operational Program (ROP) 2000-2006, Ob. 3, and addressed to unemployed and first-time jobseekers. This sort of evaluation of professional and vocational training has been previously performed in very few similar studies in Italy. To make it, we use matching techniques and duration models, starting from a wide set of pre-treatment characteristics, as identified in the available administrative archives or collected through *ad hoc* questionnaires. The outcome variables taken into account are the employment status and some of its features as well as the length of search times for a first job. More particularly, the effects of some alternative treatments are compared, which correspond to different typologies of courses (multiple treatments). Our findings show that the program has produced asymmetric results across the various typologies of beneficiaries and that not every kind of training has been effective.

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1. INTRODUCTION

The ex-post evaluation on the effectiveness of public programs addressed to workers, firms or territorial areas has rapidly developed in the last decades. It has thus become a vital tool for the management of programs and political interventions in the economic and social areas.

As regards the programs supporting employment, the Tuscan Regional Administration has implemented a set of Active Labour Market Policies (ALMPs) whose outcome measures require a specific effectiveness assessment in terms of the improvements that they were able to bring on the individuals' employability. In particular, among the main active policies designed and implemented during the 2000s we find: individual profiling of jobseekers (information, career guidance, and so on provided by Employment offices), vocational training interventions for (re)entering the labour market, subsidies given to firms to hire specific categories of unemployed or to change fixed-time into open-ended (permanent) contracts, and so on.

The present impact evaluation study focuses on the second typology of interventions – i.e. the training of unemployed and first-time jobseekers (FTJSs) – made by regional policymakers also thanks to the substantial support of European cofunding. In general, evaluation should be meant to determine how far a specific intervention contributed to change the pre-existing situation. In our case it should establish if and how far has training made it easier for trainees to find (re)employment. In other words, we want to establish whether the situation observed after the intervention is different from the one we would have observed in its absence. This task is made particularly difficult by the impossibility of observing the situation of non-intervention for those who have been under training: this situation can however be approximated by turning to a set of subjects who are very similar to those who benefitted from training, apart from not having benefitted from it.

In order to make a comparison of subjects whose characteristics are similar for many aspects, a questionnaire was developed and administered to all the unemployed and FTJSs who completed a training course that had started between July 2007 and June 2008, as well as to an appropriate sample of non-beneficiary respondents registered at the Employment offices by the end of 2007 as being in search for a job. The information gathered through the questionnaires added to the already available data from the administrative archives, thus making possible to make an impact evaluation consistent with the international methodological standards, whose previous applications in the field of professional and vocational training are still rare in Italy.

The paper is divided as follows. In section 2, we recall the major theoretical issues underlying training programs and summarize the main results of the international and Italian evaluation literature. In section 3, we introduce the causal model on which the methods for evaluating the microeconomic effects of public policies are based; we also briefly describe the methodologies used in the subsequent empirical analysis, such as matching in the presence of binary or multiple treatments, and some elements of survival analysis. In section 4, we present the training program under evaluation, while in section 5 we illustrate the data employed as well as some comparison statistics for the program' beneficiaries vs the non-beneficiaries. In section 6 we present the results of the evaluation and, finally, in section 7 we close the paper with some brief remarks intended to offer some hints and suggestions for the design of future policies.

2.
TRAINING PROGRAMS: JUSTIFICATIONS AND EMPIRICAL EVIDENCE

Labour market policies are traditionally separated into two categories: the so-called passive policies, aimed at mitigating the social distress caused by unemployment (unemployment subsidies, social transfers, early retirement) and the so-called active policies, which have a direct effect on individuals’ employment opportunities. The latter include typologies of interventions such as: career guidance and assistance in the job search, public employment services, training expenses, hiring incentives, job creation in the public sector.

The Italian model is traditionally centred on the hiring incentives for firms, where the latter are constituted by a rather wide and scarcely selected audience. In relatively recent times, also on the spur of the European Union’s programs, the Italian regions have made increasing recourse to forms of active policies, following the model already experimented for a long time in various North European contexts. The purpose of this section is to recall the main theoretical elements that frame the role of active policies, and thus define some key aspects that will be taken into account while evaluating our Tuscan program.

As evidenced by Calmfors (1994) and the subsequent literature, one of the main objectives of ALMPs, and particularly of training interventions, is to accelerate the matching process between demand and supply in the labour market. This can occur thanks to a series of mechanisms, such as: i) the current level of mismatch in labour submarkets reduce when the competencies and qualifications of unemployed/out-of-work individuals tend to adapt to labour demand; ii) a more active job search behaviour is promoted for the out-of-work individuals; iii) the public program that involves on-the-job training provides potential employers with more accurate information on the actual employability of individuals (screening function).

From a theoretical point of view, a clear and linear relationship seems to exist between the participation in a training course and a successful entry in the labour market. However, the literature hints at the possibility of a lock-in effect of training. In fact, on the one hand, the participation to the program might imply an undesirable decrease in the intensity of job search for the subjects under training (since they are engaged in the lessons); and in some cases, as observed by Calmfors (1994), the prospect itself of participating in a training program might make the active search more sporadic (ex-ante effect). On the other hand, the decision on the subjects’ part on whether to keep on attending the course is partly “endogenous”, i.e. it depends on the employment outcomes eventually reached already during the training.

Given these reasons, a strategy for the impact evaluation of training programs for the unemployed and FTJSs should take into account the overall employment outcomes obtained from the beginning of the program, and not only those occurring after its conclusion.

Starting from the 1990s, and more intensively in recent years, a vast empirical literature has accumulated on the microeconomic impact of ALMPs. Most of the contributions refer to countries that have long ago decided to implement extensively these typologies of interventions – the Scandinavian countries, particularly Sweden, the United Kingdom and the German-speaking countries – and have already available a well-organized surveying system for the careers of single individuals. Another group of contributions comes from the United States, while the number of Italian studies is still so small that the literature on ALMPs' impact must be seen in our country as merely at a nascent stage (Martini and Trivellato, 2011).

We will now offer a brief summary of the main results emerged in the international and Italian empirical literature as regards occupational training policies, results that are quite controversial. First of all, occupational training policies do not appear to be univocally effective: against a large number of studies underlining their positive effects, there are as many that find no effect at all (Card, Kluve and Weber, 2010; Kluve, 2010). And even when successful, occupational training – at least in the short run – does not seem to be as much effective as other forms of ALMPs, like hiring incentives or employment services, where the latter are accompanied by the enforcement of appropriate sanction mechanisms (Sianesi, 2008; Kluve, 2010). Instead, in the medium-long run, it is possible to appreciate more substantial positive effects on the beneficiaries' employability (Lalive, van Ours and Zweimüller, 2008; Card, Kluve and Weber, 2010). There are also studies that highlight how occupational training can even produce negative effects: for example, when it strengthens competencies associated to declining economic sectors (Lechner, Miquel and Wunsch, 2007), or when it is addressed to particular categories of socially-disadvantaged beneficiaries (Friedlander, Greenberg and Robins, 1997; Heckman, LaLonde and Smith, 1999).

Italian studies have mainly focussed on the evaluation of labour market policy tools other than occupational training (for a quick review of the main contributions in this field, see Martini and Trivellato, 2011). As regards the latter, among the few existing contributions worth mentioning are the following, which again do not obtain univocal results. Referring to the courses of office automation implemented in the Turin area in the mid-1990s, Battistin and Rettore (2002) implement a regression discontinuity design and find no positive results for the 17 months following the end of the course. While Bellio and Gori (2003), analyzing a sample of young people (less than 35 years old) having benefitted from training in Lombardy at the end of the 1990s, come across a positive trend in the enhancement of their employability prospects within one year from the completion of the course. Finally, Berliri, Bulgarelli and Pappalardo (2002) using 1997 data for Emilia-Romagna and Lombardy, conclude that training has a positive impact for both men and women, but more strongly for men, and individuals with an intermediate (high school degree) or low education level.

3.
DOES THE PROGRAM PRODUCE EFFECTS ON ITS BENEFICIARIES? FEASIBLE ANALYSES AND METHODOLOGICAL CHOICES

3.1 The problem of causal inference

What we would like to do now is to sketch the possible paths of analysis for ALMPs' evaluation from a methodological and operational viewpoint, making special reference to the occupational training activity carried out by the Tuscany Region. The methodology that we will adopt for evaluating these interventions takes into account: the existing regional database on the subjects who attended the courses (source: information system of the European Social Fund); some of the information made available by Employment Offices regarding the unemployed and FTJSs (source: data from the Employment offices); the information acquired through *ad hoc* questionnaires, which were administered to a sample constituted of both trained and untrained individuals. Notwithstanding that some concepts are presented in a slightly formal way, the language style we choose to treat this issue is meant to reach even readers not acquainted with statistical methods. However, the list of references below provides the essential indications needed to go further into the issue. In fact, in these last twenty years we have witnessed the development of a vast literature, which has developed synergies between methodological studies in the field of statistics and econometrics, and empirical analyses, as documented in the review by Imbens and Wooldridge (2009), which can be considered an excellent reference framework for this study.

One of the main goals of the evaluation of public interventions – or, more generally, of program evaluation – is to measure the absolute effectiveness, or impact, of interventions (also called *treatments*). Assuming that an intervention is a set of actions addressed to specific subjects with the aim of changing their condition or behaviour in a desired direction, the impact is meant as the intervention's *net contribution* to the change of such condition or behaviour. Just before providing a statistical formalization of this question, we should make a preliminary remark often left implicit in impact analyses, which concerns a fundamental assumption underlying any evaluation. This is the hypothesis on the absence of interference between/among individuals, named by Rubin (1980) Stable Unit Treatment Value Assignment (SUTVA). What is assumed, in other words, is that the intervention on a given subject does not modify the behaviour of the other subjects that are not involved. For the area of economic and social programs, this assumption leads to suppose that the intervention does not change the result for non-participants, its extent being sufficiently reduced as to leave the system's general framework unaltered. In economic terms, impact evaluation takes place in a situation of partial equilibrium, where displacement and/or substitution effects are neglected or assumed away, as they can only be examined in the broader context of a general equilibrium analysis.

To proceed with the statistical formalization, we first represent the situation under analysis with a statistical variable Y (or, if needed, with more variables), which is called outcome variable. The impact evaluation should compare the post-treatment situation of the subject (in our case, a trained unemployed or FTJS) with the hypothetical situation, usually defined *counterfactual*, that might have been observed for the same subject in the absence of treatment (i.e. in the absence of training). So, each subject is characterized by two potential outcomes (Rubin, 1974), Y_1 and Y_0, corresponding to the values that the outcome variable Y would take, respectively, in the presence (Y_1) and in the absence of treatment (Y_0): the treatment effect for each subject is therefore defined as (Y_1 - Y_0). Obviously, it is impossible to observe both variables for a single subject, a fact to which Holland (1986) refers to as the fundamental problem of causal inference. Given this unobservability, the attention then shifts to empirically estimable quantities, namely to the characteristics of the distribution of (Y_1 - Y_0) in the population under study. Usually, the aim of evaluation is expressed in terms of the expected value $E(Y_1\text{-}Y_0) = E(Y_1)\text{-}E(Y_0)$, which is known as average treatment effect (ATE). Another interesting dimension is the average treatment effect for the treated individuals (ATT), i.e. $E(Y_1\text{-}Y_0|D=1)$, where the value of D is 1 when a subject has been treated, and 0 otherwise.

The key problem of evaluation is whether it is possible to employ the information given by the treated and untreated subjects to measure the average effects. This possibility depends on the assignment mechanism used to allocate the subjects to the treated or the untreated (*control*) groups.

If we can work in an experimental setting, the allocation to the two groups is random. Randomization implies that the two potential outcomes are independent of the assignment to treatment, $Y_1,Y_0 \perp D$, which assures that there is, probabilistically, a substantial homogeneity between the two groups in terms of both the observable characteristics (e.g., age group, previous work experiences), and those which are not directly observable but are potentially relevant. Consequently, by comparing the (average) outcomes for the two groups, it is possible to obtain a correct estimate of the average causal effect.

Instead, if we work in an observational setting (that is, in case the benefit is provided to subjects upon application and/or meeting specific legal requirements, like with occupational training), the comparison between the treated and the untreated groups can lead to systematic errors. These are generically called selection biases, i.e. errors due to the individuals' (self)-selection process into treatment, whose consequence is to make the two groups potentially different even before the intervention takes place.

As a result, the comparison between the treated and the untreated groups can be carried out only where it is possible, on the one hand to make appropriate hypotheses about the treatment assignment mechanism – a point we will discuss below – and on the other when it is possible to use statistical tools that account for the differences between the two groups.

A possibility to overcome the identification problem recalled above (that is, the impossibility of observing both results on the same individual) is offered by the *unconfoundedness* assumption (Rosenbaum and Rubin, 1983), or *selection on observables*, according to which, conditional on the observable pre-treatment variables X, the treatment assignment is independent from potential outcomes: $Y_1,Y_0 \perp D|X$. This means that, although individuals with different characteristics may have a dissimilar propensity to apply for treatment, and these characteristics can be associated to potential outcomes, we assume that individuals with the same observable characteristics are randomly assigned to the treatment or non-treatment (control) groups. This is a strong and not always plausible assumption, given that some non-observable characteristics are likely to be diversely "distributed" in the two groups; however, it constitutes a good starting point for evaluation in observational settings. Obviously, the richest is the set of observable variables at hand, the more credible and sticking to reality the unconfoundedness assumption will be. This leads to several promising statistical methods, which allow for *ceteris paribus* comparisons, like the procedures of matching, stratification, weighting and regression (Rosenbaum and Rubin, 1983; Heckman, LaLonde and Smith, 1997; Dehijia and Wahba, 1999; Hirano et al., 2000).

Matching consists in coupling the result for each treated subject with that of one (or more) subjects belonging to the control group and having the same observable characteristics. Basically, we build a control group with the same distribution of observable characteristics, and this makes it possible to ascribe the (average) differences to the treatment alone.

The effect's estimate for the *i*-th treated individual is given by:

$$Y_{1i} - Y_{0(i)},$$

where (i) is an untreated individual with the vector of observable characteristics $x_{(i)}=x_i$.

Therefore, the average effect for all treated individuals can be estimated by:

$$\sum_{i=1}^{N_1} Y_{1i} - Y_{0(i)} \ ,$$

where N_1 corresponds to the number of individuals belonging to the treated group. This matching procedure is free from any parametric hypothesis and should avoid estimates of the causal effect based on the comparison between subjects that are too far apart with respect to their observable characteristics. This last, instead, is a real possibility, for example, when not enough caution is taken in the use of regression models.

When the observable variables are many, it is often impossible to make an exact matching (i.e., to find, for each treated subject, an individual in the control group that has exactly the same characteristics). Given these circumstances, the matching is made by assessing the distances between subjects using as reference point the value of observed covariates (e.g., with Mahalanobis metrics; Rubin, 1980). Thus, two subjects whose covariates' values are not exactly the same can still be considered sufficiently similar to be compared.

The measurement of distances can also be based on a model estimating the propensity to receive the treatment, conditional on these covariates. The probability of receiving the treatment $p(X)=Pr(D=1|X)$, which can be estimated from data, is called *propensity score*. Rosenbaum and Rubin (1983) have shown that if $0<P(D_i=1|X_i)<1$ for each X_i, then the unconfoundedness assumption also implies that the two potential results are independent from D, conditional on the value of the *propensity score*: $Y_1,Y_0 \perp D|X$ implies that $Y_1,Y_0 \perp D|p(X)$. Given this result, the use of the propensity score is sufficient to assure that the potential outcomes are independent of treatment assignment; in fact, it is sufficient to guarantee that the distributions of the observed covariates is the same for the treated and the untreated groups. Once estimated (by means of a logit or probit model), the propensity score can be employed with matching, stratification, weighting or regression procedures. In this work, we give preference to the matching procedures, in which the control group is constituted taking, for each treated individual, one (or more) untreated individuals with the same (or closest) propensity score value.

It is worth stressing that, no matter which is the method used to estimate the effect of treatment, an accurate analysis of the distribution of the propensity score in both the treated and the control groups should be performed in order to assess the "distance" between the two groups. Given that only a comparison between individuals with similar propensity scores makes sense, if we found, for example, no overlap in the propensity score distributions of the two groups, then the control group chosen would be invalid for comparison.

In this short methodological note, as well as in those that we will make in the following, we make reference to binary treatments, that is to situations in which attention is focussed on the effects of a specific intervention against the situation of non intervention. Frequently, and especially for training, an *active* treatment may have different levels: differences concern for example the doses of a medicine, the typologies of training, or the amount of subsidies given to firms. In the literature, various contributions have extended the concepts of unconfoundedness and propensity score, as well as the relative methodologies, to multiple treatments (Imbens, 2000; Lechner, 2001; Imai and van Dyk, 2004). However, in case the effects under examination regard individuals treated at a specific level (e.g. training type A), as compared to an alternative treatment level (training type B), such generalizations are not needed; what is necessary, instead, is to consider the two alternative treatments as a binary treatment, which is exactly what we are going to do in section 6.6.

3.2 The choice of the outcome variables and its statistical implications

As regards the evaluation of public interventions, the definition of the outcome of interest is not always easy and straightforward. In the previous sections, we have highlighted the aims of ALMPs, which in the case of training are typically concerned with an increase in the employability of the trained subjects, through their acquisition of adequate competencies and motivations. Employability is usually measured using the subsequent labour histories of both the trained and the untrained individuals. Since the labour histories of individuals occur over time, one should survey their careers and employment choices, also paying attention to the duration of each employment and unemployment spell. The tracking of labour histories is not an easy task. In fact, on the one hand, a straight request of retrospective information often gives rise to vague memories by individuals, and consequently to an improper positioning of events in time. On the other hand, the administrative archives (such as those of Employment offices) are not always fully available and do not include information on informal or “irregular” work experiences. To solve this kind of problems, and thus guarantee the quality of information, the choice of the outcome variables to analyze has finally fallen on a single event of labour histories, i.e. the first job found after the start of training, and on the present (at the time of interview) employment condition. The analysis of these two variables, including the various features of the present job position (stability, coherence, and so on) allows to evaluate the impact of training on some fundamental aspects, that is to: i) verify the existence of a negative effect for the beneficiaries, at least in the short run, due to the reduced intensity of job search during the training period. This is the so-call lock-in effect, which can be appraised by analyzing the distribution of the duration of the search for a first placement, starting from the beginning of the training program; ii) evaluate the impact of training on the current employment situation of beneficiaries (April 2011) at a certain distance of time from the training period.

Typically, in the ii) case the outcome variables will be binary and denote specific events. In this case, the estimator of the average impact on treated individuals will be the bias-adjusted matching estimator developed by Abadie and Imbens (2011), which combines Mahalanobis’ distance-based matching with a regression-based adjustment of the outcome variable in the control group, which does in fact reduce the bias deriving from that the matching is not exact for all variables. In general, the literature suggests to employ – just like we do in the present study – methods that variously combine matching and model-based techniques, because these are more robust against specification errors.

In the i) case, instead, it is not possible to use directly this estimator, because duration variables are censored. In our case, the length of search for a first placement is censored for those individuals who have not found a job yet at the date of interview. Consequently, the analysis will be carried out by combining matching techniques – aimed at building a control group whose covariates’ distribution is exactly the same as that of the trained group – with survival analysis techniques.

3.3 The duration of search for a first placement

The analysis of duration or *survival* data has received a great attention in the biometric literature (Kalbfleisch and Prentice, 1980; Cox and Oakes, 1984). More recently, survival models have also been employed in the socio-economic field, especially for the examination of the duration of (un)employment, strikes or firms' lifecycles. The variable under study in duration analysis is the time span an individual spends in a particular condition – for example, unemployment – which closes when he finds a way out of it (e.g. he finds a job), or else, in the moment or instant of time after which we cannot observe his history any further, which can be previous to the moment he finds a job, because further data are not available. In this case, we say data are right-censored, since they only offer the information that the duration of a given spell (e.g. unemployment) is longer than the time span observed. In particular, we have here a case of non-informative censoring, since the presence of censoring provides us with no additional information about duration, compared to what we already know thanks to the covariates we may use in the specification of an appropriate model.[1] We suppose that, for each individual in a population, the interval of time he spends in a certain condition up to when he leaves it can be represented by a random variable T; for the moment, we also assume that the population is homogeneous in terms of the factors that affect T's distribution. In other words, we assume that the random variables Ts, defined for each individual, are independent from each other and equally distributed, with the density function f and the cumulative distribution function F:

$$F(t)=Pr(T<=t).$$

The function

$$S(t)=1-F(t)=Pr(T> t)$$

is known as survival function, since it is used in the analysis of mortality. If T represents the age of an individual, this function represents the probability that he is still alive at t. Usually, survival analysis does not directly refer to the above-illustrated functions; instead, it moves from the examination of the so-called hazard function, which can be formalized as:

$$h(t)= \lim_{dt\to 0} Pr(t< T\leq t+dt|T > t)/dt$$

where the probability in the numerator is that of exiting the condition in the interval $(t, t+dt)$, conditionally on the fact that such exit has not occurred yet, or alternatively that the individual has "survived" up to t. From the above definition it is possible to obtain the relation that links the hazard and survival functions:

$$S(t)=exp\left(-\int_0^t h(\delta)d\delta\right).$$

[1] This work only takes into account the case of non-informative censoring; for other, more complex forms of censoring see Kalbfleisch and Prentice, 1980.

The integral defined in the previous equation is called combined or cumulative risk, and will be designated as $I(t)$.

The relation that exists between h and t is known as duration dependence: if the derivative of $h(t)$ with respect to t is greater than zero, such dependence is positive and the probability of exit grows as the situation goes on; if it is smaller than zero, then duration dependence is negative, and exiting becomes less likely over time. Duration dependence can also be analyzed by examining the combined risk function $I(t)$, which is concave if dependence is negative and convex if dependence is positive. Under the homogeneity hypothesis, that is in the absence of systematic differences among individuals, and also in the possible presence of non-informative right censoring, the empirical hazard and survival functions can be easily obtained using the Kaplan-Meier or Nelson-Aalen non-parametric estimators.

In the area of impact evaluation, the aim is to compare the survival functions of treated and untreated subjects. In our analysis we will make a comprehensive comparison, punctually for each instant t. In particular, after selecting the control group with the proper matching techniques, we will use the Kalplan-Meier estimator to obtain two average survival functions for the treated group (trained individuals) and for controls; the average will be calculated with respect to the distribution of the observable characteristics of the trained subjects.

4.
THE PROGRAM UNDER EVALUATION

4.1 A brief overview of Tuscany's active policies in the 2000s

In the context of the legislative framework in the field of education, training and employment defined by Tuscany's Regional Law 32/2002, and subsequent amendments, the Regional Operational Program (ROP) Ob. 3 has represented in the mid-2000s one of the main instruments by which the Tuscany Region has pursued the aims of the European Strategy on Employment set by the Lisbon Council in 2000 and by subsequent European councils. The goal established there was that, starting from that date, Europe had to become by 2010, "the most competitive and dynamic knowledge-based economy in the world, capable of a sustainable economic development leading to new and better jobs, and a greater social cohesion". The action of ROP Ob. 3 was detailed in a series of Axes corresponding to as many objectives of Communitarian policies, which in their turn were organized in a set of Measures (Tab. 1).

Given the final data on financial implementation of ROP Ob. 3 – Tuscany for the whole planning period 2000-2006, the total expenditure (in terms of public and private resources) was about 729 million Euros by 30 June 2009. Table 2 shows how this amount was allocated to the each Measure. About 64 thousand activities were completed, involving 610,559 subjects (Tab. 3).

Table 1
AXES AND MEASURES OF INTERVENTION FOR ROP OB. 3 2000-2006

Axis A	Measures	
Development and promotion of active labour market policies to contrast and prevent unemployment, avoid long-term unemployment to women and men, facilitate replacement of long-term unemployed and sustain the start into professional life of youth, and all other men and women entering the labour market	A1	Organization of employment facilities
	A2	Placement and replacement of youth and adults as preventive approach
Axis B		
Promotion of equal opportunities to enter the labour market, with particular attention to people at risk of social exclusion	B1	Placement and replacement of disadvantaged groups
Axis C		
Promotion and improvement of training, education and vocational guidance within a lifelong learning framework, in order to: ease and enhance the access to and integration into the labour market, improve and support employability, and promote professional mobility	C1	Upgrading of the occupational training and education systems
	C2	Prevention of school and training dropout
	C3	Upper education
	C4	Lifelong training
Axis D		
Promotion of competent, qualified and adaptable workforce, of innovation and adaptability in work organization, of development of an entrepreneurial spirit, of favourable conditions for new jobs creation, and of qualification and reinforcement of human potential in the field of research, science and technology	D1	Development of lifelong training, flexibility in the labour marker, competitiveness in public and private firms, with a priority to SMEs
	D2	Upgrading of competencies in Public Administration
	D3	Development and consolidation of entrepreneurship with priority to new employment areas
	D4	Advancement of human resources in the area of technological R&D
Axis E		
Specific measures aimed at improving female access to and participation in the labour market, including the development of careers and the opening to new job opportunities and entrepreneurship, as well as reducing vertical and horizontal gender segregation in the labour market	E1	Promotion of female participation in the labour market
Axis F		
Support to policy implementation	F1	Administrative, operating, monitoring and control costs
	F2	Other costs for technical assistance

Source: Tuscany Region, *Planning Complement Objective 3, 2000-2006*, January 2006

Table 2
FINANCIAL IMPLEMENTATION BY MEASURE, TUSCANY REGION. 30 JUNE 2009

	Total expected cost		Total expenditure	
	Absolute value (Euros)	%	Absolute value (Euros)	%
Measure A.1	16,759,743	2.4	22,682,391	3.1
Measure A.2	193,612,943	27.5	195,232,844	26.8
Measure B.1	41,329,375	5.9	40,376,799	5.5
Measure C.1	19,069,760	2.7	20,109,668	2.8
Measure C.2	34,160,701	4.8	30,111,374	4.1
Measure C.3	103,114,508	14.6	103,423,834	14.2
Measure C.4	40,208,578	5.7	38,143,473	5.2
Measure D.1	103,155,305	14.6	126,238,955	17.3
Measure D.2	15,472,038	2.2	16,745,435	2.3
Measure D.3	32,526,676	4.6	32,418,163	4.4
Measure D.4	8,001,710	1.1	7,467,585	1.0
Measure E.1	72,110,187	10.2	70,781,297	9.7
Measure F.1	14,776,238	2.1	15,468,565	2.1
Measure F.2	10,706,468	1.5	9,951,635	1.4
TOTAL	705,004,230	100.0	729,152,017	100.0

Source: Tuscany Region, ROP Tuscany, Objective 3 2000-2006, *Final Implementation Report*, 2010

Table 3
PHYSICAL INDICATORS OF IMPLEMENTATION BY MEASURE, TUSCANY REGION. 30 JUNE 2009

	Completed activities		Formats	
	Absolute value (Euros)	%	Absolute value (Euros)	%
Axis A	10,636	17	203,877	33
Axis B	1,759	3	15,198	2
Axis C	23,881	37	175,599	29
Axis D	21,420	34	163,332	27
Axis E	5,488	9	52,553	9
Axis F	752	1	0	0
TOTAL	63,936	100	610,559	100
Measure A2	10,230	16	203,432	33

Source: Tuscany Region, ROP Tuscany, Objective 3 2000-2006, *Final Implementation Report*, 2010

4.2 A focus on Measure A.2

This study particularly focuses on Measure A2, “Preventive approach in job placement and re-placement for youth and adults”, which represents a significant class of intervention in terms of both the financial resources employed and the number of beneficiaries: the corresponding expenditure is more 195 million Euros, which is 90% of the Axis A’s total, and more than one forth of total expenditure; the completed activities have been more than 10 thousand, and have involved 203,432 individuals. Table 4 briefly summarizes the main characteristics of this Measure.

Table 4
THE MAIN CHARACTERISTICS OF THE MEASURE UNDER EVALUATION

Implementing subjects	Beneficiaries	Typology of intervention	Form of supply	Implementation timetable	Part of the program evaluated in this survey
Region, local bodies, training agencies, schools, universities, single operators/professionals, firms, beneficiary subjects	Unemployed, FTJSs, subjects who maintain or suspend the unemployment condition; other employed or inactive who benefit from preventive intervention	*Support to people* (guidance, counselling and information; work experiences; training; geographical mobility incentives to individuals; hiring incentives to firms) *Assistance to organizations and systems* (training for trainers, teachers, business tutors, etc.; creation and development of networks/partnerships; economic and social studies and analyses)	Financing of corporate bodies that develop interventions and/or single beneficiary subjects (grants, vouchers, etc.)	1 January 2000-31 December 2008	Training interventions addressed to unemployed/FTJSs during the period July 2007-June 2008

Source: Tuscany Region, *Planning Complement Objective 3, 2000-2006*, January 2006

On the whole, this Measure was addressed to prevent long-term unemployment, and its priority aims were to:

- reduce for youth and adults (in particular, the over 50s) the waiting times before they enter or find their way back into the labour market, by intervening on the demand and supply sides, specifically with guided experiences in working environments, school-to-work transition, training aimed at job placement, hiring incentives, support to geographical mobility;

- offer support to individuals in completing compulsory education and in their right/duty to education within the three existing channels (school, training, apprenticeship), with particular regard to school dropouts;
- improve the effectiveness of ALMPs through personalized and integrated approaches, support and assistance, actions aimed at strengthening and developing the supply side (e.g. information, guidance and competencies balance; identification of educational requirements and training of trainers; information).

The diverse typologies of intervention provided for in this Measure have been chiefly addressed to people who were certainly looking for a job and were immediately ready to work (as from the Legislative Decree 297/2002), or were in a condition of (effective, maintained or suspended)[2] unemployment, or FTJSs. Likewise, it was also directed to other people subject to the risk of unemployment (e.g., individuals benefiting from *Cassa Integrazione Straordinaria*, i.e. who get their income from a public wage guarantee fund, individuals with apprenticeship or similar contracts) or inactive people (housewives, students, etc.), with the aim of placing them into employment.

These typologies of intervention provided both the support to individuals and the assistance to organizations and networks.

For this first area, the program could support: information, guidance, tutorship, competencies balances and counselling, delivered by a service desk and/or personalized services; work experiences for youth and adults even at European level (e.g. stages, work grants, etc.); financial subsidies to incentivize geographical mobility at national and Communitarian level, even for young people still attending compulsory education, in view of their improvement in the basic competencies (particularly, foreign languages); financial contributions to firms aimed at promoting hiring by both the transformation of fixed-time into open-ended contracts, and the permanent hiring of individuals actively looking for a job; training of various kinds (within compulsory education, actions for the area of apprenticeship, or for the professional requalification of people in search of re-employment, etc.).

In the area of the assistance to organizations and networks, the interventions were aimed at training and refreshing their operators through seminars or other formal activities, or by encouraging informal opportunities to learn.

The present study focuses on the training activities addressed to individuals that have been put in place in the final period of the Measure’s implementation; therefore, it takes into account the interventions carried out between the end of 2007 and the first half of 2008.

The training interventions for the whole programming period have involved more than 86,7 thousand students (individuals who have actually attended the course and/or intervention).[3] More than half of trainees are unemployed or FTJSs.

[2] The condition of effective unemployment concerns individuals who have lost a previous job, are immediately available to find a new position and are looking for it; the maintenance and the suspension of unemployment concern, respectively, individuals who have a job that provides them with an annual income, but not above the minimum personal income established yearly by the fiscal norms in force for dependent employment or the like, and individuals who have a fixed-term job for less than eight months, or four months for the young.

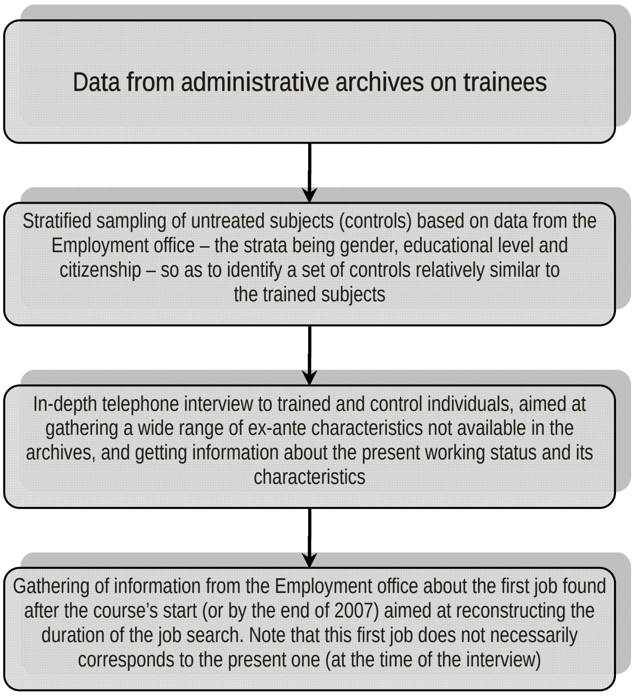
[3] Data from European Social Fund Information System of Tuscany Region (data mining dated 2 February 2011).

5.
THE DATA AND CHARACTERISTICS OF BENEFICIARIES

5.1 Data and sampling strategy

Figure 1 briefly illustrates the sampling strategy followed in this work and the main methodological choices made. First of all, we have acquired from the program managers the data about the beneficiaries of all the training interventions which took place between July 2007 and June 2008. Then we have selected the individuals who, according to administrative reporting, have attended the whole course. After that, we have extracted from the full population of people registered at the Employment offices a stratified sample of individuals who were unemployed or looking for the first job in January 2008 (that is, at an intermediate date following the start of the training courses considered in this work),[4] the strata being gender, educational level and citizenship (Italian vs foreign), which allowed to confine from the start a set of controls possibly similar to the trainees at least in terms of some very general characteristics.

Figure 1
A SKETCH OF THE SAMPLING STRATEGY



The final sample is composed of 760 treated individuals, i.e. subjects who attended a training course started in between the second part of 2007 and the first half of 2008, and 1,573 controls, i.e. subjects registered at the Tuscan Employment offices (Centri per l'Impiego – CPI) by the beginning of 2008 as being unemployed or FTJSs.

The two samples, of treated and controls, were interviewed in the course of April 2011 using the technique of Computer Assisted Telephone Interviewing (CATI),

[4] It is worth noting that the area of unemployment as defined by the Employment offices include people that may be employed but only on a short temporary basis (contracts of no more than eight months for the over 25, and four months for the under 25, or 29 if graduates), and have an annual income which is below the floor of the minimum untaxed income. For these cases, which were in fact left out from our control sample, we can talk of maintenance or suspension of unemployment.

administering them a structured questionnaire, divided into various sections meant to survey two main sets of information. The purpose was, on the one hand, to look closely at the subject's profile immediately before the course had started (or by the beginning of 2008 for the controls), in terms of the interviewees' socio-demographic variables, educational and training backgrounds, expectations and motivations about work, and previous career and labour histories (only for the unemployed). And on the other hand, to acquire information about what had happened after that date (that is, from 2008 until April 2011), in terms of working status and career potentially followed, paying a particular attention to the moment of time and conditions of the first (re-)placement and, later on, to the actual employment condition.

5.2 Some descriptive statistics

The following tables present a few descriptive statistics for some of the most significant pre-treatment variables and compare, separately for the unemployed and FTJSs, the peculiarities of the individuals who benefitted from training associated to Measure A2 (i.e., the treaded individuals) with those of untrained controls.

As regards the socio-demographic variables, the four groups do not differ much in terms of gender since in all of them about two third of interviewees are females. The beneficiaries are relatively younger, particularly in the category of FTJSs; consistently with this personal profile, a sharp majority of treated individuals is still living with the parents, especially the FTJSs (60% against 38% of controls), while the most part of controls is residing with a spouse/live-in partner. Similarly, as regards the family dependents of beneficiaries, the large majority has no children (65% of unemployed and 81% of FTJSs) differently from about half of controls (Tab. 5).

Table 5
SOCIO-DEMOGRAPHIC CHARACTERISTICS
% values (except average age)

	Interviewed (trained) beneficiaries		All interviewed (untrained) controls	
	Unemployed	FTJSs	Unemployed	FTJSs
Total interviews	485	273	914	644
Gender				
Males	31.4	33.3	45.9	33.2
Females	68.6	66.7	64.1	66.8
Average age	*35.7*	*29.4*	*38.3*	*36*
Citizenship				
Foreign	12.5	8.4	15.1	11.1
Italian	87.5	91.6	84.9	88.9
Kinship with breadwinner				
Breadwinner	27.3	15.4	33.6	12.0
Spouse/live-in partner	33.1	23.1	38.9	49.1
Son/daughter	38.6	60.4	26.8	37.7
Parents/parents in law	0.6	1.1	0.0	0.0
Other relatives/friends	0.4	0.0	0.8	1.2
Number of children				
No child	64.7	80.6	47.1	50.1
At least one child	35.3	19.4	52.9	49.9

As regards the level of education, the most part of treated individuals has a high school degree, while the share of graduates as well as that of low-educated persons is smaller. With reference to a previous experience with training courses (before 2007), the majority of all the examined groups declares he/she had never attended a training course before (83% for the case of beneficiary FTJSs); for the case of the unemployed (no matter if beneficiaries or not) the share of subjects who have already availed themselves of training interventions in the past is relatively higher (Tab. 6).

Table 6
EDUCATIONAL AND TRAINING CHARACTERISTICS
% values

	Interviewed (trained) beneficiaries		All interviewed (untrained) controls	
	Unemployed	FTJSs	Unemployed	FTJSs
Education degree				
University degree, including:	13.8	13.9	13.7	18.5
Humanities	8.4	9.9	8.0	12.6
Scientific disciplines	4.5	2.6	4.0	4.3
Health sciences	0.4	1.1	0.9	0.8
Social sciences	0.4	0.4	0.8	0.8
High school degree, including:	60.0	59.0	48.4	51.5
Lyceum	21.6	18.3	14.1	12.8
Professional/Vocational school	21.6	23.8	19.1	23.4
Technical school	16.8	16.9	15.3	15.4
Compulsory education	24.9	26.7	32.8	23.5
None	1.4	0.4	5.1	6.5
Attendance to training courses before 2007				
Yes, once	17.2	11.4	16.9	21.1
Yes, more than once	14.6	5.9	10.4	4.8
No	68.2	82.8	72.7	74.1

As regards the professional career of trainees from the end of compulsory education to 2007, we observe there is a higher share of people who have been actively looking for a job among beneficiaries, both unemployed and FTJSs; as for the latter, we observe a relevant disparity between beneficiaries and controls, 74% vs 56% (Tab. 7).

Table 7
JOB SEEKING ACTIVITY BEFORE 2007
% values

	Interviewed (trained) beneficiaries		All interviewed (untrained) controls	
	Unemployed	FTJSs	Unemployed	FTJSs
Yes	98.6	74.4	93.5	55.7
No	1.4	25.6	6.5	44.3

If we look at the labour and unemployment histories of the unemployed, there is not a significant difference between beneficiaries and controls, if not a very slight predominance of unsteady workers among the former; this is evidenced by the lower share of people who declare only one or, at the most, two previous jobs (56% of beneficiaries against 60% of controls) as well as by the higher share of fixed-term workers referring to the last position occupied (71% against 65%). Despite this, the condition of medium to long-term unemployment appears to be prevalent among controls (Tab. 8).

Table 8
CHARACTERISTICS OF LABOUR AND UNEMPLOYMENT HISTORIES BEFORE 2007
% values

	Interviewed (trained) beneficiaries	All interviewed (untrained) controls
Number of jobs held before 2007		
1	36.8	40.2
2	20.3	19.8
3	18.3	14.9
4	10.9	8.1
5	13.8	16.9
Last job held before 2007		
Open-ended (permanent) contract	25.5	28.1
Fixed-term contract	71.0	65.3
Self-employed	3.9	6.6
Length of unemployment before 2007		
More than 24 months (long-term)	16.5	20.0
From 12 to 24 months (medium-term)	8.0	9.5
Less than 12 months (short-term)	75.5	70.5

Considering the expectations and motivations about the training experience as well as the entrance into the labour market, the results of our survey evidence a stronger motivational profile among the beneficiaries, be them unemployed or FTJSs. They more frequently see the training course as useful to find a job or, at least, as an opportunity to acquire self-esteem and new knowledge. The moment they decided to participate to a training course, many beneficiaries had wished to find a job consistent with their previous educational and work careers, while the most part of controls would have been satisfied with any kind of job. Consistently with these figures, more treated individuals responded that they were well-motivated to find a full-time job, even if it involved geographical mobility or commutation (Tab. 9).

Table 9
EXPECTATIONS AND MOTIVATIONS TOWARD TRAINING AND THE LABOUR MARKET
%values

	Interviewed (trained) beneficiaries		All interviewed (untrained) controls	
	Unemployed	FTJSs	Unemployed	FTJSs
What was your attitude to training courses (multiple answer allowed):				
I had no idea they even existed	0.8	1.1	13.1	18.6
I thought they would be helpful	3.7	2.9	10.9	12.6
I believed they could help acquire self-esteem and new knowledge	77.1	77.7	59.0	36.5
I believed they could help find a job	54.4	50.2	45.4	45.8
When you decided to attend the training course (at the end of 2007, for the controls)*, your ambition was to:*				
Find any job	32.2	35.9	61.3	67.5
Find a job in line/consistent with the one you had before or your education degree	67.8	64.1	38.7	32.5
When thinking about a possible new job, at the time you would have preferred to find:				
A part-time job	28.7	30.8	33.8	49.7
A full-time job	71.3	69.2	66.2	50.3
When thinking about a possible new job, at the time, which distance from the workplace were you ready to agree to:				
Less than 30 minutes from home	38.1	44.7	52.2	53.7
Within 60-90 minutes from home	29.3	24.9	20.1	28.8
Even farther	0.6	1.1	1.6	4.9
I was even available to move elsewhere	32.0	29.3	26.1	12.6

6.
HAS THE PROGRAM BEEN EFFECTIVE?

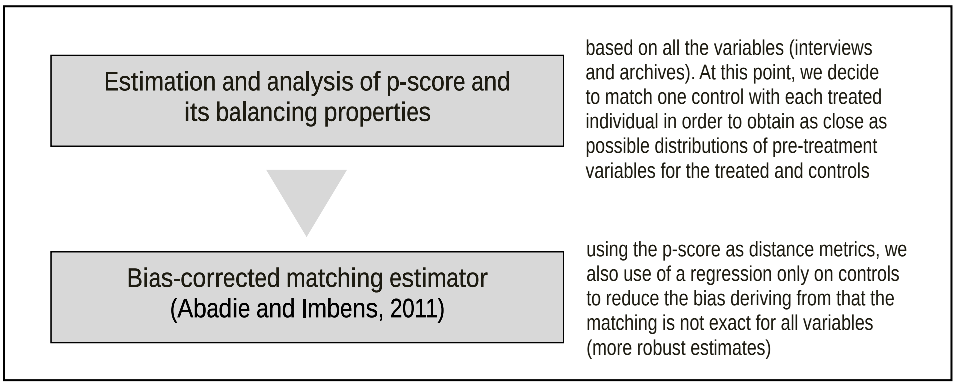
6.1 Some additional details on the implementation of an identification strategy

In Figure 2 we schematically illustrate how we have practically implemented our identification strategy based on unconfoundedness, as well as the main related methodological choices.

In order to guarantee the comparison accuracy, in other words to make sure that beneficiaries and controls were homogeneous enough to supply reliable results, we have chosen to use a matching procedure, according to which for each individual belonging to the treated group there is a corresponding equivalent (or much similar) subject among the controls. The matching procedure we have used is based on the propensity score (Rosenbaum and Rubin, 1983).

The peculiarity of this work, as compared to the existing empirical literature, is that it tries to reconstruct, for the purposes of matching, a very high number of pre-treatment covariates. To accomplish this, we have resorted to an appropriate combination of administrative sources – which in Italy are not so rich in information like in other North European countries – and information drawn directly from the questionnaire survey. A quantity of pre-treatment variables have been introduced with relation to the individual's previous educational and labour history. Consequently, the propensity score estimation could take into account both the static and dynamic characteristics of subjects. In the literature, it is generally believed that the highest is the number of pre-treatment covariates measured, the more convincing is the selection on observables assumption (unconfoundedness), not only because this option allows to compare subjects who are similar from many points of view, but also because of the greater possibility to capture, at least indirectly, the role that potentially omitted variables might have played in determining the participation in the course.

Figure 2
A SKETCH OF HOW AN IDENTIFICATION STRATEGY BASED ON UNCONFOUNDEDNESS HAS BEEN PRACTICALLY IMPLEMENTED



The complete details on the matching variables used are given in the Appendix.

From a methodological point of view, further caution had to be adopted because – in this work– the estimation of treatment effects is made in a small-sample context. This fact may generate some difficulties, after the propensity score is estimated, at the stages of balancing checks. It also calls for caution when choosing the number of controls to match against each treated individual. These aspects will be discussed in more details in section 6.2 with reference to the data under analysis.

6.2 Estimation and analysis of the propensity score

The impact analysis must be preceded by a careful examination of data that demonstrates how the estimation of the causal effects under the unconfoundedness assumption is an actual possibility.

In this respect, it is necessary to verify empirically whether the distributions in the pre-treatment characteristics of the course's participants and non participants present an overlapping area large enough to associate each treated individual with, as control, at least one untreated individual having the same distribution of pre-treatment characteristics. This is a fundamental analysis whose purpose is to verify the existence of a so-called common support.

The propensity score does in fact allow to carry out this sort of analysis. Its estimates have been obtained through a logit model. The assessment of a regression model's performance is traditionally made with respect to how it fits to data; moreover, special attention is paid to the sign and significance of coefficients. This kind of approach is not considered useful, in the methodological literature, when assessing a propensity-score model. The recommendation here is to verify instead the model's balancing capability, i.e. to check that observations with the same value of the propensity score have the same distribution of observable pre-treatment characteristics independently of the treatment status.

In the following, we will first verify the existence of a common support, and later briefly present the techniques used for the balance test.

Figures 3 and 4 graphically illustrate the presence of a common support between the beneficiary and control groups, respectively for the unemployed and the FTJSs.

Figure 3
ANALYSIS OF COMMON SUPPORT – UNEMPLOYED (RELATIVE FREQUENCIES)

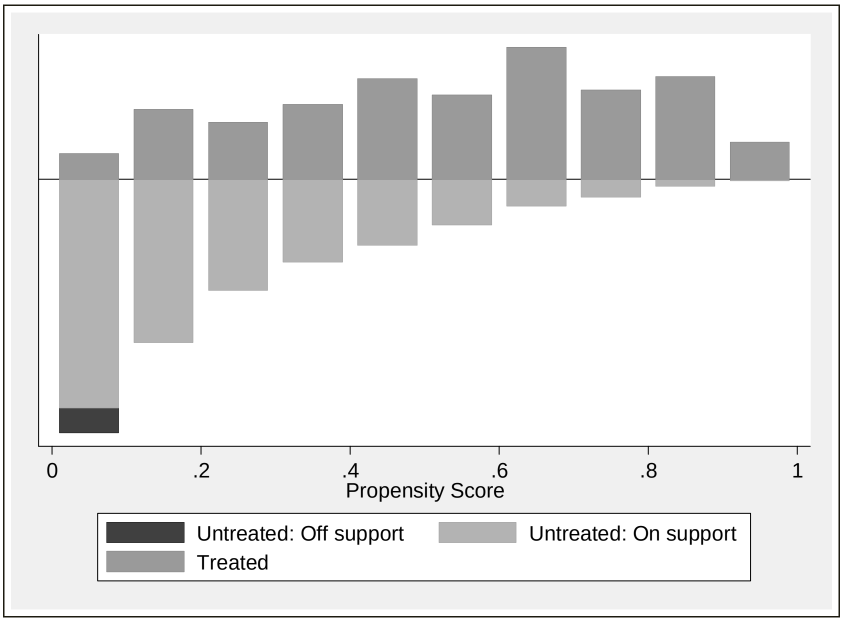
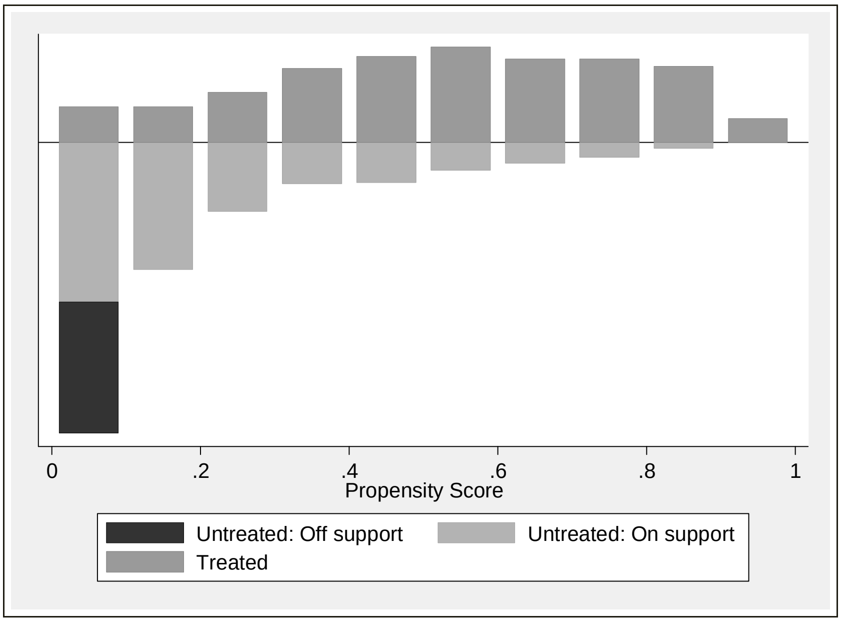


Figure 4
ANALYSIS OF COMMON SUPPORT – FTJSs (RELATIVE FREQUENCIES)



We can notice that, for both categories, the right side of the distribution presents a reduced amount of controls; we recall that this area of the distribution corresponds to higher values of the propensity score, and consequently to a higher probability of receiving treatment. Because of the scarcity of observations, we are forced to allow each control to be used more than once as a match to the treated cases; in other words, a single control will not be univocally assigned to a single treated subject (*matching with replacement*).

As already mentioned before, the propensity score obtained can be considered valid if covariate balance is ensured. Therefore, the balance test consists in verifying whether for each value, or interval, of the propensity score, the matching variables have the same distribution in the two groups. Despite the small sample size, the estimated propensity score assures that the level of balance is on the whole good, except for two "motivational" variables: for the unemployed, the aspiration to find a job consistent with previous career and/or education; for the FTJSs, the positive attitude toward training in terms of increased self-esteem and newly-acquired generic competencies.

Following the mainstream methodological literature, we have chosen to make a second balance test, which consists in measuring, for each variable: i) the standardized difference of means for the control and the treated groups; ii) the ratio between the two groups' variances. This analysis enables us to assess the balance improvements obtained through matching, and thus to choose the number of controls that should be matched to each treated subject in order to assure the highest balance improvements. As regards the choice of the number of matches, given that the literature does not provide univocal guidance, we have made some strategic choices based on empirical evidence. From a theoretical viewpoint, there is a trade off between sample variability (which is higher in the presence of many matches and is believed to bring to more precise estimates) and the bias potentially introduced by an increase in the number of matches: the higher the number of matched controls for each beneficiary subject, the stronger the risk of comparing subjects that are far apart from each other, i.e. values for the observed variables that are not sufficiently similar. As documented in Table 10, we have carefully assessed the improvements that could be obtained using different numbers of matches (e.g. 1, 2 or 3 matches). The final number of controls matched to each beneficiary is one, because this option brings to the highest average balance improvements in terms of both measures i) and ii). This means that matching did not apply to the whole sample of interviewed controls, but only to a subset of subjects more similar to the treated individuals.

Table 10
ASSESSING BALANCE IMPROVEMENTS

	Unemployed		FTJSs	
	Std mean difference	Variance ratio	Std mean difference	Variance ratio
Average improvement with 1 match	0.065	0.046	0.167	0.138
Average improvement with 2 matches	0.057	-0.011	0.159	0.131
Average improvement with 3 matches	0.059	-0.029	0.161	0.116

To estimate the effects ascribable to training, we use the bias-corrected matching estimator put forward by Abadie and Imbens (2011; see section 3.2), (Fig. 2). In deciding on which variables to apply the bias correction, we have chosen to correct the bias for the "motivational" variables, whose balance appeared to be unsatisfying. In addition, we have forced the matching to be always exact for some characteristics which have, in principle, a great influence on employability, such as gender, level of education and age group.

6.3 Evaluation questions

The strategy followed in the evaluation analysis carried out in this study has basically focused on three aspects:

1) ***Estimating the effect of the program on the current (April 2011) employment situation of trained unemployed and FTJSs***. This has been done by taking into account, after the course's conclusion, the outcome in terms of placement in a *job tout court* (i.e., any job, be it temporary or permanent), or in a *permanent job* (employment with an open-ended contract or self-employment) by using a procedure based on the propensity score and the specifications described in the previous section.
2) ***Analyzing whether the effect is heterogeneous*** across the beneficiaries' subgroups (e.g., females/males, low/highly educated subjects, etc.).
3) ***Estimating and comparing the effects of alternative types of treatment***, i.e. different kinds of training, such as short-term vs long-term training, intensive vs non-intensive training, courses focussed on general skills vs more specific ones.
4) ***Verifying the existence of a lock-in effect***, by combining matching with survival analysis techniques, so as to establish whether trained individuals have suffered from negative short-run effects due to the fact that they had to slow down their job search while under training.

The steps of the analysis described above were undertaken separately, dividing the beneficiaries of training programs into two groups with rather different socio-demographical, educational, professional and motivational characteristics – as evidenced in the descriptive statistics listed in section 5.2. These groups, in particular, constitute different targets for labour policies, since we have, on the one hand, the unemployed, i.e. subjects whose pre-treatment working status has to do with the loss of a previous job, and for whom the rationale of the active policy intervention is also precautionary, meant to avoid the constitution of an area of long-term unemployment; and, on the other hand, the FTJSs, individuals (usually, younger) who are looking for a first job.

6.4 Effects of training on the current employment situation of the unemployed and FTJSs

The results presented in Table 11 show that the effect of the program for the trained unemployed corresponds to a 10.3% higher probability (with respect to control unemployed) that, in Spring 2011, they have a job, no matter if temporary or permanent. Conversely, the program does not positively impact the probability of gaining a stable job or a job that is consistent with the previous educational and work careers.

Table 11
AVERAGE TREATMENT EFFECTS ON THE TREATED (ATTs) FOR THE PARTICIPANTS IN TRAINING COURSES
% values

		All controls	Only matched controls	Beneficiaries	ATT	p-value
Unemployed (485 under training)	Rate of employment in any job	38.6	41.9	52.2	10.3	0.038
	Rate of employment in a permanent job	17.5	21.6	20.4	-1.2	0.776
FTJSs (273 under training)	Rate of employment in any job	20.0	27.0	46.5	19.6	0.002
	Rate of employment in a permanent job	7.6	9.6	21.2	11.7	0.023

Statistically significant ATTs are shown in bold

As concerns FTJSs, instead, not only is there a more relevant impact on the probability of being presently employed – which is 20% higher for trained individuals – but also a positive, significant effect in the probability of obtaining a permanent placement (11.7%). As can be observed in Table 11, ATT corresponds to the difference between the employment rates of beneficiaries and matched controls, i.e. the subjects who are more similar to the treated ones. This last measure is the so-called *deadweight*, which represents the employment rate the same beneficiaries would have obtained without having being trained. If we compared the employment rate of treated individuals with that of all possible controls, the effect would surely be wider, but not fully ascribable to the training intervention, being partly associated with the subjects' (self-)selection process.

6.5 Has the program been equally effective across all types of beneficiaries?

We will now verify whether the training intervention gives more or less advantages to certain categories of beneficiaries (heterogeneity of effects). Generally, with respect to the goal of a job whatever its quality, the most important effects can be found among males, for individuals with a low level of education or for those belonging to "marginal" age groups typically characterized by modest levels of employability (Tab. 12).

In fact, the effect for the unemployed is 13.3% for trained males, while it is not significant for females. Also, positive effects can be found only for subjects who have merely fulfilled compulsory school (20.5%), and not for more educated individuals. In addition, training only improves the employability of the unemployed aged more than 30 years, and more specifically over 45 (23.69%), but not in the lower age group (up to

30). Finally, the effects of training are higher among the long-term (21.25%) compared to the short-term unemployed (10.89%).[5]

Table 12
HETEROGENEITY OF EFFECTS ACROSS SOME DIFFERENT TYPES OF TRAINEES

		Observations under training		Any job		Permanent job	
		Unemployed	FTJSs	Unemployed	FTJSs	Unemployed	FTJSs
				ATT (p-value)	ATT (p-value)	ATT (p-value)	ATT (p-value)
Gender	Males	153	91	13.30%	28.40%	-2.40%	-0.70%
				(0.072)	(0.002)	(0.718)	(0.885)
	Females	332	182	9.10%	15.90%	11.10%	12.40%
				(0.156)	(0.055)	(0.166)	(0.056)
Education degree	Compulsory education	127	74	20.50%	30.60%	3.10%	8.40%
				(0.032)	(0.028)	(0.703)	(0.491)
	High school degree	292	161	5.40%	14.40%	-2.40%	12.80%
				(0.408)	(0.064)	(0.637)	(0.04)
	University degree	66	38	9.40%	21.40%	-6.40%	16.30%
				(*0.429*)	(0.137)	(0.609)	(0.103)
Age group	Up to 30 years	185		1.73%	–	-12.04%	–
				(0.844)		(0.116)	
	31-45 years	227		12.97%	–	6.92%	–
				(0.054)		(0.213)	
	Over 45 years	73		23.69%	–	1.02%	–
				(0.052)		(0.906)	
	Up to 19 years		72	–	25.82%	–	18.13%
					(0.067)		(0.105)
	20-30 years		108	–	14.11%	–	6.67%
					(0.122)		(0.347)
	Over 30 years		93	–	18.66%	–	13.61%
					(0.062)		(0.118)
Length of unemployment	Short	405		10.89%	–	-7.46%	–
				(0.049)		(0.635)	
	Long	80		21.25%	–	12.50%	–
				(0.022)		(0.074)	

Statistically significant ATTs are shown in bold

Although training, on average, does not produce appreciable effects on the unemployed in terms of finding a permanent job (cf. section 6.4), we may observe that the only category in which a positive impact of this kind can be found is that of long-term unemployed (12.5%). The abovementioned evidence suggests that training interventions are moderately effective in helping work replacement, though often in precarious jobs, and that they seem to work for specific typologies of “marginal” subjects.

On the other hand, as to the FTJSs, training interventions produce less heterogeneous results. Again, the impact is stronger for males (28.4%), although there is also an increase in females’ employability (16%). It is also worth noting that training favours people who have fulfilled not only compulsory school (30.6%), but also – though moderately –those who have completed high school (14.4%). Again, marginal age groups take more advantages: on the one hand, the teenagers (25.82%), who find the opportunity to raise and professionalize their basic competencies, and on the other the over 30s (18.66%), who approach the labour market rather late in their lives.

[5] In this work, we arbitrarily choose to label an individual as being long-term unemployed when he/she has been without a job for more than 12 months.

As compared to the more desirable goal of finding a permanent job, we have seen that the training intervention leads to positive result, on average, only for FTJSs (section 6.4). Among them, women rather than men show an increased (permanent) employability (12.4%). Also, training enhances the employability of high-school graduates (12.4%), but not more than what it does for compulsory school or university graduates.

6.6 Which kinds of training work?

So far, we have looked at training as if it was uniformly delivered to the unemployed and FTJSs, and evaluated its effectiveness as compared to non-trained individuals, all other characteristics being equal (*ceteris paribus*). Actually, training courses are in fact not all the same and thus we might expect some of them to be more successful than others in promoting job (re)placement. Training may be different, for instance, in terms of length (long/short) or intensity (intensive/non intensive). Again, training may reinforce general competencies, or instead it may be aimed at conveying specific, occupation-related competencies. In addition, the comparative evaluation of these factors offers the opportunity to separate, among the interventions under examination, those that are more expected to bring job positions that are consistent with the course's contents (e.g., professionally-oriented courses), and those that are not necessarily expected to do so (e.g., generalist courses).[6]

From a methodological viewpoint, the comparison of alternative types of courses requires that the subjects treated in each typology are compared with both the non-intervention situation and all the other forms of treatment. Both comparisons always take place *ceteris paribus*.

The comparison with the non-intervention situation is similar to that already illustrated in the previous sections: the impact of training is identified by matching each treated subject (now in training type A) with one or more subjects who are similar under a set of observable characteristics but did not participate in training. At this stage, what is investigated is the effect of having attended, for example, a hairdresser course with respect to a set of untrained individuals with the characteristics of those who enrol in a hairdresser course.

The comparison of alternative forms of treatment regards instead only treated individuals. In this case, what we compare is the effect of having attended a hairdresser course, instead of a foreign language course, for the individuals having the characteristic of those who enrol in a hairdresser course. And vice versa. This second kind of cross-comparison accounts for the fact that participants in different courses may also differ as regards their personal features. Consequently, they would not be comparable unless that rationale of *ceteris paribus* comparison subsists. Where the personal features of the participants in alternative types of course are much far apart (e.g., course A is attended only by women, course B only by men, and so on), this *ceteris paribus* comparison is made impossible by the absence of a common support. Whereas, if the personal features

[6] Any evaluation of the consistency of the course contents with the job obtained later is ultimately ambiguous. Most studies made on this point do in fact rely on the personal evaluation given by the subjects, who are asked to declare whether the job they found is consistent with the course they had attended. This approach has obvious limits: two individuals who followed the same course and found the same job may very well give very different evaluations of consistency. To make an example, a course on basic computer skills followed by two subjects who later find a job as vendors might be judged coherent by one because it turned useful for the electronic management of orders. This approach is also clearly incompatible with the strictness and objectivity of counterfactual evaluations, since a consistency judgment is surely not well-defined nor can it be asked to uneducated individuals.

of the participants in alternative courses are not completely different, they can be performed limited to the subjects in the two groups who are most similar to each other.[7]

Using all the information on the courses available from the administrative archives, in this study we compare the effectiveness of:

- *long vs short courses*. The course is labelled as long when it lasts more than the median length of all the courses;
- *intensive vs non-intensive courses*. The course is labelled as intensive if the required daily attendance (hours) is above the median for all the courses;
- *four types of courses identified on the basis of their goals and characteristics*. These are: i) courses aimed at training professional workers in the manufacturing (blue collars), sales and tourism industries; ii) courses aimed at shaping professionals in personal care services (e.g. hairdresser, caregivers, etc.); iii) orientation courses and courses aimed at improving general skills; iv) back office and office automation courses.

We will first analyze the effectiveness of the different types of courses as compared to the situation of absence of training (Tab. 13), and then show the most relevant results obtained from comparing the types of courses pairwise.

Table 13
AVERAGE EFFECTS ON PARTICIPANTS IN DIFFERENT TYPES OF TRAINING COURSES (ATTs) COMPARED TO THE SITUATION OF ABSENCE OF TRAINING

		Observations under training		Unemployed, any job		Unemployed, permanent job		FTJSs, any job		FTJSs, permanent job	
		Unemployed	FTJS	Employment rate	ATT (p-value)	Employment rate	ATT (p-value)	Employment rate	ATT (p-value)	Employment rate	ATT (p-value)
Length of course	Long course	123	84	47.15	7.40% (0.308)	16.3%	-7.46% (0.22)	57.14	**27.23%** (0.009)	27.4	**24.17%** (0.002)
	Short course	362	189	53.87	**11.31%** (0.033)	21.8%	1.07% (0.809)	41.80	**13.78%** (0.036)	18.5	5.78% (0.285)
Hour load per day	Intensive course	254	132	49.21	**10.39%** (0.090)	18.9%	-3.56% (0.482)	53.79	**25.34%** (0.004)	26.5	**21.78%** (0.001)
	Non-intensive course	231	141	55.41	9.65% (0.062)	22.1%	-2.38% (0.632)	39.72	10.31% (0.124)	16.3	2.25% (0.694)
Course contents	Specializations in industry, sales and tourism sectors	176	91	53.41	**14.94%** (0.035)	19.3%	-2.02% (0.731)	43.96	**17.12%** (0.076)	20.9	**11.35%** (0.108)
	Personal care services	72	52	57.33	**24.67%** (0.044)	25.3%	8.67% (0.403)	64.29	**30.95%** (0.030)	31.0	**23.81%** (0.043)
	Orientation and general skills	75	42	43.06	9.38% (0.289)	19.4%	3.52% (0.594)	36.54	1.96% (0.867)	17.3	5.88% (0.484)
	Back office and office automation	52	19	44.23	12.77% (0.282)	13.5%	2.00% (0.81)	52.63	16.67% (0.188)	26.3	**26.32%** (0.017)

Statistically significant ATTs are shown in bold

[7] Even so, it is still likely that the effects of course A compared to those of course B, and vice versa, are not perfectly symmetrical. In fact, they would be symmetrical only if both courses were attended by the same typology of trainees, or if the effects were constant, i.e. not dependent on the subjects' characteristics. However, this is not always the case: suppose, for example, that hairdresser courses are predominantly attended by women and blue collar courses by men. In cases like these, even if a common support may exist, the cross-comparison could lead to asymmetrical results.

With reference to the outcome of finding any kind of job, only short courses are effective for the unemployed (11.31%), while both long and short courses are effective for FTJSs, although a longer training better assures the probability of employment (27.23% vs 13.78%), and is the only form that helps finding a stable position (24.17%).

Accordingly, intensive courses are the only ones that work for FTJSs, in terms of both finding any kind of job (25.34%), and a permanent job (21.78%), while both the intensive and non-intensive courses are similarly effective for the unemployed, but only in terms of finding any kind of job (10.39% and 9.65% respectively), but not a permanent one.

These findings suggest that the replacement of unemployed workers should be pursued, maybe exclusively, by means of short courses, and that these latter should preferably be intensive. Therefore, attendance of unemployed individuals to long courses should be discouraged. In contrast, FTJSs should be directed to attend long, intensive courses, which are more likely to provide subjects with no professional qualification with that stock of competencies necessary to enter the job market.

We will now compare the effects of the four different typologies of courses identified on the basis of the goals and characteristics of the training course itself.

With regard to the unemployed and the result of obtaining any kind of job, only a few types of course are effective, particularly the ones addressed to shape professionals specialized in the sectors of personal care (24.67%) and blue collars, and sales and tourism professionals (14.94%). Instead, the courses in office administration, or the more generalist orientation and general skills courses are not effective.

Partly similar considerations can be extended to FTJSs. Even in their case, the most effective courses in terms of job placement are those in the area of personal care (30.95%) and, at quite a distance, courses shaping blue collars, and sales and tourism professionals (17,12%). Considering the attainment of a permanent job, however, it emerges that not only courses on personal care services are effective (23.81%), but also those for blue collars, and sales and tourism professionals (11.35%),[8] as well as office administration courses (26.32%).

The cross-comparison of the various types of course is not always possible (given the lack of common support), since the differences among their participants are too marked. In addition, even where the comparison is technically possible, now and then the effects are not statistically significant because of the very small size of the sample. Where a statistically significant effect may be appreciated, it generally confirms the effectiveness ranking we have already pointed at.

Particularly for the unemployed, it should be stressed that they are so heterogeneous as to preclude in many cases to make cross-comparisons per typology of course (lack of common support) and that, even when this comparison is technically feasible, it brings to results that are always below the threshold of statistical significance.

Our findings are particularly interesting with respect to FTJSs (Tab. 14), given that this category of participants is intrinsically more homogeneous compared to that of the unemployed.

[8] The p-value associated with the ATT of courses meant for blue collars, sales and tourism professionals is slightly below the commonly accepted level of statistical significance. Notwithstanding this, we still think it is worth to highlight this point since the p-value is strongly affected by the small size of the sample on which the estimation was made and, at the same time, the result is extremely consistent with the others obtained for this typology of course.

Table 14
AVERAGE EFFECTS ON PARTICIPANTS IN DIFFERENT TYPES OF TRAINING COURSES (ATTs) AS COMPARED TO AN ALTERNATIVE TREATMENT SITUATION (p-VALUE INTO BRACKETS)

		Length of course		Average daily hours		Course contents			
		to long	to short	to intensive	to non intensive	to blue collars, and sales and tourism professionals	to personal care services	to orientation and general skills	to back office and office automation
ANY JOB									
Length of course	from long course	–	-12.90% (0.22)	–	–	–	–	–	–
	from short course	13.25% (0.18)	–	–	–	–	–	–	–
Average daily hours	from intensive course	–	–	–	-18.80% (0.035)	–	–	–	–
	from non-intensive course	–	–	6.60% (0.525)	–	–	–	–	–
Course contents	from blue collars, and sales and tourism professionals	–	–	–	–	–	36.37% (0.036)	-15.69% (0.219)	5.26% (0.779)
	from personal care services	–	–	–	–	-32.12% (0.018)	–	-36.17% (0.056)	no c.s.
	from orientation and general skills	–	–	–	–	5.62% (0.667)	35.71% (0.04)	–	no c.s.
	from back office and office automation	–	–	–	–	5.17% (0.799)	no c.s.	no c.s.	–
PERMANENT JOB									
Length of course	from long course	–	-13.98% (0.127)	–	–	–	–	–	–
	from short course	16.87% (0.049)	–	–	–	–	–	–	–
Hour load per day	from intensive course	–	–	–	-16.91% (0.03)	–	–	–	–
	from non-intensive course	–	–	1.17% (0.894)	–	–	–	–	–
Course contents	from blue collars, and sales and tourism professionals	–	–	–	–	–	23.75% (0.086)	-13.73% (0.221)	5.26% (0.75)
	from personal care services	–	–	–	–	-21.73% (0.083)	–	-17.02% (0.248)	no c.s.
	from orientation and general skills	–	–	–	–	5.62% (0.555)	21.43% (0.086)	–	no c.s.
	from back office and office automation	–	–	–	–	5.17% (0.745)	no c.s.	no c.s.	–

Statistically significant ATTs are shown in bold. “No c.s.” indicates the absence of common support

If we moved an individual enrolled in an intensive course to a non-intensive one, we would have a -18.8% decrease in the probability for this subject of having found today any kind of job; in addition, his/her probability of having found a permanent job today would be 16,91 points lower.

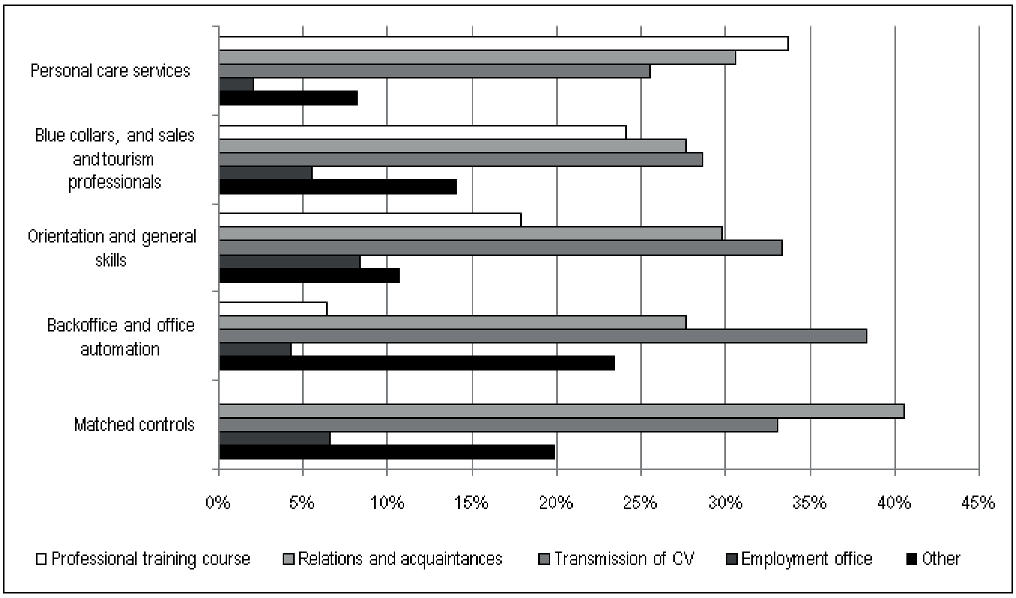
And again, if we moved a FTJS from a short to a long course, we would have a 17 points increase in his/her probability of being permanently employed.

By a similar reasoning, we observe that the courses for the personal care sector are always more effective than the others in terms of finding both any kind of job and a permanent job. Generalist courses, instead, are the least effective.

From the whole set of factors discussed above, what comes to light is that for both the unemployed and FTJSs attendance to courses with a strong professional orientation should be encouraged, while attendance to generalist courses should be discouraged.

It has finally to be noted that training often acts as a go-between in the search of a job (Fig. 5). This is very often the case for courses in personal care services, in which a 34% of trained individuals presently employed have in fact found a job thanks to it, as well as for courses for blue collars, and sales and tourism professionals (24%). In both cases, it should be underlined that personal channels have a strong relevance, while the role of Employment offices is rather negligible. This intermediation role is instead infrequent for courses in office administration and general skills. Even for these cases, just like for that of the matched subjects (controls) who did not participate in training, Employment offices have only a marginal role.

Figure 5
CHANNELS USED BY TRAINED INDIVIDUALS AND MATCHED CONTROLS TO FIND A JOB, PER TYPE OF COURSE ATTENDED (NON PARTICIPATION IN TRAINING FOR CONTROLS)*



* The channels are those mentioned in the interviews

In brief, professional training seems not only to promise higher effects, but it also proves to be the place where the benefits associated to a specific profile of competencies more easily combine with support and assistance in the job searching activity.

6.7 The duration of first-placement search

In sections 6.4, 6.5 and 6.6, we have evaluated the effectiveness of training with respect to the present working status of subjects. It is worth noting that this situation does not necessarily correspond with the first placement the subject has found following the training intervention. In fact, the case might very well be that i) he is presently still employed with the job he has first found, but also that: ii) he has worked for a short time but he has later re-entered unemployment, or iii) he has held different jobs in this period, so his first job does not correspond to the present one. Since we cannot avail ourselves of the necessary information about all the events in his more recent career, we focus on the duration of the first placement search starting from the beginning of the course (from the beginning of 2008 for controls).

Has training led to a lock-in effect that inhibited active search?

The results of the analysis based on the Kaplan-Meier non-parametric estimator do not bring to light any significant lock-in effect for the unemployed while under training. This is shown in Table 15, where we represent the shares of subjects who, at specific timepoints after the beginning of training, are still in (that is, have not exited from) an unemployment condition. The values reported in the table are drawn from the survivor functions estimated in the groups of treated and matched controls. These functions do not differ from each other for several months, as suggested not only by the proximity of estimated values, but also by the overlap of confidence intervals. After about 11-12 months the probability of exiting unemployment tends to increase more rapidly for the treated rather than for the untreated unemployed.

The absence of lock-in during the initial stage can be interpreted in terms of the characteristics of the training courses, which do not represent a serious hindrance to active job search.

Table 15
SURVIVOR FUNCTIONS OF THE UNEMPLOYED AT SOME SPECIFIC TIMEPOINTS

	Matched controls			Under treatment		
	Survivor function	Confidence interval		Survivor function	Confidence interval	
Time (days)		Lower	Upper		Lower	Upper
30	0.975	0.968	0.981	0.996	0.983	0.999
60	0.975	0.968	0.981	0.979	0.961	0.989
90	0.974	0.966	0.980	0.975	0.956	0.986
180	0.946	0.935	0.955	0.936	0.910	0.955
270	0.919	0.906	0.930	0.886	0.853	0.911
360	0.913	0.899	0.924	0.841	0.805	0.871
540	0.889	0.875	0.902	0.761	0.720	0.797
600	0.889	0.875	0.902	0.735	0.693	0.773

Similar is the case of FTJSs (Tab. 16), for whom we find no evidence of lock-in. Here, the probability of finding a first placement starts to increase just after 4 months for treated individuals.

Table 16
SURVIVOR FUNCTIONS OF FTJSs AT SOME SPECIFIC TIMEPOINTS

	Matched controls			Under treatment		
	Survivor function	Confidence interval		Survivor function	Confidence interval	
Time (days)		Lower	Upper		Lower	Upper
30	1.000	.	.	0.996	0.974	1.000
60	1.000	.	.	0.993	0.971	0.998
90	0.996	0.989	0.998	0.978	0.951	0.990
180	0.986	0.977	0.991	0.937	0.900	0.960
270	0.951	0.937	0.963	0.889	0.844	0.921
360	0.929	0.913	0.943	0.829	0.778	0.869
540	0.880	0.860	0.898	0.762	0.707	0.809
600	0.880	0.860	0.898	0.740	0.683	0.788

7. CONCLUDING REMARKS

In this study, we have evaluated the training interventions carried out in Tuscany through Measure A2 of ROP Ob. 2 2000-2006, addressed to unemployed and FTJSs, and started in between July 2007 and June 2008. The study focuses on the training courses that have been delivered to a set of 760 unemployed and first-time jobseekers (FTJSs) between July 2007 and June 2008. Using a combination of econometric techniques, the employment outcomes achieved by these latter are compared to a control group of 1,573 very similar individuals that did not participate in any training in the same span of time. In order to collect a vast array of data, both beneficiaries and controls have been interviewed about their educational and professional profile, their labour histories and their current professional status. Survey data have been later matched with additional administrative data related to courses and post-training careers. So as to perform a rigorous comparison between trained and non-trained individuals, we have defined a wide set of variables to be used in the implementation of an evaluation strategy based on propensity-score matching. We believe that the use of such a wide set of pre-treatment variables makes the "unconfoundedness" assumption – on which matching methods rely – highly credible.

The main evaluation questions that we have addressed are: i) Has the training increased the probability of (re-)employment? And, if yes, has it increased the probability to find a permanent job or instead only a temporary one?; ii) Has the program been equally effective across all types of beneficiaries, or instead only for some of them?; iii) Which types of training work best, and which ones do not work at all?; and also iv) Have participants been locked-in in the training activity, thus delaying their active search for a job?

To respond to these questions, the outcome variables that we have considered refer to the employment status observed 22-36 months after the inception of training, as well as to the duration of the search for a first placement.

We find that the training for the unemployed results in a 10% increase – on average – of the probability of re-employment, although it does not lead to permanent jobs. The average effect on trained FTJSs is higher: 12% with respect to a permanent job, 20% with respect to any kind of job (be it permanent or, more likely, temporary). These effects vary significantly depending on the characteristics of trained individuals. Among FTJSs, the effects are relevant for females and individuals with a high school diploma; while among the unemployed they are mostly relevant for males and individuals with primary education. In both cases, positive effects may be appreciated in the presence of vulnerable targets, such as the elderly unemployed, teenagers seeking the first job, and late entrants into the labour market.

Our findings also suggest that the training has not generated an undesirable lock-in effect, as it has not significantly prevented participants from actively searching for jobs.

Finally, we find that the characteristics of training matter a lot. In particular, the re-employment of the unemployed should be exclusively pursued by means of short – and preferably intensive – training. The participation of the unemployed in long-lasting courses should instead be discouraged. In contrast, our findings suggest that FTJSs should be directed towards long-lasting and intensive training, as this latter is more likely to raise competencies from scratch and shape adequate professional profiles. Furthermore, both the unemployed and FTJSs should be encouraged to participate in training that focuses on specific and complex professional competencies, while training that provides basic skills (e.g. foreign languages, computer skills), general-purpose knowledge or counselling is not effective.

Drawing on these results, we argue that policymakers should be more selective when choosing which training has to be financed in order to tackle labour market inactivity and unemployment. On the one hand, it seems urgent to revise the accreditation criteria for training suppliers, in order to reduce their number and to reward quality and specialization. On the other hand, a greater degree of targeting is desirable: starting from the policy design stage up to its implementation, individuals should be strongly oriented to types of training that not only correspond to their actual needs, but are also more likely to lead to placement opportunities.

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APPENDIX
List of matching variables used in the estimation of the propensity score

Variable	Characteristic of the variable (observed/estimated)	Source	Meaning/description
Socio-demographic status			
gender	Dichotomous (obs.)	Archives	Male; female
age	Continuous (obs.)	Archives	Age of individual one year before the enrolment to the course
citizenship	Dichotomous (obs.)	Archives	Italian; foreign
Household characteristics and position of the interviewee within the household			
members_n	Continuous (obs.)	Interview	No. of members of the household
breadwinner	Dichotomous (obs.)	Interview	The subject is the main income recipient in the family
child	Dichotomous (obs.)	Interview	The subject is the breadwinner's child
spouse	Dichotomous (obs.)	Interview	The subject is the breadwinner's spouse
(baseline: parents and/or other relation)		Interview	The subject is a breadwinner's parent
income recipients	Continuous (obs.)	Interview	No. of subjects with an income in the household, including the interviewee
num_children	Continuous (obs.)	Interview	No. of dependent children under the age of 18
num_children	Continuous (obs.)	Interview	No. of dependent children under the age of 3
no child	Dichotomous (obs.)	Interview	With no children
own_house	Categorical (obs.)	Interview	Family-owned house/flat; rented house/flat; social housing
(baseline: free of charge)		Interview	He/she lives in a house free of charge
father_h.s. degree	Dichotomous (obs.)	Interview	The father has a high school degree
father_univ. degree	Dichotomous (obs.)	Interview	The father has a University degree
(baseline: father other degree)		Interview	The father has completed compulsory education
mother_h.s. degree	Dichotomous (obs.)	Interview	The mother has a high school degree
mother_univ. degree	Dichotomous (obs.)	Interview	The mother has a University degree
(baseline: mother other degree)	Dichotomous (obs.)	Interview	The mother has completed compulsory education
Educational and training history			
5univ.degree_scien	Dichotomous (obs.)	Interview	Scientific university degree (5 years);
5univ.degree_soc	Dichotomous (obs.)	Interview	Social sciences university degree (5 years)
5univ.degree_health	Dichotomous (obs.)	Interview	Health sciences university degree (5 years)
5univ.degree_human	Dichotomous (obs.)	Interview	Humanities university degree (5 years)
3univ.degree_scien	Dichotomous (obs.)	Interview	Scientific university degree (3 years)
3univ.degree_soc	Dichotomous (obs.)	Interview	Social sciences university degree (3 years)
3univ.degree_health	Dichotomous (obs.)	Interview	Health sciences university degree (3 years)
3univ.degree_human	Dichotomous (obs.)	Interview	Humanities university degree (3 years)
Lyceum	Dichotomous (obs.)	Interview	Lyceum high school degree
technical	Dichotomous (obs.)	Interview	Technical high school degree
profess	Dichotomous (obs.)	Interview	Professional/vocational high school degree
junior	Dichotomous (obs.)	Interview	Junior high school degree (part of compulsory education, but used not to be so for older generations)
(baseline: primary school or none)		Interview	Primary school degree (part of compulsory education)
time_distance	Continuous (obs.)	Interview	No. of years past from the achievement of last degree until training starts
years to dropout	Continuous (obs.)	Interview	No. of years from last school degree and dropping out of school
previous_training	Dichotomous (obs.)	Interview	Participation to other training courses

Past labour history and characteristics of the last job (unemployed only)			
search	Dichotomous (obs.)	Interview	He/she has made job seeking actions
job_num	Continuous (obs.)	Interview	No. of jobs held
job_months	Continuous (obs.)	Interview	No. months worked
open-ended	Dichotomous (obs.)	Interview	Last job: open-ended employment
fixed-term	Dichotomous (obs.)	Interview	Last job: fixed-term employment
p_high	Dichotomous (obs.)	Interview	Last job: Managers and entrepreneurs, intellectual, scientific and highly-specialized professions
technical	Dichotomous (obs.)	Interview	Last job: Technical professions (e.g. head nurses, bookkeepers, insurance agents, sale agents, teachers, social workers, accountants)
clerk	Dichotomous (obs.)	Interview	Last job: White collars (e.g. secretaries, cashiers, operators)
skilled_workers	Dichotomous (obs.)	Interview	Last job: Artisans, specialized workers and farmers (e.g. masons, plumbers, electricians, mechanics, carpenters)
semiskilled _workers	Dichotomous (obs.)	Interview	Last job: Plant operators and semiskilled workers attending to fixed and movable machinery
unskilled_workers	Dichotomous (obs.)	Interview	Last job: Non qualified professions
(baseline: other)		Interview	Last job: Other profession
last_sect	Dummy vector	Interview	Sector of activity of last job
expired	Dichotomous (obs.)	Interview	Last job ended because he/she had a fixed-term contract
resigned	Dichotomous (obs.)	Interview	Last job ended because he/she resigned
closed_firm	Dichotomous (obs.)	Interview	Last job ended because the firm closed
fired	Dichotomous (obs.)	Interview	Last job ended because he/she was fired, and the firm is still into business
(baseline: ceased self-employment)		Interview	He/she gave up self-employment
duration_unempl	Continuous (obs.)	Interview	No. of months from last job to training
benefits	Dichotomous (obs.)	Interview	He/she was recipient of unemployment benefits
refusal	Dichotomous (obs.)	Interview	He/she has declined at least a job offer
Expectations prior to training			
consistent	Dichotomous (obs.)	Interview	He/she wanted to find a job consistent with previous career, not any job
full	Dichotomous (obs.)	Interview	He/she wanted a full-time job
d30-60	Dichotomous (obs.)	Interview	To work, he/she was ready to commute 30 up to 60 minutes from home
d60-90	Dichotomous (obs.)	Interview	To work, he/she was ready to commute 60 up to 90 minutes from home
d90more	Dichotomous (obs.)	Interview	To work, he/she was ready to commute more than 90 minutes from home
transfer	Dichotomous (obs.)	Interview	To work, he/she was ready to move elsewhere
(baseline: less than 30 minutes)		Interview	He/she would have accepted a job not more than 30 minutes from home
Attitude towards training			
discour.	Dichotomous (obs.)	Interview	He/she believed training was useless
self_help	Dichotomous (obs.)	Interview	He/she believed it was useful to acquire generic competencies and new knowledge and/or increase self-esteem
determined	Dichotomous (obs.)	Interview	He/she believed it was useful to find a well-specified job
(baseline: doesn't_know)		Interview	Uncertain
Descriptors of local labour markets			
Place of residence	Dummy vector	Archives	He/she intends to take advantage of the peculiarities in local demand for labour