Training programmes and employment outcomes. Evidence from an Italian province.

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Abstract

Training programmes represent a key labour policy that, for many aspects, is managed at a local level. Moreover, the relevance of this policy is strictly correlated to the flexibility of the labour market, since it allows workers to acquire or improve skills to move from a job to another. The main difficulty of these analyses is to overcome the so-called "selection bias" arising from the non-random selection of trainees, that does not allow to correctly single out the net effect of the training.
This paper aims at evaluating the impact of training programmes of the province of Novara on a set of outcomes. The empirical analysis is based on a unique cohort survey of 1700 individuals resident, at the moment of the interview, in this territory. In addition to personal and household information, a specific section of the questionnaire is dedicated to training with detailed information about the typology of the programmes, their characteristics and their perceived quality.
The adopted methodology is the propensity score matching that allows overcoming the selection bias problem by comparing trainees with nontrainees that are as similar as possible for all relevant observable characteristics. As to test the robustness of our results we will adopt a partially alternative methodology (control function) that exploit the outcome equation.
The results indicate zero effect of training on employment chance, but a positive effect on the confidence of workers towards their own skills. The other results are less robust, especially if we limit our analysis to publicly-provided training programmes.

KEYWORDS: Training programmes; Active labour policy evaluation; Matching estimators; Control function.

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1.Introduction

The evaluation of training programmes and, in general, of all active labour market policies, has had in the last years a strong development[1] (between the most recent: Gerfin and Lechner, 2001 for Switzerland; Kluve and al., 2001 for Poland; Larsson, 2003 and Sianesi, 2001 and 2004 for Sweden) due to the necessity, for the policy makers, of collecting accurate information about the outcomes of their policies.

The main problem in these analyses is to correctly disclose the effect of the programmes from the effect of observable or unobservable individual characteristics, so as not to attribute to the programme a greater (or littler) effect than the "true" one. In other words, the aim of these studies is to evaluate the *net* effect of a programme, that is the difference between the outcome of a person that attended the programme (treated) and the outcome of the *same* person that did not attend the programme (counterfactual). Nevertheless, as observed by Heckman and al., "the fundamental aspect of the programme evaluation problem is that one cannot simultaneously observe the same person in a programme and out of it" (Heckman, Smith and Clements, 1997) and a different strategy has to be implemented.

In order to evaluate the impact of a treatment[2] the optimal strategy is to conduct an experiment where some people are randomly drawn from a population and treated (randomised experiments). Using the non treated as controls, the net effect of the treatment can be measured as the difference (in average) between the outcomes of the treated and the outcomes of the controls. The random extraction assures that the treated and the controls are similar, that is that they share the same distribution of the observable and unobservable characteristics. As a consequence, the difference in the outcomes can be assumed as the result only of the treatment.

In a socio-economical context such experiments are very rare for obvious reasons and the available data are often the results of observational studies, that is *ex post* surveys. Whit this kind of data, comparing the outcomes of the treated and of the non treated (if available) is misleading because the two samples can systematically differ by some important characteristics[3].

Nevertheless, also with social data it is possible to obtain unbiased estimates of the average treatment effects. If selection in the treatment depends on *observable* characteristics, the problem can be easily managed as we will see later on. The difficulty is greater if the selection in the

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[1] A special issue (125/2005) of the *Journal of Econometrics* has been recently entirely devoted to the evaluation of economic programmes and models using data from social experiments. On this see also Blundell and Costa Dias (2002).

[2] Since these analyses were firstly developed in medical/biological contexts, the terminology kept identical. With "treatment" we indicate the programme (policy) whose effects are estimated.

[3] On the comparison between econometric estimates with observational studies and experimental results, see Lalonde (1986).

treatment depends on *unobservable* characteristics[4], that is if there are some relevant differences between "treated" and "non treated" affecting both their probability to be treated and the outcome. Heckman, Ichimura and Todd (1997), however, find that this selection on unobservables is strongly reduced when two conditions occur: firstly, if the same questionnaire is submitted to both treated and controls; secondly, when the observational units share the same economic environment.

The first condition assures that it is available the same information about treated and non treated. Since often only the treated groups are administered the evaluation questionnaires, it is necessary to merge the data set containing information on the treated to external data sets, derived from other studies over the whole population. This merging may lead to biased estimates for two main reasons: firstly, answers might not be perfectly comparable; secondly, it is impossible to check if individuals belonging to the merged data set have been treated or not.

The second condition allows to reduce (eliminate) the omitted variables problem arising both from a different economic environment between treated and the controls and from an economic environment change when questionnaires are submitted in different periods.

The aim of this paper is to evaluate the net effect of training programmes of the province of Novara on a wide set of outcomes, since we assume that new skills provided by training do not only improve employment chances, but also the quality of the jobs, especially in a economic context characterised by low unemployment rates. The empirical analysis is based on a unique cohort questionnaire of 1700 individuals born in 1982 and 1983 and resident, at the moment of the interview, in the province of Novara.

The nature of our data set allows to satisfy both the conditions for reducing the selection on unobservables previously described. In order to manage the selection on observables, we adopt the propensity score matching and we the check the robustness of our results with the control function methodology. The well known propensity score matching estimator (Rosenbaum and Rubin, 1983, 1984; Angrist, 1995; Dehejia and Wahba, 1998a and 1998b; Smith and Todd, 2001; Heckman and Navarro-Lozano, 2003; Moffit, 2004; Zhao, 2004; Dehejia, 2005) rests on the detection of appropriate counterfactuals throughout the estimation of the probability to take a treatment (the propensity score) to be matched with the treated. The average impact of the treatment is then calculated by simply comparing the outcomes of trainees with the outcomes of these counterfactuals. Matching estimators[5] assume that differences in outcomes that do not depend on the treatment and that produce a typical selection bias problem can be resolved using observable variables.

[4] For a detailed illustration of both selection on observables and on unobservable, see Heckman and Hotz (1989).

[5] Propensity scores matching is, as we will see later on, the simplest matching estimator.

The paper is organized as follow. Paragraph 2 presents the propensity score matching estimator emphasising how the selection bias is managed and the remaining problematic issues. Paragraph 3 briefly presents the control function methodology introduced to test the robustness of the previous results. Paragraph 4 describes the most relevant characteristics of the trainees in the province of Novara compared with the nontrainees (controls). Paragraph 5 reports the results of both propensity score matching and control function. Paragraph 6 presents concluding remarks.

2.The Propensity score matching estimator

The propensity score matching methodology, developed by Rosenbaum and Rubin (1983, 1984) and applied in economic evaluation analysis some years later, solves the selection bias problem related to observational studies assuming that selection depends on observable characteristics. Given this assumption, the selection bias can be eliminated by comparing (matching) individuals that have similar characteristics. This methodology, known as "covariates matching", has the advantage to be very accurate since it takes account of every single relevant individual characteristics. The disadvantage is clear: if the number of characteristics to be compared is high, it is difficult to implement it from a computational point of view. Propensity score matching allows to overcome this difficulty by reducing the multidimensionality problem throughout the estimation of the conditional (to observable characteristics) probabilities to take the treatment.

We assume that the potential outcome of individual i is O_{ji} where j is 1 if individual i is treated and 0 otherwise. We then denote by T_i the treatment dummy: T_i is 1 if individual i takes the treatment, 0 otherwise. Since the treatment effect for individual i can be expressed by $(O_{1i}–O_{0i})$, the average treatment effect (ATE) is simply the mean value of this difference over the entire sample. In order to estimate the so called "average treatment effect on treated" (ATT), that generally is the most informative result, the mean has to be simply limited to individual i for which $T_i =1$, that is:

$$ATT = E(O_{1i}–O_{0i} \mid T_i =1) = E(O_{1i} \mid T_i =1) - E(O_{0i} \mid T_i =1) \qquad (1)$$

The difficulty of this simple methodology is clear: in observational studies we can generally observe for each individual either O_{1i} or O_{0i}, since individual is either treated or non treated. With a more formal expression, in observational studies counterfactuals are generally missing.

If we compare two individuals i and j for which, for example, we observe O_{1i} and O_{0j} we are implicitly assuming that the effect of the treatment is the same for every individual[6] (homogeneity assumption), independently from their characteristics. If this assumption is not valid, and in general it isn't, our estimates suffer of a typical selection bias problem.

Nevertheless, if we assume that selection bias only depends on *observable* characteristics and we can check for all these characteristics, we can solve the selection bias problem by comparing individuals (treated and non treated) that are "similar" as regards to these observables. The assumptions to verify, so as to assure that selection is on observables, is:

$$(O_1, O_0) \perp\!\!\!\perp T \mid \mathbf{X} \qquad (2)$$

where $\perp\!\!\!\perp$ represents the statistical independence and **X** is a vector of observable variables. This assumption, also known as "conditional independence assumption" (CIA), states that, when controlling for **X**, treatment is independent from the outcome, that is treatment assignment is a random event[7].

The strong result obtained by Rosenbaum and Rubin (1983) that introduces the propensity score matching as a simple device to reduce the multidimensionality problem is the following. Defining the propensity score as the conditional probability to be treated

$$PS = \Pr(\mathbf{X}) = \Pr(T=1 \mid \mathbf{X}) \qquad (3)$$

they demonstrate that if the conditional independence assumption (1) is verified for **X**, then it is also verified for $\Pr(\mathbf{X})$ if the so called "balancing hypothesis" is satisfied. Denoting by $b(\mathbf{X})$ a general balancing function, this hypothesis states that the conditional distribution of **X** conditional to the balancing function is independent from the treatment :

$$\mathbf{X} \perp\!\!\!\perp T \mid b(\mathbf{X}). \qquad (4)$$

Since Rosenbaum and Rubin (1983) demonstrate that propensity score is the "coarsest" balancing function, observations with the same propensity score have the same distribution of the observable characteristics, independently from the treatment status. The propensity score allows to summarize all the pre-treatment individual characteristics that are considered relevant to

[6] On the homogeneity/heterogeneity assumption, see Heckman, Smith and Clements, 1997.

[7] The second assumption that has to be verified, in order to have the selection on observables, is the so called "common support assumption", that is $0<\Pr(T=1 \mid \mathbf{X})<1$.

the participation decision. As a consequence, it is possible to argue that for treated and non treated having the same propensity scores (or similar propensity scores) taking the treatment is a random event and, finally, that the difference in the outcomes obtained by the treated and by the controls only depends on the treatment.

As a consequence, given the estimated propensity scores and if the balancing hypothesis is satisfied, the average treatment effect on treated (ATT) can be estimated as:

$$ATT = E_{Pr|T=1}\ \{E[\ (O_{1i}\ |\ T_i = 1, Pr(\mathbf{X}_i)] - E[\ (O_{0i}\ |\ T_i = 0, Pr(\mathbf{X}_i)]\} \qquad (5)$$

where $E_{Pr|T=1}$ denotes expectations with respect to Pr ($\mathbf{X}$) for the population for which T=1.

The advantage of estimating ATT as in equation (5) is that propensity score method allows to control for a single value $Pr(\mathbf{X}_i)$ rather than for the $\mathbf{X}_i$ vector.

Although the propensity score method is quite simple and easy to guess, it presents some crucial and problematic features that have to be attentively considered[8].

First of all, it is clear that the "goodness" of the impact estimates, which is related to the elimination of the selection bias, strictly depends on the detection of all the observable variables that are relevant in determining the participation decision. The strong implicit assumption of this method is that the assignment to treatment only depends from these observable characteristics. The choice of the relevant variables has to be made following two exigencies. Firstly, the variables have to be considered relevant from an *a priori* judgement of the researcher founded on his knowledge of the issue and on previous empirical and theoretical literature results. Secondly, and this is the strictest condition, the chosen variables have to satisfy the "balancing hypothesis" as to assure that conditional independence assumption is satisfied also conditioning for the propensity score. Nevertheless, balancing hypothesis simply assure that *if* CIA is verified for **X**, *then* it is also verified for Pr(**X**) and the weakness of the matching estimator rests on the non verifiability of the conditional independence assumption.

3.An alternative methodology: the control function approach

As to test the robustness of the results obtained through the propensity score matching, we complete our analysis with a partially different methodology based on the estimation of the so-called "control function" (Heckman and Robb, 1985; Heckman and Navarro-Lozano; Larsson, 2003). Let's assume our binary outcomes can be expressed as:

[8] On this see Caliendo and Kopenig (2005).

$O_i = I(L_i > 0)$ (6)

$L_i = \beta_0 + \beta_1 \mathbf{Z}_i + \beta_2 T_i + \varepsilon_i$

where L_i is a latent function of a constant β_0, a vector of covariates $\mathbf{Z}_i$ and of the treatment T_i with an i.i.d. error ε_i with $E(\varepsilon_i)=0$. The outcome O_i is assumed to be 1 if the latent variable L_i is >0 and 0 otherwise.

The probability of the outcome to be equal to one can be expressed as:

$$\Pr (O_i = 1) = \Pr (L_i > 0) = \Phi (\beta_0 + \beta_1 \mathbf{Z}_i + \beta_2 T_i) \qquad (7)$$

where Φ (.) is the standard normal cumulative density function.

Estimating (7) to obtain the effect of treatment on outcome gives unbiased results if we can assume that there is no correlation between the treatment T_i and the error ε_i. Instead, if the *unobservables* of the outcome function are correlated to the treatment dummy we have a typical selection bias, since we are not considering some characteristics affecting both the outcome and the decision to attend the treatment. The selection bias can be easily overcame if we assume that this correlation depends on the correlation between $\mathbf{X}_i$ and ε_i, that is if there is a selection on *observables*. To estimate the average treatment effect with the control function, we simply add the $\mathbf{X}_i$ vector to the equation (7) and we estimate:

$$\Pr (O_i = 1) = \Pr (L_i \geq 0) = \Phi (\beta_0 + \beta_1 \mathbf{Z}_i + \beta_2 \mathbf{X}_i + \beta_3 T_i) \qquad (8)$$

Since our outcomes are of different kinds (quantitative and qualitative), $\mathbf{Z}_i$ will be changed in every regression as to control for the relevant individual characteristics.

Even if also this methodology rests on the strong assumption of selection on *observables*, it allows to increase the confidence in our result by exploiting the covariates of the outcome regressions together with the covariates of the selection process.

4. The trainees in the province of Novara: main characteristics

The reduced data set used in the following analysis is composed by the respondents declaring that their prevailing employment situation is: employed, in redundancy fund (CIG) or in mobility, unemployed, in search of the first job. We then exclude all the full time students, the housewives and the other "non actives" that generally include well-off persons, but also people that are preparing to some competitive examination. The data set is then composed of 804 observations, 144 of which attended and completed a training programme.

The relevant (for our analysis) characteristics of the trainees and of the control groups are reported in table 1.

As regards to personal characteristics, the percentage of females, of born in 1982 and of residents in the town of Novara is higher for the trainees than for the controls. Trainees' education level is in average a little higher compared to the controls : both the percentages of individuals that got a diploma in a vocational or in a upper secondary school (i.e. a non compulsory school) is higher for the trainees than for the controls. As regards to the reported final marks, the performances of the controls are a little better compared to the trainees. The remarkable no answer rate on this question (especially as regards to lower secondary school) is explicitly taken into account in the following analysis. With regard to dropouts, trainees present less failures during the secondary schools, while the opposite occurs during university[9]. Finally, there aren't remarkable differences between the two groups as regards the distribution of fails.

Table 2 presents the distribution of the five considered outcomes, always distinguishing between who completed a training programme (trainees) and who did not (controls).

As regards to employment condition, for simplicity, two main employment situations have been distinguished:

1) employed, including also workers in CIG or in mobility[10];
2) not employed, including the unemployed and people in search of the first job.

From the reported data it could be argued that trainees have a little higher (1 percentage point) probability to be employed. Concluding from this first evidence that training programmes have some (little) effect on the employment chance would clearly be a mistake, since there can be a typical selection bias problem. Trainees, perhaps, would have found a job also without the training programme, because they were more skilled than the others also before having attended the training programme. The matching estimator will allow singling out the net effect of the training programmes.

As regards to the first qualitative question about the "goodness" of jobs, the questionnaire contains a large number of information on the (perceived) job quality. For our purpose, we focus on two information, wage satisfaction and general selfrealization, which have the advantage of summarizing the economical and the general facets of the jobs. Table 2 reports that trainees are,

[9] Our sample excludes, by construction, the full time students. The reported dropout percentages concerns only part-time students, that is. students that declare also an employment condition.

[10] None of the trainees is in CIG or in mobility, while 8 controls (1.21%) are in this condition.

in average, less satisfied of their wages as regards to notrainees. The opposite result occurs as regards to selfrealization. Afterwards, we will present a possible explanation to this controversial result by considering the average job tenure of the two groups.

As regards to the second qualitative question about the adequacy of the overall education to the current job, table 2 shows that an higher percentage of trainees (87% vs 65%) consider their skills almost adequate for their current job.

Finally, as regards to the third qualitative question we have distinguished the channels used to find the jobs between "formal" and "informal". Formal channels include ads on newspapers or on internet, CVs to employers, competitive examinations, services offered by training centres, public or private jobcentres, stages or other work experiences, etc. Informal channels are represented by every kind of signalling from relatives, friends or other persons. Table 2 reports that the use of these informal channel is more developed between the notrainees (47% vs. 33%) than the trainees. The interest of this last outcome lies in the idea (to be verified) that attending some training programmes should reduce the necessity to rely on household or social relationships to enter in the labour market since they often offer the possibility to directly contact firms or employers.

5. Results

The aim of the following analysis is to test the effectiveness of training programmes in the province of Novara in a twofold sense.

Firstly, from a quantitative point of view, the question is the typical of all this kind of evaluation analysis, since we ask if training is able to improve the employment chance of the participants.

Secondly, from a qualitative point of view, the questions are more complex. We will ask:

1) if the jobs found after a training course are better jobs according to two indicators (wage satisfaction and selfrealization);
2) if the skills acquired during the whole educational path are perceived as more adequate to the job when also a training programme has been attended;
3) if the participation to training programmes reduce the probability of finding a job throughout "informal" channels, such as household and social networks.

This second analysis is particularly interesting since in the province of Novara the labour market is very active and the unemployment does not represent a main problem, also for young cohorts.

5.1. Propensity score matching results

We report in table 3 the results of four logit estimates of the specification of the participation (selection) equation that satisfy the balancing hypothesis at a significance level of 5%. Column I presents the result of the estimate of the participation to a general training programme. Column II bounds the analysis to the participation to publicly-provided training programmes that are freely provided: the dependent variable, the treatment T_i, here assumes value 1 if the interviewee completed a publicly provided training programme, 0 if he didn't attend any training programme[11]. The main differences between a public and a private training programme are two: firstly, the former is generally free of charge, while the latter is for money; secondly the latter is quite general and open to everyone, while the former often has a specific target (young people, women, unemployed, …) and the participation depends on a selection procedure.

Since we are also interested on the effect of training programmes on the quality of the jobs, we report in column III and column IV the estimates respectively of column I and II over the restricted sample represented by the employed[12].

In all the reported estimates covariates include personal characteristics (gender, year of birth[13] and residence), education dummies (highest education certificate and final marks) and other events occurring during the educational path (failures and dropouts).

The reported estimates allow to distinguish, according to the estimated propensity scores, classes of treated and controls that satisfy the balancing hypothesis, i.e. classes where all the observable characteristics are in average not statistically different between treated and controls.

Personal characteristics that positively affect the probability of participating to a training programme are living in the provincial capital (TOWN) and to be born in 1982. Both these results are not surprising. Living in the biggest town of the province, where the most of the courses are organized, obviously assures the possibility to attend the programmes at a lesser cost. As regards to the year of birth, young people born in 1982 have simply had one more year to attend and complete the programme. No gender effect (FEM) results in these estimates.

As regards to education levels, we distinguish between compulsory (or less) education (the dummy excluded in the estimates), vocational schools ("qualifiche professionali") and upper secondary schools ("scuola secondaria superiore"). Vocational schools are characterized by a short duration (generally three years) and by a specific grounding devoted to labour market.

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[11] We have excluded from the analysis individuals that attended private training course (43 observations over 144) as to compare the outcome of the treated with the outcome of the not treated at all.

[12] The balancing hypothesis has been verified for each reported estimates. The specification is robust at a 5% significance level.

[13] The respondents are born, by construction, in 1982 and 1983. We add, between the covariates, the year of birth since we suppose that the older have had more chances (for a simple age reason) to attend a training programme.

Upper secondary schools have a longer duration (four or five years) and give a more general background. According to the reported estimates, participation to a training programme does not depend on a particular education attainments. If we consider training as a way to improve the skills of low-skilled labour force, this result is not satisfying from the policy maker point of view, since these programmes would not reach their main target represented by people that stopped their education at the first compulsory stage. Distinguishing the post-compulsory schools (vocational and upper secondary) between technical (training schools, technical and commercial institutes), high schools (licei) and others (art schools, teachers' training schools, language schools), a statistically significant (negative) effect emerges from the third type of school if the dummy "high school" (licei) is excluded. If we limit the analysis to publicly provided training, the negative effect is no longer significant at 5% level.

As regards to the final marks obtained in each completed education level, we distinguish four marks at the lower secondary school: pass ("sufficiente") that is the omitted dummy, good ("buono"), very good ("distinto") and excellent ("ottimo")[14]. We then classify the marks obtained at the end of post-compulsory schools (vocational and upper secondary) that range from 60 to 100 in four groups: 60-70 (the omitted dummy), 70-80, 80-90 and 90-100. We observe that high marks generally reduce the probability to later attend training programmes: these courses are probably perceived as complementary to "formal" education and people that got good results at school do not consider necessary to further improve their skills.

Finally, as regards to other events occurred during the education path, nor fails (in every school level) nor dropouts during the upper secondary school (DROPOUT SEC.) have a significant effect on the probability of participating to training programmes. Instead a dropout during university (DROPOUT UNI) significantly increases this probability. This result confirms that training programmes are chosen by medium-skilled people that consider these courses as a mean to acquire a training that they did not receive in the "formal" education system.

According to the estimated propensity scores, it is possible to distinguish different groups of treated and controls satisfying the balancing hypothesis. Table 3a reports, for each estimation, the number of blocks for which the balancing hypothesis is satisfied and the distribution of treated and controls: by construction, observations belonging to the same groups are, as regards to the observable characteristics included in the estimates, completely identical and the participation to a training programme is, for them, a random event.

The average treatment effect on treated, i.e. the average difference between the outcomes of the treated and the outcome of the same individuals if non treated, is then computed following two

[14] The "no answer rate" (table 2) is particularly high for this question. As to take account of this evidence we add a dummy (NA_LOWSEC)

methods[15]. The first one is the *best matching with replacement* (nearest neighbour) where each treated is compared with the control that has the most similar propensity score, that is with the most similar control. The average treatment effect is then represented by the average difference between the outcome of each treated and of its best control. The advantage of this method is clear, since it allows to match each trainee with the control that has the most similar observable characteristics, a sort of "twin". The main disadvantage is that a lot of information is not used (all the controls that are not the "best"), but it could also happen that the "best" control is anyway different from the treated because no restriction is imposed on the distance between the two propensity scores. In the aim of using all the available information and to test for the robustness of the results, we also present the results of a *kernel matching* where each treated is compared to an observation that is the weighted average of all controls The average treatment effect is the average (calculated over the number of the treated) of the differences in the outcomes of the treated and the corresponding "weighted" control. Since each treated is compared to all controls, this method allow to completely exploit the available information.

Table 5a and 5b report the average training effects on trainees (and the bootstrapped standard errors) on the above-described outcomes. Table 5a reports the effect of training programmes in general, while table 5b the effect of public programmes.

As regards to the employment condition, the 144 treated are matched firstly with the 203 "best" controls and secondly with all the 621 controls belonging to the common support. In both estimates, the (negative) average treatment effect is not statistically significant: attending training programmes does not have any effect on the probability of being employed. When only publicly-provided training programmes are examined, the 101 treated are matched with 131 controls with the best matching estimator and with the 630 controls belonging to the common support with the kernel estimator. The results do not change and the average treatment effect is not statistically significant.

As regards to job quality, we report the estimated average training effect on the probability of earning a satisfying wage, where satisfaction is self-defined by the interviewee. The estimated effect is always negative, but this result is not statistically robust. As regards to all training programmes (tab.5a) kernel estimator gives a statistically significant (at 5% level) negative effect, but best matching estimate is not significant. In order to check for these results we adopt two other estimators that are, in terms of exploited information, "midway" from kernel and best matching. *Stratification* estimator compare the outcome of each treated with the outcomes of all controls belonging to the same block, that is with the controls that have the same (in average)

[15] On this see Becker and Ichino, 2002. The reported results are obtained by using the "pscore" ado file developed by Becker and Ichino with Stata.

observed characteristics. *Radius* matching allows to compare the outcome of each treated with the outcome of all controls which propensity score lies within a predefined radius of the propensity score of the treated: in order to increase the comparability of the observations it suffices to impose a littler radius. Both stratification and radius[16] estimates are negative and stratification estimate is significant at a 10% level. The same negative effect of training on wage satisfaction is clear also if we consider only public programmes (table 5b). How to explain this result? Since training increase the human capital level and, as a consequence, the individual productivity we would expect, following the standard microeconomic approach, an increase in wages. A possible explanation relies in a shorter tenure of the trainees, since the participation to a training programme generally delays the entrance in the labour market. This assumption is supported by table 4 that presents the distribution of the duration of the current job distinguishing trainees and controls: while 54% of the controls has a tenure of one year or more, only 40% of the trainees has such a tenure. Later on we will discuss another possible reason to this non expected result that emphasizes the subjective nature of the outcome.

A stronger result emerges if we consider the second job quality indicator, that is self-realization related to the current job. Table 5a reports a statistically significant and positive average training effect on the probability of being "self-realized" as regards to job: having completed a training programme increases by 24% the probability of finding a job that allows the realization of one's hopes. The kernel estimator confirms this positive impact of training programmes, but the estimated effect is littler (18%). If we consider self-realization as an indicator of the conformity of the job to individual expectations and ambitions, the interpretation of this result can be that training increases the probability to have good matches between labour demand and offer. Nevertheless, if we limit the analysis to publicly-provided training programmes the positive estimated effect is not statistically significant (table 5b).

As regards to the third objective, we find a robust positive result. Having completed a training programme increase by 17.7% (15,1% with the kernel estimator) the probability of considering the acquired skills adequate to the current job. This result is confirmed when only public training programmes are considered. The skills developed during a training programme contribute to increase the confidence in one's own competences. This result could in part explain the negative effect of training on wage satisfaction: people that are conscious of their own skills probably expect an higher wage and, as a consequence, are less satisfied about their actual economic condition.

[16] We report the estimates obtained with a radium of 0.001. This reduces the analysed treated and controls to respectively 75 and 160. The result is robust to other radium dimensions.

As regards to the use of informal channels to find the job, the result is controversial. With the best matching estimator a positive but not statistically significant effect on the probability to get a job trough informal channels (family and social networks) is estimated. Nevertheless this result is not robust, since the kernel estimator shows a negative (and statistically significant at 10% level) effect: participating to a training programme would decrease the probability of finding a job trough informal channel by 9.4%. As to test these controversial results we adopt also here stratification and radius estimators: both the estimated average effects are negative and radius estimate is statistically significant at 5%.

The opposite occurs when only public training programmes are considered (table 5b): the average treatment effect estimated with the best matching estimator is positive and statistically significant at 5% but the results of the kernel and of the stratification estimators are not significant and, moreover, the radius estimator gives a negative effect that is statistically significant at 10%.

The obtained results are controversial. Nevertheless, two result clearly emerge: firstly training programmes do not affect employment probability as regards to this young cohort; secondly, they increase the consciousness of the acquired skills and, as a consequence the reliance of the workers in their own capabilities.

Even if propensity score matching is easy to implement and very intuitive, it presents some important problematic features: firstly the selection process has to be correctly defined as to single out all the relevant observables affecting the choice to attend the treatment; secondly, as previous analysis put in evidence, results may vary depending on the estimator and, as a consequence, on the information exploited. The advantage of this approach is that it is not necessary to specify the outcome functions, that are instead exploited within the "control function" methodology.

5.2. Control function approach results

In order to test the robustness of the obtained results and to give some answers to some questions left open we present the results obtained trough the control function methodology briefly presented in par.3. For every outcome we then estimate a probit function where the covariates are represented, following equation (8), by:

a) variables affecting the selection process, that is $\mathbf{X}_i$;
b) variables affecting the outcome ($\mathbf{Z}_i$), that vary according to the outcome and that, in some cases, partially coincide with $\mathbf{X}_i$;

c) the treatment dummy ($\mathbf{T}_i$) where we distinguish the participation to a general training programme from the participation to a public training programme.

Table 6 reports the marginal effect of the probit estimates of the employment probability. The first surprising result is that the estimates are very bad: even if we can check for many education characteristics, "quantitive" (education attainment) and "qualitative" (final marks, failures), our specification explain not much. The only variable that, not surprisingly, has a significant (at 5% level) effect on the probability to be employed is having a technical diploma. As regards to training programmes effects, the result does not differ from the propensity matching estimator confirming that attending a training programme does not improve employment chance for our sample. Table 7 and 8 reports the marginal effects of the probit estimates of the remaining four considered outcomes: in particular, table 7 presents the results when treatment is a general training programme, while table 8 reports the results limited to public training.

For each outcome we add a set of variables representing some job characteristics that are included in the $\mathbf{Z}_i$ vector since are supposed to be relevant to the outcomes.

We classify jobs in four main groups according to the skill that characterized them: blue collars and low-skilled workers (baby sitters, domestic helpers, outworkers) constitute the first group which dummy is excluded in the estimate; skilled blue collars, white collars and teachers form the second group (GROUP2); high qualified workers such as executives, managers, professionals compose the third group (GROUP3); remaining workers such as dealer, craftsman, farmers and others constitute the last group (GROUP4). As regards to the type of jobs we distinguish, as usual, employees and self employed (SELFEMPL), but within employees we distinguish who has a steady job (LONTERM) from who has a short term contract of any kind (so called "lavoratori atipici"). We check also for the macro sector of the job between primary, industrial[17] and service (SERVICE) sector. As regards to earnings, we classify monthly wages in four groups: less than 500 euros (the omitted dummy), from 500 to 1000 euros (INCOME 2), from 1000 to 2000 euros (INCOME 3) and more than 2000 euros (INCOME 4). For all these job characteristics we include, as usual, a dummy representing the "no answer". Finally, we include a dummy that is one when the individual had an unpaid work experience (INTERN).

As for the estimation of the last outcome, that is the use of informal channels to get the job, we include in the analysis others three dummy. In order to catch some household effect we include two dummies that are one if the individual does the same job respectively of his/her father (SAME_F) or of his/her mother (SAME_M): the idea is that informal household channels are more useful to get a job if the children do not stray from the parents' job, since parents can

[17] Since the workers of the primary sector are very few (2%) we put them together with the workers of the industrial sector.

better exploit their relationships to help their children. Finally, in order to catch in some way the effect of a sort of "social capital" that strictly depends on well-established relationships between individuals, we include a dummy that is one if parents come from a different region (OTHREG).
Since we are interested in the result of the dummy representing the treatment (TRAINING in table 7; PUBLIC TRAINING in table 8) we omit the description of all other results that are anyway, for completeness, reported (in bold type marginal effects that are statistically significant at 5% level).
As regards to the effect of general training programmes (table 7), the estimates confirm the results obtained throughout the propensity score matching methodology. Self realization (col. II) and adequacy of the skills (col. III) are positively influenced by the "treatment": the probability to feel self-realized thanks to the job increase by 13% when one attended a training programme, while the probability to consider the acquired skills adequate to the job increase by 18%. The previous controversial result concerning the informal channels to get the job here is clear, since their use decrease by 11%. The only result that remains valid also when we consider public training programmes (table 8) is their positive effect on the perceived adequacy of the skills. We also find a statistically negative effect of the treatment on the probability to observe wage satisfaction (col. I), a result that emerged also previously with the propensity score matching.

6. Conclusions

The purpose of this paper was to study the effect of training programmes on a cohort of youth of the province of Novara. The analysed outcomes were five: employment chance, wage satisfaction, self-realization, skills adequacy and use of informal channels to get the job. Since the analysed programmes include both private (for money and open to everyone) and public (free of charge and partially selective) training programmes, we did for each outcome two estimates: one for training programmes in general and one for public programmes only.
The strong assumption of the analysis was that selection bias, occurring in non experimental studies, was due to observable characteristics, since the nature of the data-set allows to strongly reduce the selection on unobservables. Given this assumption, we estimated the average treatment effect on treated through the propensity score matching estimator and, in order to test the robustness of the results, through the control function approach. Both these methodologies give unbiased results when selection is on observables.
The robust results are two. Firstly, employment chances are not affected by the participation to training programmes, also if we limit our analysis to public programmes that often aim at

increasing employment chances of "weak" workers (female, unskilled, etc.). The control function approach, that exploit the outcome regression, also reveal that covariates that usually affect employment chances (for individuals sharing the same economic environment) such as educational attainments do not explain employment outcomes of our sample. In this context, the result obtained from training programmes that represent, like formal education, a way to improve individual skills, is less surprising. The second robust result is that training programmes increase the confidence of workers on the adequacy of their skills to their jobs. This result indirectly confirm the utility of training, since it offers labour-oriented skills, that often are not acquired in the formal education.

We obtained some other interesting results that, however, are less robust for two reasons: either they are not confirmed when the analysis is restricted to public programmes, either different propensity score matching strategies lead to partially different estimates. Firstly, training increases the chances to get a job favouring self-realization. This positive effect, that is robust to control function approach, is confirmed also in the restricted sample, but it is not statistically significant. Secondly, general training reduces the probability to use informal channels such as household or social networks to find a job. This result, that is statistically significant at 5% level only with radius matching, is confirmed in the estimation of the outcome regression of the control function approach but when we restricted the analysis to public programmes the effect change of sign. Finally, also the negative effect of training on wage satisfaction strongly depends on the chosen estimation approach and on the sample.

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Appendix

Table 1 Characteristics of treated and controls

	Treated		Controls	
Personal information	Obs.	%	Obs.	%
Female	71	49.31	281	42.58
Born in 1982	83	57.64	314	47.58
Resident in Novara	50	34.72	172	26.06
Highest education attainment				
Compulsory school	35	24.31	218	33.03
Vocational school	21	14.58	68	10.3
Upper sec. school	88	61.11	370	56.06
University diploma			4	0.61
Lower secondary school final mark				
Sufficiente (Pass)	48	33.33	206	31.21
Buono (Good)	43	29.86	175	26.52
Distinto (Very good)	1	0.69	64	9.7
Ottimo (Excellent)	1	0.69	26	3.94
No answer	48	33.33	187	28.33
High or vocational school				
Technical/Commercial	87	79.82	293	66.29
Liceo	17	15.6	79	17.87
Others	5	4.59	48	10.86
No answer	0	0	22	4.98
Upper secondary school final mark				
60-70	38	43.18	145	38.77
70-80	32	36.36	79	21.12
80-90	3	3.41	67	17.91
90-100	7	7.95	36	9.63
No answer	8	9.09	47	12.57
Dropouts				
During upper sec. school	18	12.5	134	20.3
During university	18	12.5	22	3.33
Fails				
One or more	59	40.97	297	45
No answer	4	2.78	25	3.49

Table 2 Outcomes by treated and controls

Outcomes		Treated		Controls	
		Obs.		Obs.	
Employment condition	Employed or in CIG/Mobility	122	84.72%	551	83.48%
	Unemployed or in search of first employment	22	15.28%	109	16.52%
Total		*144*	*100.00%*	*660*	*100.00%*
Satisfied for the wage	Much or enough	79	64.75%	395	71.69%
	Not very much or not at all	43	35.25%	156	28.31%
Total		*122*	*100.00%*	*551*	*100.00%*
Selfrealization	Much or enough	82	67.21%	304	55.17%
	Not very much or not at all	40	32.79%	247	44.83%
Total		*122*	*100.00%*	*551*	*100.00%*
Adequacy of the acquired skills	Much or enough	106	86.89%	357	64.79%
	Not very much or not at all	16	13.11%	194	35.20%
Total		*122*	*100.00%*	*551*	*100.00%*
Use of informal channels	Yes	40	32.79%	261	47.37%
	No	82	67.21%	290	52.63%
Total		*122*	*100.00%*	*551*	*100.00%*

Table 3 Logit estimates of the probability to attend a training programme (t value in parenthesis)

	I	II	III	IV
FEM	.1216 (.216)	-.147 (.250)	.2946 (.242)	.1453 (.273)
TOWN	**.6744** **(.230)**	**.625** **(.268)**	**.5181** **(.257)**	.347 (.302)
1982	**.5934** **(.204)**	**.962** **(244)**	**.4989** **(.225)**	**.8642** **(.264)**
VOCAT. SCH.	.228 (.487)	.178 (.564)	.5153 (.566)	.1302 (.661)
UPP.SEC.SCH.	-.0363 (.442)	.0414 (.514)	.323 (.525)	.0202 (.621)
GOOD	.1142 (.262)	-.3463 (.322)	.2181 (.288)	-.0877 (.340)
VERY GOOD	**-3.086** **(1.057)**	**-3.066** **(1.064)**	**-3.270** **(1.102)**	**-3.131** **(1.101)**
EXCELLENT	-2.122 (1.135)	-1.996 (1.123)	-2.343 (1.253)	-1.878 (1.237)
NA_LOWSEC	.3562 (.267)	.337 (.296)	.1632 (.297)	.2258 (.326)
70-80	.3675 (.305)	.3594 (.346)	.1365 (.337)	.2088 (.383)
80-90	**-2.205** **(.697)**	**-1.689** **(.692)**	**-2.399** **(.830)**	**-1.845** **(.815)**
90-100	.379 (.524)	.6886 (.542)	.8792 (.620)	1.142 (.641)
NA_HIGHSEC	-.7158 (.457)	**-1.228** **(.597)**	-.5496 (.482)	-1.007 (.620)
TECHNICAL	.4167 (.328)	.482 (.377)	.5074 (.377)	.7594 (.462)
OTHERS	**-1.325** **(.600)**	-.8146 (.619)	**-1.168** **(.682)**	-.5421 (.731)
FAIL	-.3867 (.244)	**-.5933** **(.280)**	-.3644 (.268)	-.5249 (.303)
DROPOUT SEC.	-.2092 (.387)	-.2694 (.461)	.1397 (.452)	.0912 (.505)
DROPOUT UNI.	**1.735** **(.423)**	**1.132** **(.496)**	**1.599** **(.459)**	.9143 (.556)
CONS.	**-2.099** **(.362)**	**-2.371** **(.413)**	**-2.378** **(.425)**	**-2.568** **(.466)**
Observations	804	761	673	638
Pseudo R^2	0.1316	0.13	0.1392	0.1222

In bold type: statistical significance at 5%

I: Prob (T_i=1) where T_i is a general training programme. Estimates over the whole sample.
II: Prob (T_i =1) where T_i is a public training programme. Estimates over the whole sample.
III: Prob (T_i =1) where T_i is a general training programme. Estimates over the sample of the employed.
IV: Prob (T_i =1) where T_i is a public training programme. Estimates over the sample of the employed.

Table 3a Propensity scores blocks satisfying the balancing hypothesis

	I		II		III		IV	
Block	Controls	Treated	Controls	Treated	Controls	Treated	Controls	Treated
1	285	21	401	24	308	27	207	10
2	126	23	161	35	175	56	188	27
3	80	29	64	39	35	28	119	25
4	125	55	4	3	4	11	14	25
5	5	16						
Total	621	144	630	101	522	122	528	87

Table4 Job tenure by treated and controls

	Treated		**Controls**	
	Obs.	**%**	**Obs.**	**%**
Less than 3 months	11	9.02	23	4.17
From 4 to 6 months	12	9.84	42	7.62
From 7 to 12 months	2	1.64	46	8.35
From 13 to 24 months	12	9.84	59	10.71
More than 2 years	39	31.97	240	43.56
No answer	46	37.7	141	25.59
Total	122	100.00	551	100.00

Table 5a Average treatment effect on treated (treatment: general training programme)

Matching strategy	**Treated**	**Controls**	**Average effect**	**Std. Err.**
		Employment		
Best matching	144	203	-0.039	0.055
Kernel	144	621	0.005	0.038
		Wage satisfaction		
Best matching	122	147	-0.069	0.069
Kernel	122	522	**-0.096**	**0.044**
Stratification	122	522	-0.085	0.045
Radius*	75	160	-0.053	0.079
		Self-realization		
Best matching	122	147	**0.239**	**0.068**
Kernel	122	522	**0.184**	**0.042**
		Adequacy of the skills		
Best matching	122	147	**0.177**	**0.051**
Kernel	122	522	**0.151**	**0.037**
		Use of informal channels		
Best matching	122	147	0.020	0.072
Kernel	122	147	-0.094	0.059
Stratification	122	522	-0.088	0.049
Radius*	75	160	**-0.205**	**0.095**

Table 5b Average treatment effect on treated (treatment: publicly-provided training programme)

Matching strategy	**Treated**	**Controls**	**Average effect**	**Std. Err.**
		Employment		
Best matching	101	133	0.024	0.067
Kernel	101	630	0.028	0.042
		Wage satisfaction		
Best matching	87	118	-0.072	0.087
Kernel	87	528	-0.090	0.064
		Selfrealization		
Best matching	87	118	0.108	0.078
Kernel	87	528	0.095	0.059
		Adequacy of the skills		
Best matching	87	118	**0.195**	**0.087**
Kernel	87	528	**0.159**	**0.046**
		Use of informal channels		
Best matching	87	118	**0.162**	**0.077**
Kernel	87	528	0.005	0.064
Stratification	87	528	0.053	0.055
Radius*	87	528	-0.092	0.059

In bold type: statistical significance at 5%
* the size of the radius is 0.001

Table 6 Results of probit estimates (marginal effects; t values in parentheses)

	I	**II**
FEM	-.0171 (.0275)	-.0133 (.028)
TOWN	-.0285 (.0318)	-.0402 (.033)
1982	.0081 (.026)	.0154 (.026)
VOCAT. SCH.	.0683 (.053)	.0362 (.063)
UPP.SEC.SCH.	-.0467 (.051)	-.0795 (.052)
GOOD	-.0162 (.035)	-.0271 (.037)
VERY GOOD	.0753 (.0423)	.0768 (.041)
EXCELLENT	.0532 (.060)	.0601 (.056)
NA_LOWSEC	.0015 (.037)	.0097 (.038)
70-80	-.0162 (.045)	-.0097 (.045)
80-90	-.0043 (.055)	.0121 (.052)
90-100	.1002 (.080)	-.0874 (.078)
NA_HIGHSEC	-.0747 (.068)	-.0866 (.072)
TECHNICAL	**.1015** **(.037)**	**.1083** **(.037)**
OTHERS	.0042 (.056)	.0245 (.052)
FAIL	.005 (.032)	.0077 (.032)
DROPOUT SEC.	.0113 (.045)	.0065 (.047)
DROPOUT UNI.	-.0753 (.071)	-.1538 (.089)
TRAINING	.0168 (.033)	
PUBLIC TRAINING		.0321 (.037)
Observations	804	761
Pseudo R^2	0.0483	0.0575

In bold type: statistical significance at 5%

Table 7 Results from a probit regression (marginal effects; t values in parentheses)

	Wage satisfaction	**Self-realization**	**Adequacy**	**Informal channels**
	I	**II**	**III**	**IV**
FEM	-.0465 (.046)	**-.118 (.051)**	.0420 (.041)	-.0569 (.044)
TOWN	-.01 (.047)	.008 (.052)	**-.0989 (.047)**	.0909 (.049)
1982	.0535 (.039)	-.0237 (.044)	.0145 (.038)	.0474 (.040)
VOCAT. SCH.	.0846 (.09)	-.1339 (.124)	**.3111 (.030)**	.0736 (.103)
UPP.SEC.SCH.	**-.2198 (.087)**	**-.5601 (.082)**	**.5290 (.075)**	-.0350 (.0913)
GOOD	**-.1439 (.059)**	-.073 (.061)	.0468 (.05)	.0262 (.056)
VERY GOOD	-.0698 (.091)	**.1559 (.079)**	**.1403 (.063)**	-.0625 (.085)
EXCELLENT	**.2221 (.054)**	.1125 (.132)	-.0914 (.149)	.1731 (.136)
NA_LOWSEC	-.0422 (.059)	**.1640 (.059)**	-.1462 (.057)	-.0459 (.057)
70-80	.0513 (.058)	**.1902 (.065)**	-.1164 (.08)	**-.1595 (.065)**
80-90	-.0686 (.089)	.0001 (.092)	.0524 (.084)	**-.1683 (.079)**
90-100	-.0264 (.108)	.1668 (.099)	**-.2816 (.128)**	**.2541 (.100)**
NA_HIGHSEC	**.2186 (.039)**	.0499 (.099)	-.1095 (.103)	.0947 (.097)
TECHNICAL	.0231 (.066)	**.1508 (.076)**	.0556 (.065)	-.0064 (.067)
OTHERS	-.0250 (.101)	.0807 (.107)	**-.2753 (.113)**	.1108 (.104)
FAIL	.0088 (.047)	.0740 (.054)	.0125 (.047)	.0057 (.049)
DROPOUT SEC.	.0174 (.076)	**-.2149 (.085)**	**.1310 (.049)**	.0986 (.073)
DROPOUT UNI.	.0581 (.087)	.0587 (.103)	.099 (.100)	-.1720 (.1)
GROUP 2	**.1354 (.045)**	**.2524 (.049)**		
GROUP 3	-.299 (.223)	.0179 (.166)		
GROUP 4	**-.1973 (.088)**	-.0022 (.088)		
LONGTERM	.0771 (.046)	.0359 (.052)		
SELF EMPL	**.1997 (.0522)**	**.3064 (.088)**		
NA_JOBTYPE	**-.2408 (.101)**	**-.4745 (.073)**		
SERVICE	**.2164**	**.2602**		

	(.047)	**(.053)**		
NA_SECTOR	-.1438 (.102)	-.1656 (.106)		
INCOME2	**.1419** **(.070)**	**-.2007** **(.082)**		
INCOME3	**.2801** **(.036)**	-.0384 (.108)		
NA_INCOME	**.1334** **(.064)**	.0392 (.092)		
INTERN	-.0242 (.048)			-.0299 (.050)
SAME_F				.0318 (.051)
SAME_M				.0559 (.055)
OTHREG				-.0381 (.042)
TRAINING	-.081 (.059)	**.128** **(.055)**	**.1844** **(.040)**	**-.1196** **(.055)**
OBS.	673	673	673	673
PSEUDO R^2	0.2673	0.2210	0.2475	0.0751

In bold type: statistical significance at 5%

Table 8 Results from a probit regression (marginal effects; t values in parentheses)

	Wage satisfaction	**Self-realization**	**Adequacy**	**Informal channels**
	I	**II**	**III**	**IV**
FEM	-.0453 (.048)	**-.1571 (.054)**	.0541 (.043	-.0367 (.045)
TOWN	-.0301 (.049)	-.0252 (.055)	**-.1032 (.049)**	.0831 (.050)
1982	.0423 (.041)	.0166 (.046)	.0023 (.040)	.0395 (.042)
VOCAT. SCH.	.0663 (.094)	-.1258 (.126)	**.3238 (.032)**	.0443 (.106)
UPP.SEC.SCH.	**-.2524 (.088)**	**-.5633 (.085)**	**.5464 (.0769)**	-.0910 (.0943)
GOOD	**-.1365 (.061)**	**-.1573 (.064)**	.0440 (.052)	.0311 (.058)
VERY GOOD	-.0589 (.091)	.1219 (.087)	**.1460 (.067)**	-.0538 (.087)
EXCELLENT	**.2219 (.053)**	.064 (.144)	-.0936 (.152)	.1871 (.133)
NA_LOWSEC	-.0289 (.060)	**.1954 (.061)**	**-.1337 (.058)**	-.0654 (.058)
70-80	.0839 (.055)	**.1697 (.071)**	-.115 (.083)	-.1357 (.069)
80-90	-.0727 (.091)	.0210 (.094)	.0465 (.090)	**.2414 (.100)**
90-100	-.0093 (.107)	**.2027 (.098)**	**-.2901 (.129)**	**.2395 (.100)**
NA_HIGHSEC	**.1907 (.049)**	-.0909 (.116)	-.1482 (.11)	.1661 (.098)
TECHNICAL	.0508 (.067)	**.1753 (.079)**	.0727 (.068)	.0231 (.070)
OTHERS	.0053 (.096)	.0827 (.112)	**-.2695 (.114)**	.1364 (.103)
FAIL	-.005 (.048)	.0711) (.057	.0028 (.05)	.0121 (.051)
DROPOUT SEC.	.0437 (.073)	**-.1996 (.087)**	**.1514 (.051)**	.1032 (.074)
DROPOUT UNI.	**.1797 (.064)**	-.066 (.125)	.0845 (.116)	-.1324 (.117)
GROUP 2	**.1451 (.046)**	**.2518 (.051)**		
GROUP 3	-.2695 (.237)	-.1749 (.203)		
GROUP 4	-.1611 (.088)	-.0391 (.091)		
LONGTERM	.0644 (.047)	.0506 (.054)		
SELF EMPL	**.1860 (.059)**	**.3557 (.078)**		
NA_JOBTYPE	**-.2535 (.103)**	**-.4286 (.080)**		

SERVICE	**.2239 (.050)**	**.3377 (.056)**		
NA_SECTOR	-.1926 (.109)	-.1596 (.111)		
INCOME2	.1069 (.071)	**-.1790 (.085)**		
INCOME3	**.2661 (.040)**	-.0129 (.111)		
NA_INCOME	**.1420 (.064)**	.0741 (.094)		
INTERN	-.040 (.049)		-.0110 (.050)	-.0217 (.051)
SAME_F				.0130 (.054)
SAME_M				.0691 (.057)
OTHREG				-.0341 (.043)
PUBLIC TRAIN	**-.1655 (.069)**	.0055 (.067)	**.1875 (.0451)**	-.0513 (.064)
OBS.	638	673	673	673
PSEUDO R^2	0.2659	0.2314	0.2419	0.0659

In bold type: statistical significance at 5%