Workplace Training Programs: Instruments for Human Capital Improvements or Screening Devices?

Brunetti, Irene and Corsini, Lorenzo

12 May 2014

Workplace Training Programs: Instruments for Human Capital Improvements or Screening Devices?

Irene Brunetti
Department of Economics and Management, University of Pisa

Lorenzo Corsini

Dipartimento di Scienze per l'Economia e l'Impresa, University of Florence

Abstract

In this paper we analyse the effect of an Italian training program on the re-employment probability of young unemployed workers. The program consists solely of workplace training and is coordinated by employment centre, but it is fully implemented by firms.

We develop a discrete duration analysis and our results suggest that workplace training improves only the immediate re-employability of trained workers, failing to bestow them with durable human capital improvements. These results appear to be robust to spurious duration dependence and to self-selection. Our analysis focuses on the role of unobserved heterogeneity and, accounting for it, we show that the training implementation is useful to sort "good" trainees from "bad" ones: therefore we suggest that firms are exploiting training as a screening device.

Keywords: duration model; policy evaluation; propensity score matching; screening device; workplace training; youth unemployment.

JEL Classification: C41; I38; J64; J68; M53

Corresponding Author:

Lorenzo Corsini. Dipartimento di Scienze per l'Economia e l'Impresa, University of Florence, e-mail: lorenzo.corsini@unifi.it

Acknowledgements

We are grateful to the Employment Centre of the Province of Pisa to provide us with data and assistance. We particularly thanks Simone Nannetti for assisting us in handling the data.

1. Introduction

During the last decades there has been an increasing interest in active labour market policies (ALMPs), i.e. measures to facilitate employment or re-employment conditions. As a matter of facts, from a policy point of view, ALMPs are becoming a key instrument to fight unemployment and to improve workers' standard of living. According to conventional definitions, ALMPs comprise: i) job search assistance to improve the matching between vacancies and unemployed; ii) labour market training; and iii) job creation (subsidized employment).

The recent interest in ALMPs motivates a thorough evaluation of how successful these actions in various countries have been in fighting unemployment, in improving employment conditions and productivity and even in promoting a fairer income distribution. Overall assessments of ALMPs can be found in Kluve and Schmidt (2002), Kluve (2010) and Card et al. (2010) while a discussion of methods and issues in the econometric evaluations of ALMP is contained in Heckman et al. (1999).

This article tries to assess the effect of a specific ALMP implemented in Italy, consisting in workplace training for young unemployed. This article intends to assess the causal effect of this specific training program on the probability of employment after a certain period from the treatment. Broadly speaking, generic training programs usually encompasses measures like classroom training, workplace training and work experience. These measures can provide general education, specific vocational skills or even firm-specific skills. Their main objective is to enhance the productivity and employability of the participants and to enhance human capital by increasing their skills. In addition, while not their main objective, training programs can also act as a mean to connect workers with firms and, in some cases, can also be used by firms as a screening device or, when misused, even as a source of cheap labour.

In the specific case we are observing and evaluating, the program consists solely on workplace training and work experience for young unemployed workers and is activated and coordinated by Employment Centres but implemented directly by firms. This training program is ruled by Italian laws but some of its specific aspects are overseen and determined by regions, Tuscany in our case.

The evaluation of this typology of program is important for several reasons. First, it is relevant because it allows to focus specifically on the workplace component of training, something that has not been deeply examined or evaluated in previous analysis on generic training. Second, we evaluate a program targeting youth unemployment which is currently considered one the most urgent economic issues. Finally, our analysis also helps in understanding, from a sociological and economic perspective, the actual processes adopted by firms when implementing workplace training.

Among the few studies that cover this specific form of training, Bonnal et al. (1997) found that workplace training in France increases the transition from unemployment to employment but only for some educational groups while Brodaty et al. (2001), using again evidence from France, found that workplace training is more beneficial for re-employment than forms of training erogated by public institutions. Caroleo and Pastore (2001) focused on long term unemployed and find that workplace training and participation to ALMP in Italy do not significantly improve the employability of these workers.

From a methodological point of view, the evaluation of the efficacy and success of ALMP should focus on their causal effect, defined as the difference between the outcome of the units affected by the policy (the actual situation) and the outcome that these *same* units would have experienced if the policy had not been implemented. The fundamental evaluation problem is that it is not possible to observe simultaneously the same unit in the two scenarios, i.e. the scenario in which the policy is not implemented, the *counterfactual*, and the scenario in which the policy is implemented. Therefore it is needed an adequate control group that is similar as possible to the affected one. To find such a *control* group is not easy and there are mainly two different approaches: i) comparing the outcome of interest of the affected units before and after the intervention; and ii) comparing units affected by the intervention with those not affected. Both approaches have their own pro and cons, for a discussion of these aspects (see Loi and Rodrigues (2012)(.

Our analysis uses the latter approach as our sample, individuals aged between 18 and 30 years old registered at the Employment Centre of the Province of Pisa, comprises both individuals that underwent the workplace training program (the treated) and individuals that did not underwent it (the non-treated). In particular, we compare the re-employment process of individuals that just finished their workplace training program with individuals that just lost their job.

In addition, the empirical analysis to assess the causal effect of a treatment may suffer from two further problems: *spurious duration dependence* in the survival analysis and *self-selection into treatment*. While survival analysis is usually able to deal with duration dependence, in some cases specific problems may arise. In particular, the presence of unobservable heterogeneity and time-varying effect of the treatment could complicate things and we deal with it specifically including a component of unobserved heterogeneity. As for self-selection into treatment, the problem is that it is not easy to distinguish whether a given observed effect is due to the participation to the program or to have been selected to participate to the program. In general, it is possible that the characteristics that induce individuals to be selected into the program also have direct effect on the re-employment probability. There are several methods to overcome the self-selection problem, in our analysis we adopt the propensity score matching (PSM) methodology.

In the specific case we are assessing, the descriptive data on re-employment for treated and non-treated individuals would suggest that the treatment has a positive effect immediately after the end of the training program but this effect quickly dissolves and even reverts to negative during later stages of unemployment. Starting from this simple descriptive evidence we perform an econonometric analysis and we account for spurious duration dependence and self-selection into treatment: our results show that the causal effect of training is to all extent initially positive and becomes negligible (but not negative) after few months from the end of the training.

A decline of the treatment effect of generic (classroom) training program is also found in Osikumino (2013) and in Richardson and Van Den Berg (2013). The latter pays special attention to this decline and conclude that, for classroom training, the decline does not concern actually the causal effect of training on the workers' skills but rather is related to: i) the interaction between the treatment effect and the unobservable heterogeneity (so that training is more profitable in terms of skills acquisition for "better" workers); ii) the boost in the job assistance at the termination of the training. Our conclusions are in part similar to Richardson and Van Den Berg (2013), as we argue that "better" workers are more likely to increase re-employment as an effect of having participated to training, and in part different, as we suggest that this is due to screening mechanisms inherent in the workplace training program rather than from an increase in actual skills of workers. From a sociological and economic point of view our results also shed light on how the firms actually use the training programs.

It is worth to note that the presence of a similar screening mechanism was detected in analyses that tried to assess the role of fixed-term contracts in future employability (see Baranowska et al. (2011)): from this point view firms appear to exploit workplace training and fixed-term positions to obtain similar objectives.

The article is organized as follows. Section 2 briefly presents the Italian workplace training program; Section 3 describes the data used and present the empirical analysis and its results. Section 4 concludes.

2. The Workplace Training Program

While Italian law contains some guidelines on interniships and training (contained in Art. 10, legge 196/97) the actual implementation and regulation of the programs are left to the regional-level governments. According to the Tuscany quality charter on internships and workplace training, "the workplace training program is a measure that aims to create a direct link between a job-seeker and a firm, and to allow the trainee to gain more experience to upgrade his curriculum and to facilitate a future possible business relationship with the host firm. It consists in a period of professional training and work counseling that allows young individuals to be in direct touch with the production sector". Therefore it aims to accelerate the matching process between demand and supply in the labour market.

The training program consists of heterogeneous instruments that differ in the form and intensity of the human capital investment as well as in their respective duration. Regarding to the duration, it ranges from a 2 months to 6 (including extensions), and it can be extended to 1 year only for a graduate or disadvantaged people. In our analysis we consider only those training programs with a maximum duration of 6 months.

Workplace training is associated to work experience. It is mainly reserved to young people, under 30 years of age, who have completed their compulsory schooling. It envisages an agreement between the Employment Centre, that promotes the training, and the host (public or private) firm providing a training project. The official agreement between the participant and the host firm is not a labour contract and hence no remuneration is compulsory, but the host has to ensure the participant against workplace injuries. For each agreement the Employment Centre appoints an in-firm tutor whose duties consist in following the workplace activities of the trainee.

Several specifications qualify the position of the participants to the program and try to avoid its misuses from the firm. In particular, it is specifically ruled that: 1) the trainee cannot be assigned to activities that do not necessitate actual training, 2) the trainee cannot be assigned to strictly seasonal activities nor can fill in for employees currently on leave, 3) all the activities of the trainee within the firm must be related to the training objectives of the agreement, 4) firms cannot sign more than an agreement with each single trainee. Together with these specifications and to avoid that the participant to the workplace training will be considered as an unpaid worker, some further guarantees are entailed within the agreement: pre-determination of promoters, maximum duration of the training, and transmission of the agreement to public authority (Regions and Direzioni Provinciale del Lavoro). Moreover, the workplace training should be distinguished from the apprenticeship since the latter is a contract of employment.

3. The Empirical Analysis

We perform now an empirical analysis on the determinants of re-employment focusing on the effect of the workplace training program we described above.

In our analysis we observe individuals for a total of 8months after they started searching for a job. We discretize duration of unemployment in blocks of 2 months, obtaining thus a maximum duration of 4 time periods. We choose to use discrete duration because it allows for the fact that the effect of a given variable (workplace training in our case) can differ at different points in time. While continuous models can deal with time-varying effect, their estimation is mostly restricted to cases where the effect varies continuously and smoothly with time, failing to capture discontinuity or time threshold in those effects. At any rate, we also perform continuous duration regressions as a robustness check for our analysis.

3.1 Data

The dataset used comes from the database of the Employment Centre of the Province of Pisa. We consider the totality of individuals, aged between 18 and 30 years old, enrolled at the Employment Centre: some of them underwent the workplace training program while others did not.

In our evaluation the treatment group is composed by those individuals that terminated the training program during the last nine months of year 2012, whereas the non-treatment group is composed by individuals that became unemployed during the same period of year 2012. Overall we have a sample of 4, 087 observations.

The variables used in the analysis are: gender, age, age squared, a dummy for the participation to the workplace training program (TRAINED), education represented by dummies for vocational secondary school (VSS, implying only three years of vocational school), upper secondary school (USS), university degree (UD) and using compulsory education as the reference category. We include also a dummy indicating whether the individual previous training/job ended during the third or fourth quarter of 2012 (Q3 and Q4 respectively and we use the second quarter as the reference category).

3.2 Descriptive Analysis

We start our analysis simply presenting the share of individuals that are able to find a job within a certain amount of time, distinguishing the trained from the non-trained individuals. In particular, we focus on time blocks of 2 months so that we report data for 4 blocks of time. The first block reports the share of individuals that found an employment within two months from the end of their previous working experience, the second block reports the share that found employment within four months conditional of still being employed after two months and so on.

Table 1 reports the share for each time block and it highlights some clear patterns. First, the share of individuals that finds employment is decreasing in the duration of unemployment (with 29.29% in the first block and 11.55% in the last block). Second, in the first period, the share of individuals that find an employment is much higher in the trained group (41.15% for the trained versus 28.70%for the non-trained). Third, during later periods, this latter pattern reverses and the non-trained group shows higher re-employment rates than the trained group: this is evident starting from the second period and becomes particularly relevant in the fourth one.

[Table 1: Share of individuals finding a job within 2 months (conditional of still being unemployed after *x* months).]

Clearly this evidence is merely descriptive and it does not necessarily implies a causal effect of time or training on re-employment probabilities. However, the reversal of the effect of training is particularly interesting and can be further investigated performing a complementary log-log regression where the binary outcome represents the event of finding a job.

The complimentary log-log regression is similar to the logit regression but assumes a complementary log-log distribution for the errors and it thus uses a different link function (the function that transforms the actual outcome to its estimated value): in particular, it uses the inverse of the generalized extreme value cumulative distribution function. These assumptions allow to obtain better estimates when one of the possible outcomes (finding a job in our case) is observed to be consistently less likely than the alternative (see Long (1997)).

To emphasize the differences within the different periods we perform the regression both for the first period (with all the sample) and for the fourth period (with only individuals that were still unemployed after six months). The results are reported in Table 2 and they represents the effect of the covariates on re-employment probabilities.

[Table 2: Estimate of log-log regression for the first and fourth period]

As it is apparent from the above regressions, the effect of the training appears to revert from positive in the first period to negative in the fourth period. This result could suggest that the training program initially facilitates finding a job but during later stages it instead reduces the probability of re-employment.

However, extreme caution should be put in formulating such conclusions. In facts, there may be factors that alter and bias the detected relationship between training and re-employment. In particular, we should put great attention in dealing with the issue of i) *spurious duration dependence* that generate an actual selection into sample during later stages of unemployment and ii) *self-selection into treatment*. The first issue could emerge because during later stages we only observe individuals unemployed up to that specific period. If the probability to remain in unemployment is only due to observed variables no problems should arise; however, if instead there is some unobserved heterogeneity that is individual-specific our result could be biased. Basically, the presence of unobserved heterogeneity could account for the fact that, apart the observed variables, some individuals are simply “better” than others. In particular, it could exist some selection mechanism that relates a certain category of worker (the trained, for example) with the probability that “good” workers are actually awarded with a job. In these instances the estimated effect of belonging to that category (the trained) could be biased during later stages and there could be an actual selection into (later stages) sample of the “good” or “bad” workers that is asymmetrical across certain categories. Therefore the difference in different periods in the effect of being trained may be due to an asymmetrical selection into sample during later stages of unemployment. To account and check this aspect we are going to perform a discrete duration regression where we explicitly take into account for unobserved heterogeneity, which is called "frailty" in duration analysis terminology (see Subsection 3.3).

The second issue is a known problem in the program evaluation analysis: in practice, it is possible that selection into treatment is not random and therefore certain categories of individuals are more likely to participate to it: in these circumstances, the effect of the treatment is confounded with the effect of the characteristics that make the participation to treatment more likely and the result we obtain from the regression are biased: to explore and account for this issue we perform a propensity score matching analysis (see Subsection 3.4).

3.3 Discrete Duration Analysis

We develop now a survival analysis using discrete duration regression. As we already mentioned, we have partitioned unemployment duration in 4 time periods, each of which is 2 months long. Given this partition, the discrete duration analysis consists in panel binomial regressions, where each individual, in each time period, has a probability to find a job that depends on observed characteristics.

This specification of the model allows to specifically estimate in each single stage of unemployment, the effect of having participated to the training program. The discrete structure allows this effect to change from period to period and to follow non-linear and non-continuous patterns. From this point of view, the discrete duration estimation is better than the continuous one as the latter would only allow to estimate whether the effect of training is time-varying and what the direction of this variation is but would not allow to assess if and when this effect reverts its sign or becomes negligible.

Within this setup, we introduce time dummies (T2, T3 and T4 for second, third and fourth time period, with the first one being the reference period) to capture the duration dependence and the pure effect of time. Given that some individuals find a job during the first period, not all of them are present in the following periods, therefore the panel is unbalanced. However, the robustness of the estimates is not compromised by this self-selection in the later periods. The panel structure in fact allows to capture the unobservable components that produce higher probabilities to stay (or leave) the sample during each period. In particular, we are de facto assuming a random effect that is individual-specific and that explains the fact that, during the later stages, individuals still unemployed might have "worse" unobservable terms than those that found a job. Within the duration analysis the unobservable term is called "frailty" term and its explicit inclusion and estimation avoids the bias that could arise if the unobservable becomes asymmetrical across certain category of workers during the later stages of unemployment.

In our analysis we use again the log-log complementary regression but we do so in a panel model where the random effect component is represented by a gamma frailty: that is, we assume a Gamma distributed individual-specific error term. To check for robustness we also specify and estimate the model assuming a Gaussian distributed frailty. We test the presence of the frailty term to assess its presence and to verify that we are controlling for it.

Table 3 reports results for the two specifications of the model (in the Table, we represent with Trained1, Trained2, Trained3 and Trained4 the effect that training have during first, second, third and fourth period of unemployment respectively): in table, the coefficients represent the effect of the covariates on re-employment probability.

[Table 3: Estimation of discrete duration model.]

The estimates contained in the table highlights how the effect of participating to a training program has a strong significant positive effect on re-employment during the very first stages of the unemployment spell, but this effect becomes smaller and non-significant starting from the second time-period (that is, after two months of unemployment).

Comparing these results with those in Table 2 (in which we are not controlling for unobservable heterogeneity) we observe that the result for the first period is confirmed, while that for the fourth one changes, with training being actually non-significant. These differences between the results in Table 2 and Table 3 suggest that, during the later stages of unemployment, individuals with training are associated with "worse" unobservable characteristics, and thus, during these time periods, training may be erroneously considered detrimental to re-employment. In fact, once we explicitly account for individual-specific heterogeneity, the negative effect of training disappears.

This line of reasoning suggests that individuals participating to this training program and that remains unemployed after two months are somehow "worse" than their non-trained counterparts, suggesting that individuals with training are better screened or they are better in signaling their true skills. This result, together with the finding that training program appears ineffective after the first two months, suggests that training program fails in bestowing the individuals with better skills or competences but they instead act as good screening devices Table 3 also shows other relevant, even if expected, results: men have better chance to find a job than women and older individuals are better-off than younger, though the returns from age are decreasing (remember that we are referring only to the 18-30 age group). Education is positive but not significant and this result may be conditioned by the lack of information on fields of study: individuals with higher degree might have more re-employment chances but this result may revert for some fields of study.

Time effect seems to be not significant while the dummies representing the quarter when the search process began are associated with significant negative coefficients: this could signal an overall worsening of the employment opportunities, something which is perfectly in line with unemployment rate at the aggregate level (within the Province of Pisa, unemployment rate raised from 6.8% in 2012 to 8.6% in 2013). All the results remain stables in both specifications of the model and are fully compatible from what we would obtain estimating the model with continuous duration regressions (see Appendix A).

3.4 Propensity Score Matching Estimation

We turn now to deal with the problem of self-selection into treatment. To understand the extent of the problem when measuring the effectiveness of the training program, suppose that $(Y_1, Y_0)_i$ are the two potential outcomes on the i-th population unit of being treated or not treated, respectively. If a specific member of the target population receives the treatment then is observable (≡factual), while Y0 is irreversibly non-observable and corresponds to what we would have observed if this unit had not received the intervention (≡*counterfactual*).

The equation that relates Yi, the real outcome that is observed on unit i of the target population, to the potential outcomes of the same unit is the following:

$$Y_i = Y_{1i}D_i + Y_{0i}(1 - D_i) = \begin{cases} Y_{1i} \text{ if } D_i = 1 \\ Y_{0i} \text{ if } D_i = 0 \end{cases}$$

where D_i is the treatment status of the i-th population unit: $D_i = 1$ if the unit received the intervention and 0 otherwise. Given that to calculate individual-unit causal effects is typically impossible (and it is also less interesting from the policy point of view), the literature focuses its attention on the estimation of aggregated causal effects. In this article we estimate the average effect on units in the target population that were assigned to treatment (ATT):

$$ATT = E(Y_{1i} - Y_{0i}|D_i = 1) = E(Y_{1i}|D_i = 1) - E(Y_{0i}|D_i = 1)$$

ATT measures the average treatment effect for the units actually exposed to the intervention and is the parameter of major interest for policy evaluation. It is important to note that the first term is observable (≡factual outcome), while the second term, E($Y_{0i}|D_i = 1$), the average effect on the treated in the case they had not been treated, is not (≡counterfactual outcome).

One could take the outcome of non-participants as an approximation to the outcome that participants would have had without treatment. This would be a correct approach if (and only if) participants and non-participants have similar characteristics, i.e. if they are comparable *a priori*, had the treatment not been implemented. In general, however, participants and non-participants differ in crucial characteristics that are related both with the participation status and the outcome. This problem is known as "*self-selection* bias". The bias arises when treated and non-treated are systematically different even before the participation to the intervention. This could happen because assignment to the program is non-random and, the estimated effect on re-employment of the participation to the program could be due not to the actual attendance of it, but to the unobserved characteristics that makes more likely to attend the program.

A possible way to account for the presence of self-selection and to mitigate its bias is the use of the propensity score matching (PSM) methodology.

The basic idea behind PSM is to compare treated individuals that are "similar" to non-treated individuals and to produce then an estimate of the treatment impact given by the difference between the outcome of the treated with the outcome of the matched comparison cases. The key issue is how to define similar individuals and the PSM determines the similarities computing what is called "propensity score": this measure is defined as the probability that an individual receives the treatment given a set of observed variables. Two individuals whose propensity scores have similar values are thus considered "similar". The very computation of the propensity score is obtained with the estimation of a selection model, a probit model in our case, in which the participation to the treatment is regressed on the characteristics of the individual and the probability to participate is thus computed.

In our analysis we assume that being part of the training program defines the treatment to which individuals are exposed. Once the propensity score is computed, matched individuals can be compared and the effect of the treatment estimated averaging the differences in the outcome of all matched cases. There are several methods to perform the match: to check for the robustness of our results, we use the four most common techniques: stratification, radius, nearest neighbor and kernel. We apply this methodology to assess the effect of training to the probability of finding a job within two months: the results[1] are reported in Table 4, and they measure the average effect on re-employment probabilities of the treatment on the treated.

[Table 4: Estimation of the effect of training via Propensity Score Matching.]

According to the PSM analysis, the effect of training in the initial stage of unemployment is strictly positive confirming the significance and the magnitude of the effect. Therefore, even when we control for the possible self-selection into treatment, the positive effect of training is confirmed. This also means that the self-selection, if present, is not particularly strong and, at any rate, it is not strong enough to bias the findings of the discrete duration regression. This is true at least for the initial effect of training: a similar PSM analysis cannot be performed for later stages of unemployment given that, for those stages, a selection into the surviving sample is also present. However, if self-selection is not present at the beginning (as the PSM analysis suggests) there is no reason why it should arise and begin during the later stage and thus, the results we obtained in the discrete duration analysis should be robust for all the time horizons.

3.5 Summary of Results

Our analysis has highlighted two key aspects. First, attending a workplace training program increases the initial probability of finding a job and this result holds true even when we account for self-selection into treatment. Second, the effect of training becomes negligible after about two months and, more in details, it appears that, among the individuals that were unemployed for at least two months, those with training are associated with "worse" unobserved heterogeneity.

These results seem to suggest that workplace training programs only offer more chance to be hired immediately after their end. This can happen either because the training is strictly firm-specific and the firm providing the training directly hires the worker either because the worker may have exploited the permanence in the firm to improve network and contacts so that this can be exploited to obtain more easily a job. However, this improvement in reemployment probability is not permanent, suggesting that training is not actually good in permanently providing or improving skills.

Moreover, the asymmetric selection into sample that we observed during later stages of the unemployment suggests that workers with training are better screened so that, among the trained, the effectively "worse" workers are more likely to stay unemployed than their counterparts. This is also informative on how firms actually use and take advantage from the program and our results strengthen the idea that firms are using the training programs merely as screening devices.

Summing up, the effect of the workplace training programs is partly disappointing as they fail to provide an enduring increase in the skills and re-employment prospects: on the contrary, the program is successful in easing the connection between workers and firms (as trained have a higher initial probability of finding a job) and act as screening devices for firms. Given that matching and screening issues are known to prevent full employment, workplace training programs are not completely useless but they still appear to fail at least in one of their main aims.

4. Conclusions

In this article we evaluate the effect of workplace training program for young unemployed. The core of our analysis is to assess if and how this training is beneficial to the employability of the trained individuals. Descriptive evidence shows that the participation to the training program has a positive effect immediately after the end of it, but this beneficial effect rapidly dissolves and reverts to negative during later stages of unemployment. However, performing a more accurate econometrical analysis, we confirm the initial positive effect on re-employment but we find that the beneficial effect, while not persisting in time, does not revert to negative.

Our analysis indicates that what is actually happening is that during later stages of unemployment we observe an asymmetric selection into sample among trained and non-trained individuals: this suggests that workers with training are better screened so that, among the trained, the effectively "worse" workers are more likely to stay unemployed than their counterparts.

Our evaluation of this workplace training suggests that the implemented program is successful in easing the connection between workers and firms, and acts as screening devices for firms, but it fails to provide a durable improvement in the skills and in the re-employment prospects.

Notes

1 Propensity score is obtained with a probit regression where participation is regressed on the same variables used in the duration analysis with the exception of the time dummies. The number of blocks for the propensity score is 8 and the balancing property is respected: this ensures that the mean propensity score is not different for treated and controls in each block. Standard error are computed via bootstrap for the Kernel matching and analytically for the rest of the matching methods

References

Baranowska, A., M. Gebel, and I. E. Kotowska (2011). The role of fixed-term contracts at labour market entry in Poland: stepping stones, screening devices, traps or search subsidies? *Work, Employment & Society 25*(4), 777–793.

Bonnal, L.,D. Fougere, and A. Serandon (1997). Evaluating the impact of french employment policies on individual labour market histories. *Review of Economic Studies 64*(4), 683–713.

Brodaty, T., B. Crépon, and D. Fougére (2001). Using matching estimators to evaluate alternative youth employment programs: Evidence from france, 1986-1988. In M. Lechner and F. Pfeiffer (Eds.), *Econometric Evaluation of Labour Market Policies*, Volume 13 of *ZEW Economic Studies*, pp. 85–123. Physica-Verlag HD.

Card, D., J. Kluve, and A. Weber (2010). Active labour market policy evaluations: A meta analysis. *Economic Journal 120*(548).

Caroleo, F. E. and F. Pastore (2001). How fine targeted is almp to the youth long term unemployed in Italy? (62).

Heckman, J. J., R. J. Lalonde, and J. A. Smith (1999). The economics and econometrics of active labor market programs. In *Handbook of Labor Economics*, Volume 3, pp. 1865–2097. Elsevier.

Kluve, J. (2010). The effectiveness of European active labor market programs. *Labour Economics 17*(6), 904–918.

Kluve, J. and C. Schmidt (2002). Can training and employment subsidies combat European unemployment? *Economic Policy 17*(35), 409–448.

Loi, M. and M. Rodrigues (2012). A note on the impact evaluation of public policies: the counterfactual canalysis. *MPRA Paper* (42444).

Long, J. (1997). *Regression Models for Categorical and Limited Dependent Variables*. Advanced Quantitative Techniques in the Social Sciences. SAGE Publications.

Osikumino, A. (2013). Quick job entry or long-term human capital development? The dynamic effects of alternative training schemes. *Review of Economic Studies 80*, 313–342.

Richardson, K. and G. Van Den Berg (2013). Duration dependence versus unobserved eterogeneity in treatment effects: Swedish labor market training and the transition rate to employment. *Journal of Applied Econometrics 28*, 325–351.

APPENDIX 1

In this appendix we present results from estimation of the survival model using continuous survival models. This allows to test the robustness of our estimation and to check whether our results depend too much on the discrete hazard model we used.

In Table 5 we present the estimates for Cox parametric regression assuming that baseline hazard is distributed according to a Weibull distribution: in the regressions we also assume that the effect of training is time varying and that a frailty component is present. In specification (i) the frailty component is distributed as a Gamma distribution while in specification (ii) it is distributed according to an inverse-gaussian distribution.

[Table 5 Estimation of continuous duration model.]

We are mainly interest in the effect of training and from the above table we see that its effect on re-employment is, in both specifications, initially positive (direct effect is significantly positive) but it declines with time (interaction with time is significantly negative), possibly reverting to negative. This is clearly compatible with the results we obtained in the discrete hazard regression. The continuous regression, however, is not able to exactly pin whether and when the effect of training becomes negative so that the discrete hazard model appears to be more informative from this point of view. The results in the above table also confirm all the signs we found in the discrete hazard regression though significance in some case is slightly different. The only relevant difference between the discrete and continuous version stems from the effect of time (the baseline hazard in the continuous version). In the estimations in the above table we find that the effect of time is negative in specification (i) and positive in (ii). The change in its sign might be a symptom that the Weibull distribution is not the correct choice to describe the baseline hazards and it thus reinforces our choice of using time dummies in the discrete hazard model.

Tables

Table 1: **Share of individuals finding a job within 2 months (conditional of still being unemployed after *x* months).**

	Immediately	After 2 months	After 4 months	After 6 months
OVERALL	0.293	0.165	0.131	0.115
Among TRAINED	0.411	0.150	0.094	0.069
Among NON-TRAINED	0.287	0.166	0.133	0.117
Number of obs.	4,087	2,890	2,412	2,095

Table 2: **Estimate of log-log regression for the first and fourth period**

Vairables	Within 2 months	After 6 months
Female	-0.203***	-0.268**
	(0.0590)	(0.131)
Age	0.412***	-0.298
	(0.147)	(0.301)
Age Squared	-0.008***	0.005
	(0.00290)	(0.00599)
Trained	0.592***	-0.876**
	(0.122)	(0.419)
Education: VSS	0.117	0.260
	(0.126)	(0.286)
Education: USS	0.0225	0.279*
	(0.0653)	(0.143)
Education: UD	0.0285	0.620***
	(0.100)	(0.217)
Q3	-0.375***	0.505***
	(0.0681)	(0.182)
Q4	-0.383***	0.965***
	(0.0754)	(0.182)
Constant	-6.103***	1.445
	(1.839)	(3.723)
Observations	4,087	2,095

Notes: Standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 3: **Estimation of discrete duration model.**

Variables	Gamma frailty	Gaussian frailty
Female	-0.262**	-0.147*
	(0.122)	(0.0850)
Age	0.623**	0.466**
	(0.250)	(0.211)
Age Squared	-0.0118**	-0.00884**
	(0.00490)	(0.00411)
Trained1	1.048***	0.822***
	(0.329)	(0.277)
Trained2	0.656	0.291
	(0.463)	(0.350)
Trained3	0.288	-0.0546
	(0.515)	(0.402)
Trained4	-0.0479	-0.318
	(0.556)	(0.457)
Education: VSS	0.253	0.249
	(0.206)	(0.167)
Education: USS	0.116	0.124
	(0.101)	(0.0836)
Education: UD	0.208	0.213
	(0.159)	(0.131)
T2	0.168	-0.0697
	(0.289)	(0.323)
T3	0.425	-0.0493
	(0.473)	(0.460)
T4	0.700	0.00538
	(0.628)	(0.556)
Q3	-0.689***	-0.531***
	(0.163)	(0.159)
Q4	-0.600***	-0.371***
	(0.187)	(0.139)
Intercept	-8.148***	-7.383***
	(2.981)	(2.803)
LR test on Frailty Component	13.5953***	6.96***
Numb of Observations	4087	4087

Notes: Standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 4: **Estimation of the effect of training via Propensity Score Matching.**

	Stratification Matching	Radius Matching	Nearest Neighbour Matching	Kernel Matching
Effect of Training (ATT)	0.151***	0.135***	0.120**	0.138***
	(0.038)	(0.036)	(0.05)	(0.036)

Notes: Number of Blocks: 8. Balancing properties satisfied for all blocks.
Standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 5 **Estimation of continuous duration model.**

Variables	(*i*) Gamma Frailty	(*ii*) Inverse-Gaussian
Female	-0.144**	-0.123
	(0.0640)	(0.0793)
Age	0.452***	0.525***
	(0.151)	(0.194)
Age Squared	-0.00861***	-0.0100***
	(0.00298)	(0.00383)
Trained	1.122***	1.407***
	(0.181)	(0.226)
Effect of Time on Training	-0.0121***	-0.0155***
	(0.00252)	(0.00345)
Education: VSS	0.220	0.292*
	(0.135)	(0.173)
Education: USS	0.112	0.154*
	(0.0683)	(0.0878)
Education: UD	0.215**	0.303**
	(0.106)	(0.133)
Q3	-0.514***	-0.582***
	(0.0796)	(0.0932)
Q4	-0.369***	-0.346***
	(0.0839)	(0.0997)
Constant	-9.530***	-10.40***
	(1.894)	(2.426)
Log of Ancillary parameter	-0.248***	0.0928***
	(0.0393)	(0.0343)
LR test on Frailty Component	38.19***	79.01***
Numb. Of Observations	4,087	4,087

Notes: Standard errors in parenthesis: *** p<0.01, ** p<0.05, * p<0.1.