

A Successful Entrance Contract for the Youngs? Revisiting the Italian Training on the Job Contract[§].

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Abstract. The “Contratto Formazione e Lavoro” (CFL - training on the job contract) is a fixed term contract aiming at reducing young unemployment and at training young Italian workers. CFL was introduced 20 years ago, in a period of severe unemployment, and provided incentive for employers to hire young people under the program through a drastic reduction of the payroll tax and firing costs.

Using the introduction of the CFL contract as an exogenous innovation and exploiting the reform of the program over time the effect of CFL on the Italian labour market is discussed and evaluated. Difference in difference technique together with matching method is used in order to establish if CFL has improved Italian young workers hiring chances.

Our estimates support a positive CFL effect on employment at the introduction of the program and provide new evidence against previous results leading in the opposite direction. At the beginning of the nineties, the program reform with the drastic reduction of the relative fiscal benefits, reduced non-artisan firms probability to enter the CFL program, so that the overall result appears less clear cut.

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JEL classification: J38, J62, J64.

1. Introduction

Almost 22 million people are unemployed in Europe, 13 million of them in the eurozone, according to the annual Employment Outlook published last June by the OECD. Despite the magnitude of the problem, its solution seems far away and many of us share the recent sceptical view expressed by Blanchard (2006), that we know a good deal less about the causes of European unemployment than we think we do.

A high tax wedge and the protective labour legislation are frequently pointed out as one of the main culprits of this critical labour market situation. The last OECD report, Taxing Wages, (2006) claims that reducing the tax wedge on low-wage jobs helps create employment if it does not result in cuts to public services, and there is some consensus in the labour market literature

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that excessive employment protection can lead to high unemployment among certain groups, including the young¹.

Although this consideration does not imply that the selective removal of the employment protection for a single age group will prove beneficial to the general level of employment (Blanchard, Laidler, 2002), the government of many European countries, Italy and France among the largest, have dedicated much effort in making more flexible the entrance into the labour market through specific labour contracts, fiscal provisions to the firms and so on. Both countries have introduced very flexible forms of labour hirings (basically the labor on call and a much extended apprentice contract in Italy, the *Contrat Premier Emploi* and the *Contrat de Deuxième Emploi* and so on in France) and such measures have been at the centre stage of the political debate during the recent election campaign in Italy and have literally inflamed Paris in the same months.

With this problems on the back of our mind we go back to the Italian experience of the mid eighties of challenging the problem of youth unemployment through a new entrance contract, the *Contratto di formazione e lavoro* (CFL – Training on the Job Contract) that proved a successful move. At mid nineties the European Commission, on the ground of unfair competition, declared the CFL illegitimate (Contini, Revelli, 2004).

The CFL was conceived in the early eighties, with a labour marked drastically deteriorated with the number of unemployed that rose from around 300.000 to 800.000 from 1979 to 1985 while the employment in industry sharply declined. The unemployment increase was accompanied by a deep segmentation in the labour market, with severe unemployment among the young and in the South. To improve the youth chances to get a job, an active labour market policy was devised and great emphasis was placed on a new instrument, the CFL, introduced at the very end of 1984.

CFL declared goals were to reduce youth unemployment, upgrade young workers' human capital and provide stable employment to people entering the program. Eligible people were workers under 30 years of age (with some minor exception). In order to spread the new contract the policy maker granted to the firm adhering to the program almost full payroll tax rebate, exemption from firing costs relative to those the firms would bear by recruiting workers of the same age under the available alternative², the open end contract, and offered the workers an off-the-job training program. The firm was also allowed to hire the worker in a contract category

¹ this consideration does not imply that the selective removal of the employment protection for a single age group will prove beneficial to the general level of employment.

² The worker hired with CFL cannot be fired without a proper motivation (*giusta causa*); exemption from firing costs refer, of course, to the fixed duration of the contract at 24 months, relatively to the available alternative, the open-end contract.

inferior (two steps inferior) to the one that would be prescribed by the worker qualification, according to the national contract.

Using the introduction of the CFL contract as an exogenous innovation and exploiting the reform of the program over time (e.g. artisan firms were guaranteed higher fiscal benefits for a longer period in respect to non artisans) the paper evaluates the CFL impact on young people employment prospects in two Italian highly developed and industrialized provinces, Treviso and Vicenza. In this paper the evaluation is limited to the increase of young people employment, although we are well aware of the various implications of the contract on workers human capital, and of possible effects for other workers that can take advantage from a flexible buffer made by temporary hired young workers³.

Employment increase was the main goal of the program and was expected as the result of changes in the firm's structure induced by the program. The fiscal benefit would reduce, *ceteris paribus*, the average cost and the marginal cost for the firm entering the program and this would induce an employment increase of the eligibles both because of the recovered profitability of setting up a new enterprise and/or of augmenting employment by the existing firms and because of a possible substitution effect among factors of productions, favourable to the eligibles. In the case of substitution, the most easily displaceable candidates were, of course, workers with similar characteristics but just out of the eligibility boundary, i.e. workers with 30 years of age or over.

The present study is based on a panel derived from the social security data-set built at the University of Venice, VWH (Veneto Worker Histories) from the Social Security files⁴. VWH data span from 1975 to 1997 and include register-based information on all establishments and employees that have been hired by those establishments for at least one day during the period of observation, independent of the workers place of residence.⁵ The unit of observation is the employer-day and such information is used to build a monthly history of the working life of each employee. VWH contains information on all participants in the CFL program in the private sector, in the two provinces of Treviso and Vicenza, for the years 1975-1997. The territorial choice has been constrained by data availability, but the two provinces considered looks an

³ The CFL might segment the labour market similarly to what happened with the fixed term contracts diffusion in recent years. For example Boeri, 1999; Blanchard/Landier, 2002. In such a situation one could argue that better educated workers or generally speaking workers with higher productivity will be offered open-end contracts because they have a lower probability of being fired and a higher probability of quitting the firm if offered a CFL, that is a fixed-term contract. See Cipollone and Guelfi, 2005.

⁴ This was made possible by the courtesy and skill of dott. Antonino Travia and signor Francesco Morabito of the INPS.

⁵ The entire working life for all employees that have worked at least one day in Treviso and Vicenza, has been reconstructed, considering the occupational spells out of Treviso and Vicenza as well.

interesting case study, because of the wide diffusion of the contract, partly a consequence of the prevalence of small size firms that adhered in large number to the program.

Our estimates support a positive CFL effect on young people employment at the introduction of the program. The subsequent 1988 and 1991 reforms halved twice the fiscal benefit for non-artisan firms and curtailed their propensity to enter the program, so that the overall result appears less clear-cut. Our study provides fresh evidence against previous studies bearing no evidence of a significant positive policy effect for this program (Contini, Cornaglia, Malpede, Rettore, 2002) and reinforces the idea of the usefulness of an entrance contract supported by fiscal benefits to the firm and precise guarantees for the worker.

The paper is structured as follows: section 2 describes the CFL program, its structure and the relevant reforms through time. Section 3 deals with the estimation of the impact effect of the program at its introduction. The following section exploits the variation in the eligible category over time that was introduced by the reforms, and discusses if the firms that have already hired CFL workers are likely to revise their past decisions and if the program revision leads to a consistent reduction of new hirings. Section 5 concludes.

2. The CFL program

CFL is a fixed term contract introduced by D.L. n.726, 30.10.1984 in order to ease young workers entrance into the labour market. The program addressed to public and private firms that hired young people between 15 and 29 years of age on the basis of a training project (training timing and pattern) endorsed by the Regional Commission for Employment and that had no record of massive firings at the application date.

Several benefits and provisions supported the CFL target. First of all a reduction to a couple of Euros of the Social Security contributions (5000 lire per week-per capita) paid by Italian firms: a substantial reduction, as the payroll tax in Italy amounted to around 40% of the salary, and was one of the largest taxes of this kind in Europe (Contini, 2000). The program granted a specific off-the job training, so that workers hired under the program were expected to develop a stable and qualified career. The CFL maximum duration of 24 months was another important advantage to the adhering firms⁶: at that time the main method of hiring in Italy was the open-ended contract and CFL, setting a time limit to the hiring, allowed a significant flexibility increase in the personnel management⁷. Entrepreneurs evaluated with favour also the possibility, attached to the CFL, to hire directly the worker, without inquiring the Italian “Ufficio di Collocamento”, which was the usual practice and meant hiring from a pool of declared

⁶ CFL is not renewable with the same training specification: in fact the contract was seldom renewed.

⁷ Fixed term contracts were introduced in 1962, but were very strictly limited (seasonal activities and temporary replacements). CFL was the first widely diffused fixed term contract.

unemployed on the first-in first-out basis. Subsequently by the law 223, 23.07.1991 direct hiring was extended to all Italian firms and one of the advantages of hiring under the program was lost.

At the beginning workers eligible under the CFL program were young people between 15 and 29 years of age⁸. In 1994 the eligibility age range was extended to between 16 and 32 years of age, the CFL was extended to liberal professions and associations (D.L. n.299, 16.05.1994); later on to research centres too⁹. The plateau of the eligibles was extended through time under demand pressure but at the same time the fiscal benefit connected to the contract was progressively reduced: the payroll tax rebate was limited to 50% of the amount due in 1988 and subsequently to 25% (table 1). Since January 1st 1991 firms applying to the program were requested to have hired with a tenure contract, during the two preceding years, at least 50% of the terminated CFL¹⁰. This percentage rose to 60% in 1994 (D.L. n.299, 16.05.1994).

The reasons for the progressive limitation for the incentives attached to CFL were first the precarious conditions of Italian public finances and second the attempt to make the program more selective, encouraging its use by firms with favourable employment prospects and discouraging firms attracted mainly by the fiscal bonus (Contini, Cornaglia, Malpede, Rettore., 2002).

Tab. 1: CFL rebates on total social security contributions by category

Year	South, artisans, high unemployment area	All other firms
1.11.84(D.L. n.726,30.10.1984*)-30.5.88	About 98%	About 98%
30.5.88(D.L. n.173,30.05.1988°)-29.12.90	About 98%	50%
1.1.1991(L.29.12.90 n 407)-...	About 98%	25%

*D.L. converted by Law 863/19.12.84, °D.L. converted by law n.291/26.07.88,

Table 2 presents the probability to transiting to employment for a young worker¹¹. The age range is split into four segments. The youngest workers (16-20) can take advantage of two “entrance contracts”, the CFL and the apprentice contract, whose effect overlaps. The proper “entrance contract” for second and third class is CFL; the 21-24 cohort includes workers that terminated the high school and the army service, if males; the 25-29 cohort includes possible

⁸ 32 years old for the South of Italy and the North-Italian regions with an unemployment rate higher than national average.

⁹ The latter were included by Law n.196, 24.06.1997. The Law n.299,16.05.1994 created two kind of CFL according to the training program envisaged by the contract. CFL with a longer training content extends up to 24 months, CFL with a reduced training to 12 months. The law allowed the rebate to be maintained one year after the contract transformation in case of CFL of the first kind, while in case of CFL of the second kind the rebate was applied only if the contract was transformed. Southern regions continued to enjoy full rebate.

¹⁰ To calculate the percentage of hirings, lay offs and firings “per giusta causa” are not accounted for.

¹¹ In the eighties the structure of labour supply was deeply influenced by the baby boom in the early sixties and the subsequent dramatic decline in the population birth rate, by the rapid increase in school attendance by young people (Canu, Tattara, 2004) and by the growth in average life expectancy.

college educated workers and any case workers with higher skills and work experience. Finally, the 30-32 cohort acts as control.

Table 2. Transition probabilities to employment by age groups.

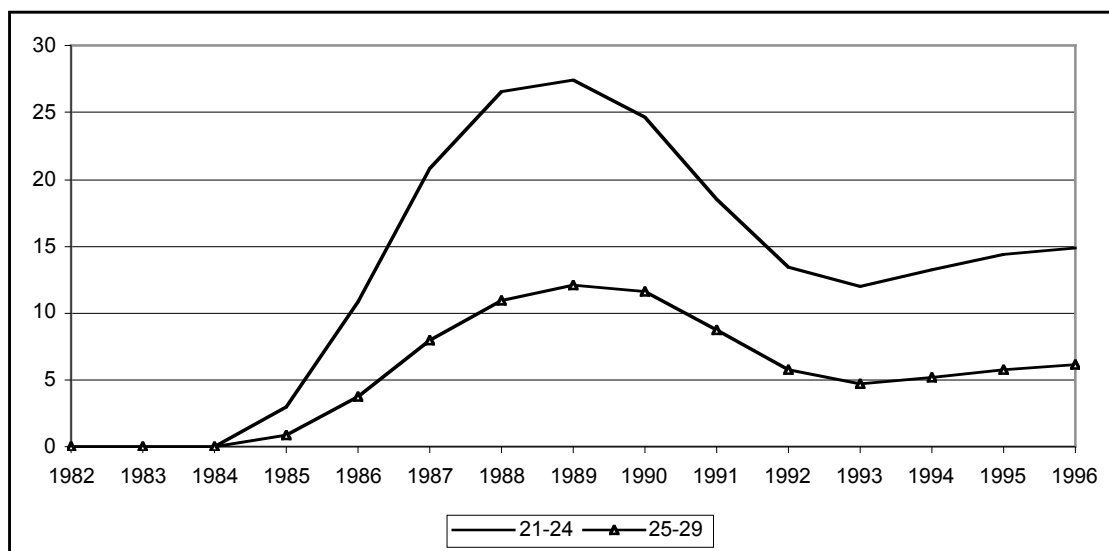
Age cohort	1982	1983	1984	1985	1986	1987	1988	1989
16-20	0,59	0,53	0,59	0,72	0,79	0,85	0,87	0,81
21-24	0,20	0,18	0,19	0,23	0,29	0,32	0,33	0,33
25-29	0,15	0,13	0,14	0,17	0,22	0,25	0,27	0,28
30-32	0,07	0,06	0,07	0,09	0,11	0,12	0,14	0,14

Entrance probability is computed as the ratio between associations (from VWH) to active population (interpolated from the Population censuses of 1981 and 1991 according to the employment rate and marginally adjusted to fit the proper age classes).

The transition probabilities for the four age cohorts are very different in absolute numbers, reflecting the progressive decline of first entrance into the labour-force as individuals get older.

In the pre-treatment years (1982-84) the dynamic of the four age cohorts was rather similar. In the first years of the treatment period (1985-1986-1987) the cohort of the eligibles (16-29 age old) shows a positive increase in the probability to transit to employment superior to that of the other cohorts, that reinforces in 1986 and 1987 for the eligible cohorts 21-29 (+53% in comparison to +13% for the 16-20 and to +43% for the 30-32 age class). The age class 16-20 has a pattern in its own, residual of the Italian young decision to attend in large number the high school and is not of much interest here.

Fig. 1: Share of workers with CFL on employment stock by age.



Source: VWH.

In 1985, at it's launching, the program had an immediate and unquestionable success. For many firms CFL became a common way to hire young people particularly in small firms: on

average about 25% of total hirings for the eligible age cohorts were hired through the program. Figure 1 depicts the share of CFL over the stock of employees in the two more significant age cohorts over time. In Treviso and Vicenza workers hired under the program were, on average, almost 15% of the employee stock in December 1987. CFL workers went on increasing till the first program reform in 1988; the more modest growth that followed (1989) was transformed into a drastic decline by the second reform in December 1990, to turn again into positive in the mid nineties, under pressure by the recovery of the economy.

From such descriptive evidence it is very difficult to assess the extent to which the CFL launching was responsible for the increase in the employment rate for eligible workers¹². A simple comparison between pre treatment and post treatment outcomes might be distorted by other confusing factors such as a temporal trend in the outcome and other parallel events that superimpose to the CFL effect. In order to control for these potential biases we cast the evaluation problem in an econometric framework.

3. The estimation of the CFL impact effect

3.1. The program evaluation strategy. The program evaluation is performed by estimating the possible effect of CFL on employment growth for firms adhering to it. Let Y_{1i} and Y_{0i} be random variables representing the employment stock of the eligibles for firm i that enters (treated) and does not enter (un-treated) the program respectively. Letting D_i be a binary variable assuming value 1 if firm i has been treated and 0 otherwise. Let X_i denote the firm i characteristics, the observables are Y_i, D_i, X_i .

The impact of entering the programme for the firm i is given by $\Delta_i = Y_{1i} - Y_{0i}$, a measure that is not directly observable since each single firm cannot be observed in both the treatment and the non treatment groups at the same time - a missing data problem. What is observable is:

$$Y_i = D_i Y_{1i} + (1 - D_i) Y_{0i} \quad (1)$$

The evaluation literature has mainly concentrated on one of the moments of the $(Y_{1i} - Y_{0i})$ distribution: the Average Treatment Effect on the Treated (*ATT*): $E(Y_1 - Y_0 | D=1)$ or its conditional version $E(Y_1 - Y_0 | D=1, X)$; *ATT* is the average gain from treatment for agents that actually select into the treatment. Estimating this effect requires to make inference about the outcome that would

¹² We set workers in those groups in order to avoid interferences with apprenticeship contract, take into account different Italian school attendance (around 20 year of age high school terminates, and around 25 people graduate), distinguish possible different skills and work experiences and, finally, to account for extension of CFL to older workers.

have been observed for participants if they had not participated (the counterfactual). In experimental design random assignment help in assuring that the two groups, treated and untreated, are equivalent prior to treatment; in non-experimental studies a direct estimate of the counterfactual is not immediately available.

Firms entering the program are non randomly assigned into the treated and untreated status, they are somehow different and it may be this difference, not the program, that leads to a successful outcome for the treated, i.e. to the employment increase: for example the firms entering the program are more prone to growth than the average firm or are bigger in size, thus allowing more people to be hired and to take part in the program.

A possible way out of the problem is to address directly selection trying to equate the two groups on observed characteristics through matching. A counterfactual is constructed by identifying a ‘match’ in the control group for each treated firm. The key assumption in the matching method is the conditional independence assumption (*CIA*) which states that the outcome of non participants, Y_0 , is assumed independent of participation, conditional on a vector of observable characteristics,

$$Y_0 \perp\!\!\!\perp D | X \quad (2)$$

However, when there are many conditioning variables it is impractical to find a proper match. A tractable solution is to match on the propensity score, $p(X)$, which is defined as the conditional probability of receiving a treatment given pre-treatment characteristics:

$$p(X) = (D = 1 | X) \quad (3)$$

Rosenbaum and Rubin (1983) have demonstrated that if *CIA* holds, matching on propensity score bears an unbiased estimate of *ATT*.

$$E(Y_0 | D = 1, p(X)) = E(Y_0 | D = 0, p(X)) \quad (4)$$

In general after having run logit, probit or semiparametric estimation on pre-treatment variables X , the fitted values, $p(X)$, are used in order to match treated and control units. Following Heckman et al. (1998) the form of the matching estimator can be cast in the following framework

$$\hat{ATT} = \sum_{i \in I} \omega(i) [Q_{1i} - \sum_{j \in C} W(i, j) Q_{0j}] \quad \text{for } X \in \bar{X} \quad (5)$$

where Q_{1i} is function of the treatment outcome, Y_{1i} , and Q_{0i} is function of the comparison group outcome, Y_{0i} ; $W(i, j)$ is a weight, with $\sum_{j \in C} W(i, j) = 1$, which depends on p-score and on the kind of matching used (nearest-neighbours, kernel, ...); $\omega(i)$ is a weight that accounts for heteroschedasticity and scale; \bar{X} is common support of treated and untreated observables:

$$\bar{X} = X_1 \cap X_0.$$

Label period $t=t^2$ the date of the reform and $t=t^1$ the earlier period. Exploiting the panel data information, the *ATT* estimate is obtained by making the difference between matching estimates before and after treatment (Heckman, Ichimura, Todd, 1998). In terms of expression (5) the treatment outcomes are defined as $Q_{1i} = (Y_{1it^2} - Y_{0it^1})$ and $Q_{0j} = (Y_{0jt^2} - Y_{0jt^1})$.

The identification assumption is the usual conditional independence assumption (*CIA*), but on differences:

$$E(Y_{0t^2} - Y_{0t^1} | X, D = 1) = E(Y_{0t^2} - Y_{0t^1} | X, D = 0) \quad (6)$$

i.e. there is no difference in the trend outcome between non participants and participants when there is no treatment. Under additive separability of the errors and if p-score is a good balancing score, the *CIA* condition becomes

$$E(u_{0t^2} - u_{0t^1} | p(X), D = 1) = E(u_{0t^2} - u_{0t^1} | p(X), D = 0) \quad (7)$$

i.e., in absence of treatment, given appropriate controls, there is no difference in the residuals of the trend outcome between non participants and participants.

The estimation strategy assumes the general equilibrium hypothesis, i.e., the program must not affect the control group.¹³ In actuality this assumption looks reasonable. Although CFL in the eighties was a widely diffused practice, a large amount of young workers was available for hiring by the non treated; moreover we assume that CFL did not affect the economic behaviour of not treated firms.

3.2. The data set. In order to estimate the CFL impact on youth employment we use the longitudinal panel VWH, which provides, at monthly intervals, a rich set of information on firms and on employees' characteristics.

¹³ For example using as counterfactual the age group 33-34 would get biased estimates as there might have been a substitution effect between the eligible and the non eligible group.

The outcome variable is the monthly stock of eligible workers for the firms that qualified for the program in 1986. The number of eligible workers is preferred to the number of CFL, as the latter might measure as an impact the hiring as CFL of a previously fired/quitted worker. Eligibles not hired as CFL by the treated firms are adequately controlled through the counterfactual.

Workers are clustered according to the birth date. The eligible workers age spans from 15 to 29 years, but as far as our evaluation estimate is concerned the lower bound of the eligible workers age cohort has been raised to 21 years of age, to get rid of a possible apprentice overlapping effect¹⁴ and has been split into two, 21-24 and 25-29 years of age in order to separate the program influence on the younger workers and on the more skilled workers, with probable college education (fig. 1)¹⁵. The panel excludes firms that are born or have died in the years under consideration, so the possible stock variation due to the natural process of firm's birth and death is ruled out¹⁶.

The model is estimated through *DID*-pscore-matching estimator that identifies the effect of the program as the change that occurred in eligible worker employment in a period of 12 months after the firm entered the program, with respect to the previous 12 months and the analogous measure referred to the matched firms that did not take part in the program. The period of 12 months before and after the entrance into the program is sufficiently long in order not to blur the result with extemporaneous spikes. Table 2 reports the sliding observation window used in the estimate with the respective number of firms: the window is 24 months long and slides month after month from January 1985 to November 1987.

¹⁴ We exclude 15-20 for two reasons: they can exploit apprenticeship program and due to the extension of scholar attendance. In this way we avoid contamination effects.

¹⁵ The two Veneto provinces have almost full employment so that the end of the studies most of the time matches with the entrance into the labour market.

¹⁶ In this way we are building a panel of same firms and same workers, avoiding intertemporal effects caused by firm births-death and by the fact that over time age cohorts are made up by different people.

Table 3. Treated firms and workers eligible cohorts

Period of observation		number of firms entering the program (treated)	
Pre treatment	Treatment	Workers 1961-1964 birth cohort*	Workers 1957- 1960 birth cohort°
85/1-85/12	86/01-86/12	153	49
85/2-86/1	86/02-87/1	87	39
85/3-86/2	86/03-87/2	77	26
85/4-86/3	86/04-87/3	77	27
85/5-86/4	86/05-87/4	88	32
85/6-86/5	86/06-87/5	74	23
85/7-86/6	86/07-87/6	100	33
85/8-86/7	86/08-87/7	30	11
85/9-86/8	86/09-87/8	98	37
85/10-86/9	86/10-87/9	118	53
85/11-86/10	86/11-87/10	109	47
85/12-86/11	86/12-87/11	61	19
Total		1072	396

*Worker age is 21-22 for birth cohort 1964, 22-23 for birth cohort 1963, ... 24-25 for birth cohort 1961.

°Worker age is 25-26 for birth cohort 1960, 26-27 for birth cohort 1959, ... 28-29 for birth cohort 1957.

A firm can enter the two classes. Firms that hire only in a single age cohort cannot enter as a counterfactual for the other age cohort, in order to avoid the occurrence of a possible substitution effect.

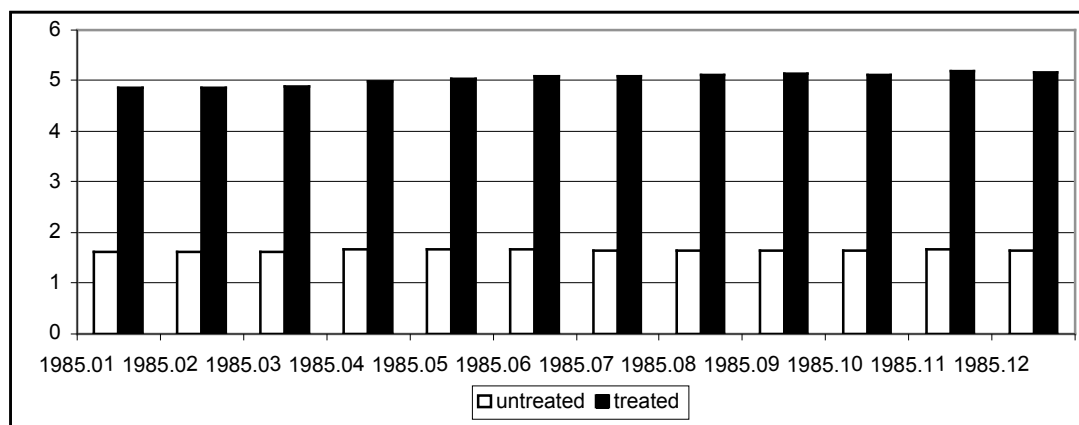
The characters observed in the last month in the pre-treatment observation are used to qualify for the p-score matching.

Treated firms are compared¹⁷ with non treated firms, during both 1985 and 1986 through appropriate control variables. Untreated firms are defined as firms that never entered the CFL program in 1986. Untreated firms are rather heterogeneous as they include firms that applied to the CFL Commission, received approval but did not hire through the contract for several reasons, firms that applied and whose application was rejected and firms that did not apply: the three categories are not separately identifiable. About 50% of applications for CFL were rejected, mainly because the training plan presented by the firms was not considered satisfactory: these firms are the best candidates to our counterfactual experiment as they presumably share many of the characters of treated, as both tried to qualify for the program (although, it may be, with different intensity). Firms that were declared eligible but that did not put into force the contract should in principle be discarded as the reason they have not entered the program is not known (either a change in the firm plans or lack of workers with the required skills) and cannot be controlled for; to our knowledge they are really very few.

¹⁷ CFL started in 1985 but only few firms adhered, thus the program is evaluated since 1986.

3.3. The estimation of the CFL impact effect. Firms self selection into treatment can introduce serious bias in our estimate. Figure 2 substantiates, with clear evidence, the possibility that firms select into treatment. The number of workers of 21-24 years of age (eligible) during the pre-treatment period for treated and non-treated firms is compared and treated firms have many more young workers in the pre-treatment period than non treated firms, i.e. firms that will enter the program employed a larger number of potentially eligible workers in the recent past than non-treated firms (they differed in size and in the stock composition).

Fig. 2: Average number of 21-24 years old worker by firm treatment status.



This evidence requires the construction of a proper counterfactual by identifying a proper ‘match’ in the control group for every treated. As suggested earlier the employment effect of the program is estimated with a two-stage procedure. First, for each firm the intertemporal employment stock variation is computed (pre-treatment and post-treatment difference). Second, the process of matching sets the treated firms and the non-treated firms that are observationally similar in terms of propensity score. The *ATT* estimator (*DID*-pscore-matching estimator) is computed as the difference of the intertemporal outcome difference between the matched treated and untreated firms.

Firms are pooled according to the month in which they adhered to the program and a logit model for each group (e.g. 12 logit) has been estimated¹⁸ on the following “pre-treatment” exogenous explanatory variables (i.e. one month before entering the program)¹⁹: size, sector, industrial area dummies, firm age, number of males, blue-collars (as proxy for capital or labour intensity), apprenticeships (as proxy for the firm inclination to use fixed term contract and training contracts), eligible workers for the CFL and their variation. In order to capture non-linearity, interaction and second order terms are allowed for.

¹⁸ As said before, looking at one particular starting month we estimate one month before (pre-treatment) one p-score for each firm, and this value is constant for all temporal observations of the same firm.

¹⁹ Of course untreated firms are the same across all iterative procedure (14308 untreated firms), but the specific observation used in the logit estimate depends on the starting date we are looking at.

Treated and untreated firms with the p-score as close as possible and which belong to the same month of observation are matched. In such a way seasonality that is particularly strong for young people hiring is dealt by matching firms within the same month and taking the year differences. The calliper nearest-neighbour matching with a tolerance under 1% imposes a common support and excludes less than 5% of the treated population²⁰. Finally the *ATT* estimator (*DID*-pscore-matching estimator) is computed as difference between the weighted averages of the treated-untreated yearly outcome variation. Twelve \hat{ATT}_t are computed, one for each starting month, but for reasons of space only the weighted (according to the observed number of treat firms in a given month) average of the several \hat{ATT}_t is reported in table 6:

$$\hat{ATT} = \sum_t \hat{ATT}_t \cdot P(D_t | D = 1) \quad t: \text{January, ..., December} \quad (8)$$

Yearly variation of eligible employees in the pre-treatment period has been introduced among the explanatory variables in order to face the self-selection problem. Lagged employment in fact forecasts rather well the future behaviour of the treated as far as employment is concerned and is independent from the CFL effect (selection independence)²¹. The autoregressive estimate of the yearly variation of the eligibles is reported in table 4. The variation of the eligibles on its most recent past is estimated by OLS over the period January 1982 - December 1984: the coefficient of the lagged variable is significantly close to 0,95 with a high R^2 .

Tab. 4: Results of AR(1) model for the yearly variation of eligible workers

lagged yearly variation of eligibles (Std. err.)	.9591724 (.0005126)
Adj R-squared	0.9049

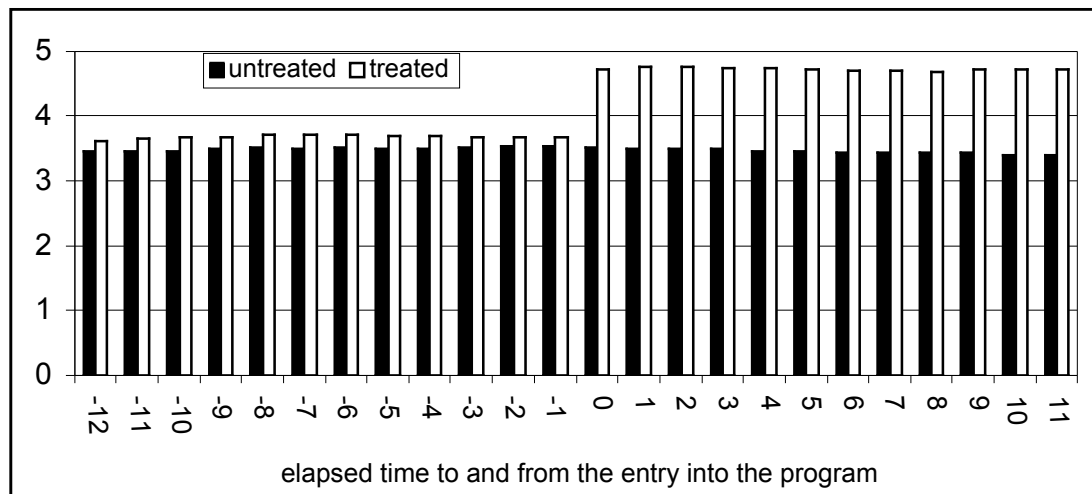
Figure 3 shows the dependent variable (21-24 years old workers) after matching by firm treatment status. The horizontal axes measures the distance in months in relation to the month of entry into the program, that is labelled 0. In the pre-treatment period, once the effect of the observables has been controlled for by p-score matching, the magnitude of the employed eligible

²⁰ In some specification we imposed not only the nearest neighbour but the 5 nearest. In this way the estimate is improved but the variance has also increased. The tolerance and the number of comparison were chosen in order to minimise the outcome difference between the two groups during pre-treatment period.

²¹ The firm dimension variable has to be matched with great care as we do not want to confuse the impact effect with a different size issue. Assume we are comparing two firms, untreated of size 10 and treated of size 100 and posit a positive cycle inducing the two firms to increase their employee stock by 1%; the outcome effect, just due to the cycle, measured as the difference between the stock of the treated and non-treated in the pre-treatment and post-treatment period, is positive, i.e., $110-100-(11-10) = 9$.

workers is similar for treated and un-treated firms (average difference 0.2, almost constant, table 5).

Fig. 3: Number of 21-24 years old worker by firm treatment status, after p-score-matching



In the treatment period the two sets differ significantly and the difference is close to 1.0 (tab. 6). The sharp step in 0 of the treated in figure 3 represents intuitively the treatment effect on the employment of eligible workers once the observed heterogeneity between treated and non treated firms has been controlled for by the p-score matching procedure.

Table 5 gives substance to the previous claim and to figure 3: matching is able to get rid of almost all selection bias, but in the pre-treatment 12 months period some heterogeneity, rather constant over time (about 0.2) is still available, and this kind of effect is dealt successfully by *DID*. Moreover a possible “Anshefelter’s dip” or anticipating effect, is ruled out²² both by table 5 estimates and by the pattern represented in figure 3.

Tab. 5: Pre-treatment differences test on the employment level, in matched treated-untreated firms.

	1985		1986	
	21-24	25-29	21-24	25-29
Matching differences (std. err.)	.1655273 (.0680191)	.2688827 (.1422469)	.2540971 (.0962501)	.2636726 (.1993668)

The CFL introduction, evaluated through the *DID*-p-score-matching, has a quite strong effect in raising employment for the eligible age cohort (tab. 6). On average each treated firm hired one young worker more than not-treated firms: the outcome is very similar for the two age cohorts.

²² The dip typically occurs if firms which have registered - pre-treatment - a decline in the number of their employees are selected so that the subsequent increase might be attributable to the “dip” and not to the program. Table 4 does not evidence any “dip” able to explain the employment increase for the firms entering the program, neither figure 5 shows any decrease in employment by the treated in the months immediately preceding the event. A “dip” would have required a negative pre-treatment difference between treated and untreated.

The effect is to be related to the treated firm average size of 22, i.e. on average firms increase their employment stock by 5%. Table 6 reports the *ATT* estimates from previous equations 5, summarized according to 8.

Tab. 6: Estimated effect of the CFL on the two eligible age cohorts by *DID*-pscore-matching.

	21-24	25-29
\hat{ATT} - p.score matching (bootstrap std. err.)	1.101183 (.0228328)	.9996684 (.0235885)

The lagged yearly variation of the eligibles plays a crucial role in identifying the program impact, and a sensibility analysis can help to understand if firms with different growth rates performed differently.

First of all we calculated for treated firms the distribution of the variable “lagged yearly variation of the eligibles” one month before the entrance into the program, and we exploited the inter-quartiles ranges of this distribution in order to perform a sensibility analysis. Treated firms are classified in firms with: decreasing, constant and growing employment according to their behaviour on month before treatment (quartile -1 , 0 , $+1$: the major part of the treated firms have a growth rate equal to zero, which is also the median).

The dataset is split accordingly in three parts and for each part the matching procedure has been replicated. There is no anticipating effect, and the impact is quite similar over the three groups (table 7). Firms with decreasing employment increased their stock of the eligibles after entering the program of an average additional worker over the pre-treatment average employment stock, and the same happened for firms that previously had constant employment and firms that had increased their employees stock before entering

Tab. 7: Sensibility analysis for lagged yearly variation of the eligibles

	Lagged yearly variation of employment of the eligibles					
	≤ -1		$= 0$		≥ 1	
	21-24	25-29	21-24	25-29	21-24	25-29
\hat{ATT} - p.score matching (bootstrap std. err.)	1.250839 (.065365)	1.077297 (.069879)	.9748806 (.013941)	.9732956 (.017048)	1.28522 (.048300)	.9856224 (.076903)

3.4. The CFL effectiveness in rising employment. The positive result of CFL introduction on young people employment requires addressing at least three questions.

First, substitution. Did the increase in employment for the eligibles crowd out other employment cohorts, so that the overall result was less satisfactory than assumed?

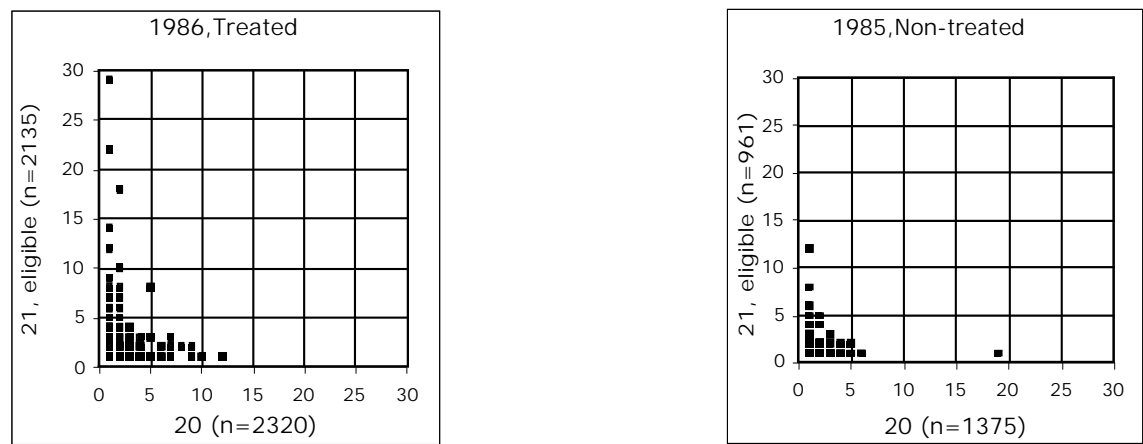
The employment increase in the age cohort 21-29 might be the result of a parallel employment decline of the younger or of the oldest workers. On one side before the CFL introduction, firms' hiring encountered a significant threshold at the worker's age of 20: many of the advantages of the CFL contract were available by hiring an apprentice and this possibility was available to workers less than 20 years old. CFL pushed forward this threshold to 29 years of age and, as a consequence of the barrier removal, we expect that workers with 20 years of age or more – now more desirable to the employers – might substitute younger workers in recruiting²³.

The scatter-plot (fig. 4) shows that, given the same set of firms, considered in a treated and in a non treated status (respectively 1986 and 1985), treated firms turn their hirings towards the age cohorts beyond the apprentice age threshold (fig. 4:a,b,c): they hire relatively more workers of age 20,21,22 years of age relatively to workers of 18, 19 years of age, than non-treated firms. The same figure shows that the displacement does not take place within the age classes beyond the threshold (es. fig.4:d), i.e. substitution is directly linked to the removal of the apprentice age superior boundary.

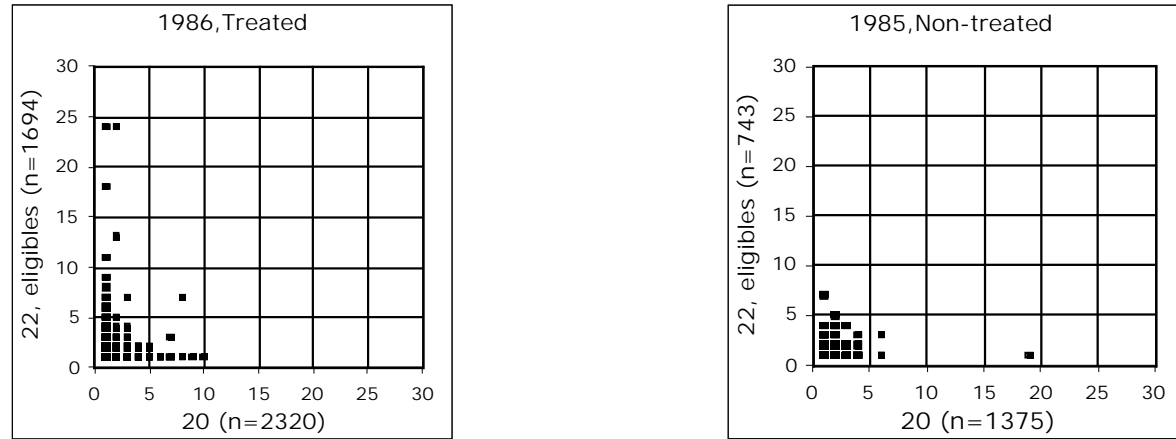
Is there a substitution effect at the upper boundary, at 29 years of age? This is much less likely because the hiring of a more mature worker reflects the choice of precise professional requisites, and these are often specific to a single individual, that is not easily substitutable. The scatter-plots of figure 4 (d, e) confirms that hirings across the upper boundary were very limited and that there was no possible substitution effect (treated and non-treated firms behave similarly).

²³ Substitution is also the result of the fact that firms applying for a CFL contract delayed the proposed hiring till the contract approval and hired in the new eligibility class.

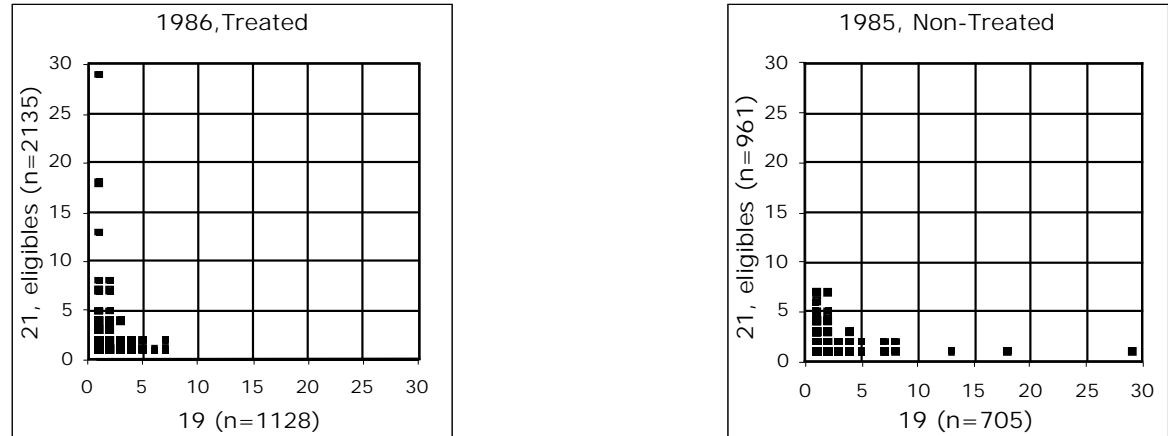
Fig. 4: Scatter plot of hirings by different ages
(a)



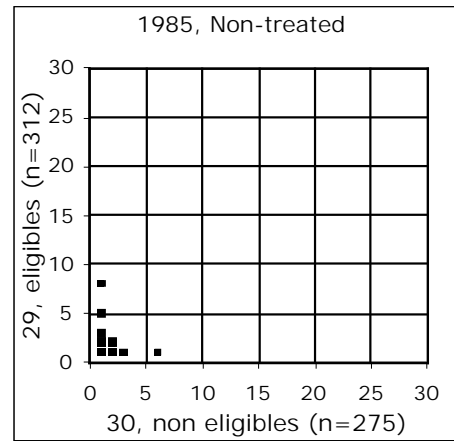
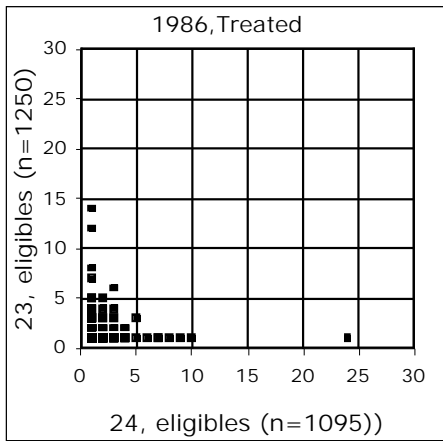
(b)



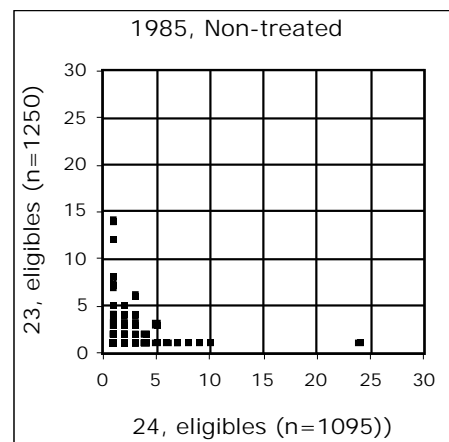
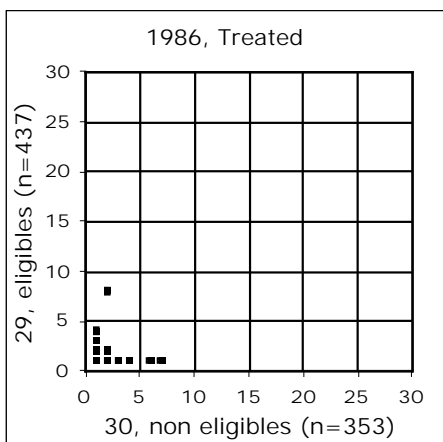
(c)



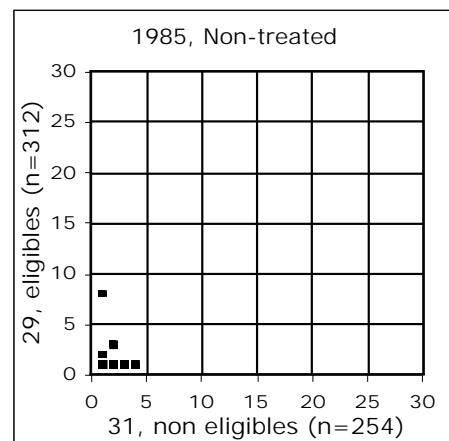
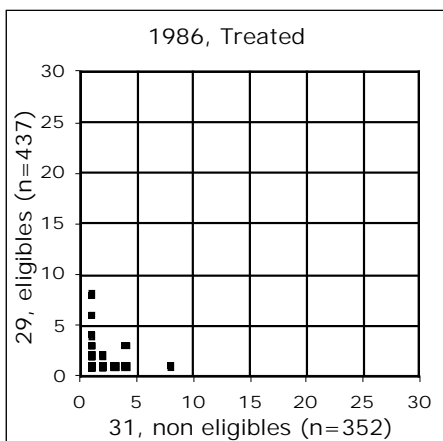
(d)



(e)



(f)



In order to measure the possible substitution effect we compute the average differences of the yearly variation of number of employees for the two adjacent age cohorts (15-20 and 30-32), between matched treated and untreated firms, for the two eligible classes (21-24 and 25-29). The estimated equation reported in table 8 is the average of the monthly ATT defined in eq. (5), where $Q_{1i} = (Y_{1it^2} - Y_{0it^1})$, $Q_{0j} = (Y_{0jt^2} - Y_{0jt^1})$ and Y_{it} is respectively the worker stock for firm i at time t for the two age cohorts 15-20 and the 30-32.

Tab. 8: Estimated Effect of CFL on the 15-20 and 30-32 age cohorts by *DID*-matching

	21-24	21-29
$\hat{ATT}_{15-20 \text{ cohort}}$ (std. err.)	.1576029 (.0226783)	.2682779 (.038752)
$\hat{ATT}_{30-32 \text{ cohort}}$ (std. err.)	-.0024007 (.0112958)	-.0031796 (.0158533)

On average, the variation due to the CFL in 15-20 years old workers is positive, significant, which means that in comparing treated with non-treated firms, the first group hires on average more workers of the cohort 15-20, given the appropriate controls, than the second group (1 more employee every 5 firms of average size). The substitution effect (fig.4, a,b,c) amounts to the fact that treated firms hire less workers in the age class 15-20 relatively to the workers hired in the subsequent age class, but not to a possible drop in relation to the practice of non-treated firms.

The absence of any effect on the 30-32 age cohort confirms the descriptive evidence from fig- 4 d,e.

Second, job changing. If people newly hired under the program, were already employed, does a positive coefficient for the program increase employment? The answer is affirmative, contrary to epidermis appearance, as people reporting separations from their previous job (quitting/firing), ceteris paribus, would be replaced by new hirings, as nothing has changed in the situation faced by the firm that has suffered the workers loss²⁴. Hence the system sooner or later will fill the vacancy caused by the new CFL hiring with an unemployed (that has no connections with the eligibility requisites, of course).

²⁴ Individuals that had a previous employment spell within one month are, on average, less than 30% of total CFL hirings.

A possible employment reduction might be due to the hiring, as CFL, of a redundant worker, i.e. a worker that is not replaced by the entrepreneur. A possibility limited by the fact that CFL are, in large majority, addressed to newcomers and would imply an economic system with declining employment.

Third, absolute relevance. Why, given a similar average effect between the two birth cohorts (table 6) empirically the aggregate effect for the age cohort 24-29 appears much weaker (figure 1) Indeed the total effect depends on two factors: the first - measured by the coefficients reported in table 6 - is the average treatment on the treated; the second is the number of firms entering the program. The first component is almost the same between the two age cohorts, but the number of firms entering the program and hiring people in the age cohort 25-29 is less than half the number of firms hiring people of the 21-24 age cohort (tab. 3); within the eligible age range the employment stock for the oldest workers increased much less than that one for the younger. Treated firms are in large measure small firms that do not demand highly educated workers, but mainly blue collar workers that were hired as soon as they finished the higher school attendance and had passed their army service, if males.

3.5. Unobserved heterogeneity. The analysis of substitution among age cohorts allow to come back to table 5 estimates and face the always-present problem of estimate bias resulting from uncontrolled heterogeneity.

Let us perform a further test in order to check for a possible bias by selection due to unobserved heterogeneity. Assume that the selection process works in the following way: firms more prone to grow self-select into the program. Denote, without loss of generality, this characteristic by a binary variable h : $h = 0$ firms that do not want to increase their stock; $h = 1$ firms that want to increase their stock. If firms self-select into treatment, the correlation between treatment ($D = 1$) and h is very high, i.e. growing firms enter the program.

Denote as ΔY_i^e the hirings of eligible workers by the firm i , and a ΔY_i^{e+} the hirings of not eligible workers of the strictly, non-eligible, adjacent age class (30-32 age cohort). Let us assume

$$E(\Delta Y_i^e | h = 1) > 0 \text{ and } E(\Delta Y_i^{e+} | h = 1) > 0 \text{ and } E(\Delta Y_i^j | h = 1) > E(\Delta Y_i^j | h = 0) \quad j: e, e+ \quad (9)$$

Hence once h is properly accounted for, the number of hirings of adjacent non-eligible workers between treated and untreated firms is the same.

$$E(\Delta Y_i^{e+} | D = 1, h) = E(\Delta Y_i^{e+} | D = 0, h) \quad (10)$$

The above condition can be exploited to test the appropriateness of the *DID*-pscore-matching procedure looking back to the results of table 6. Let us assume the reasonable hypothesis that if growing firms self-select into treatment, this move affects a wide range of age classes. From

empirical evidence treated firms hire many workers other than CFL; for example in 1986, in the age class 16-32, treated firms hired 10.595 CFL, 6561 non CFL (of whom 745 in the age class 30-32).

1. CFL affects the outcome positively and *DID*-matching account for all heterogeneity (no self-selection bias). Difference in outcome between treated and non-treated for the eligibles is positive and for the adjacent age class is negative (a clear index of substitution) or zero. The result of par. 3.4 is that substitution due to CFL hiring very unlikely affects workers in the thirties and the difference in outcome for the adjacent age class is zero (as from table 6).
2. CFL is influent on outcome: the positive differences in outcome between the treated and the non treated for the eligibles is the result of selection and is positive both for the eligibles and for the adjacent age class. A negative or a zero difference for the adjacent age class can result only from substitution. Substitution absent, a zero difference in outcome (table 6), makes self-selection very unlikely.

4. The CFL reforms.

The first CFL reform was introduced in the middle of 1988 and the second at the beginning of 1991. Since June 1988 and since January 1991 the fiscal rebate diminished over the previous level by 50% for non-artisans firms (treated) leaving the complete benefit to artisans (non-treated).

The Average Treatment Effect on the Treated estimates the average employment variation for the eligibles due to the program reforms for the firms that were entitled to the lower fiscal benefit. The dependent variable is the monthly variation of the employee stock for the eligibles, and treated and non-treated are artisan and non-artisan firms, both adhering to the program, hence *DID* is used having artisans as the control group.

The estimation strategy is radically different from the previous one, because treated and non-treated firms (non artisan and artisan firms) running the contract are now “exogenously” determined by the program, and the basic problem is heterogeneity between the treated and the control group, i.e. a problem of diversity, not a problem of selection.

Observable heterogeneity is controlled by the matching procedure. Artisan and non-artisan firms differ primarily in size and sector, and matching loses about half of treated firms. Moreover these two kind of firms are quite different, even after matching, at least in term of their behaviour along the business cycle; such heterogeneity²⁵ needs to be directly faced and

²⁵ Blundell et al. (2003) observe that “for the evaluation to make sense with heterogeneous effects, we must guarantee that the distribution of the relevant observables characteristics is the same in the four cells defined by eligibility and time”. They suggest to use two propensity score: one for eligibility and one for time period.

can be taken into account using the regression-adjusted matching²⁶. Heckman, Ichimura, Todd (1998) combine matching method and regression adjustment on the X : their method extends the classical matching by utilizing information on the functional form of the outcome equations and faces directly the problem of the difference in behaviour in the pre-treatment months.

Regression-adjusted matching is performed by the following procedure (Heckman, Ichimura, Todd, 1998). Assume a conventional econometric model for outcomes in the non-treatment state that is additively separable in the observables X and unobservables U

$$E(Y_0 | X, D = 0) = X\beta_0 + E(U_0 | X, D = 0) \quad (11)$$

before estimating ATT by matching methods (5), $X\hat{\beta}_0$ is removed from Y_0 and Y_1 by setting $Q_{1j} = (Y_{1j} - X_j\hat{\beta}_0)$ and $Q_{0j} = (Y_{0j} - X_j\hat{\beta}_0)$.

In particular to account for the different reaction to the business cycle by the treated and the non-treated, the number of non-CFL hirings is considered. Indeed hirings are strongly procyclical (Tattara-Valentini, 2004), treated (non artisan) and non-treated (artisan) have a different cyclical behaviour through time – previous controls given – and hirings capture the variability that depends on the cycle for the two categories.

Again we combine regression-matching methods with the difference-in-differences method. The ATT estimates for the 25-29 age cohort shows no significant impact, and a slight positive impact for the 21-24 age group²⁷. The following table reports the ATT estimates for previous equations 5, summarized according to 8.

Tab. 8: Estimated effect of the CFL reforms on two eligible age cohorts by DID -regression-adjusted-pscore-matching.

	6.1988-5.1989 (first reform)		1.1991-12.1991 (second reform)	
	21-24	25-29	21-24	25-29
\hat{ATT} (Bootstrap std. err.)	.107756 (.017400)	-.020937 (.029064)	.043564 (.010108)	.025117 (.013646)

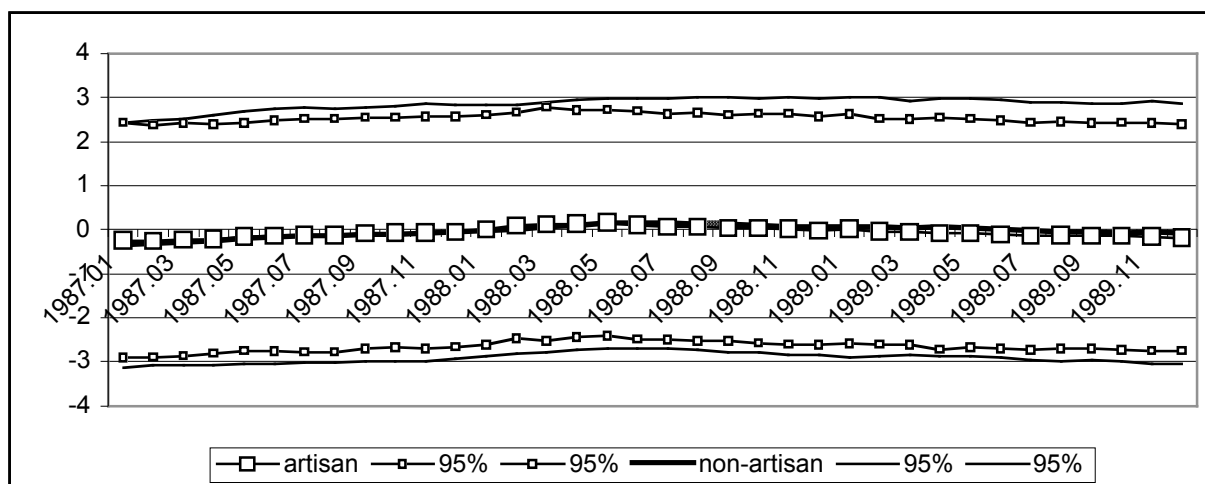
Among firms that were part of the program at the launching of the reform, artisan firms, with full benefit, did not show an employment pattern significantly different from non-artisan firms that received half of the benefit in 1988 and a quarter of it in 1991.

²⁶ We do not use this technique in the previous analysis because treated and untreated firms were well distributed, both classes were rather numerous, there was no a-priori reason to expect a different response to the cycle between the two classes and because the lagged variation of the eligibles did account for the (possible) self-selection.

²⁷ Observables are quite balanced between treated and untreated firms. Results are available on request.

Figure 5 reproduces the estimate process for the age class 21-24. The residuals of the outcome for the treated and the non-treated firms after regression-adjusted matching are computed and plotted: the horizontal axes measures the time, in months, in relation to the beginning of the reform that is labelled 0. In both periods, once observable factors have been properly controlled for, the residuals of the difference is non significantly different from zero (at the 1% significance level).

Fig. 5: Residual of the difference of the outcome after regression matching and 95% confidence intervals



Indeed the transition to the new regime for non artisan firms was gradual through time: the CFL benefit continued to maturity and at maturity several contracts were transformed into open-end contracts that were entitled an additional year benefit, according to the reform. But although gradual, the benefit reduction was substantial and would show up eventually in the 12 months following the reform introduction and hence should affect table 9 estimates²⁸.

Given that the reaction to treatment was significantly positive few years before (benefit allowance) why is now (benefit reduction) negligible? What intervening factors have determined the continuance of the same level of employment for the treated, although faced with a much reduced benefit? A possible answer is that once the firm has entered the program, the firm's structure has adapted to the new situation (production volume, market position etc) and, the benefit reduced, the entrepreneurs are faced with the choice of confirming the CFL worker as soon as the contract comes to maturity and/or shifting to a new worker but are unwilling to reduce the employment level, *ceteris paribus*. So treated and untreated do not behave differently as far as the employment level of the eligibles is concerned.

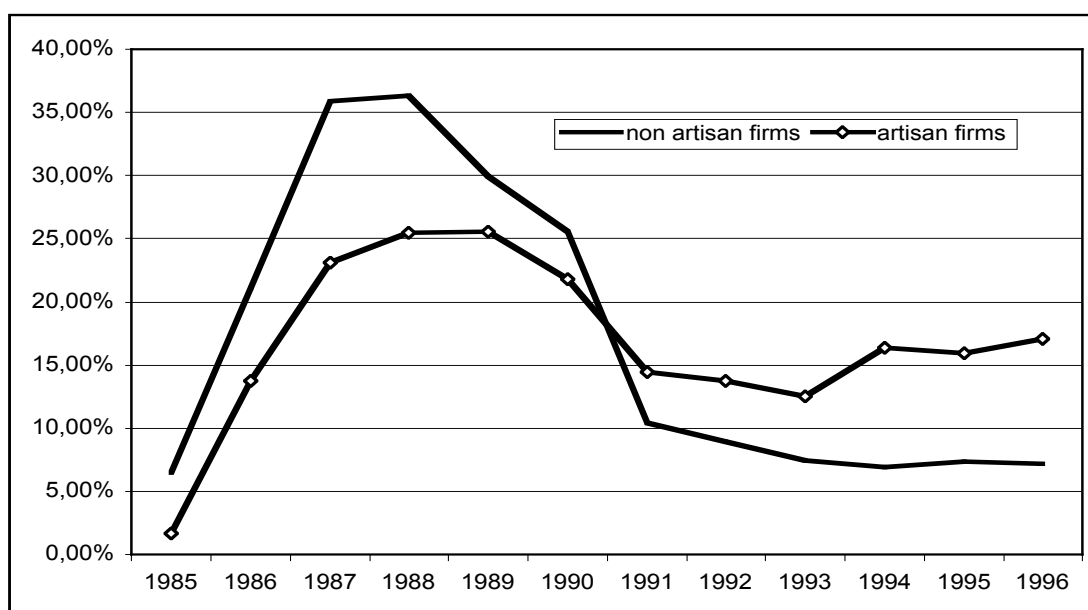
²⁸ A 12 months period is preferred in order not to blur the result with other confounding factors. Estimates for a larger time period, up to 18 months, do not change the results.

In order to study the number of firms entering the CFL program, a logit model is estimated. The dependent variable is the probability of firms adhering to the program and is labelled 1 when the firm enters the program and 0 otherwise: the set of firms is split into artisan and non-artisan.

The period of analysis starts in 1985 and ends when the firm enters the program or in 1996, maximum. The following explanatory variables are considered: sector dummy, area dummy, firms age, lagged worker stock (October the 31st), lagged number of: males, blue collars, young workers, apprentices (lagged variables in order to avoid the endogeneity issue, indeed CFL program might affect these variables in the entrance year), number of hirings (net of CFL, in order to control for business cycle) and a year dummy. In order to capture non-linearity, interaction and second order terms are allowed for.

Indeed, given the appropriate controls, the year dummy describes the effect of the CFL reforms on the probability to enter the program in respect to the baseline year and to artisan firms. The program reform affected only non-artisan firms, hence the variation of the probability to enter the CFL program during the pre-treatment period should be the same between treated (non-artisan) and untreated (artisan firms). Table 9 confirms our expectations: log-odd²⁹ variations are non-significant until 1988; with the first reform, 1989-90, log-odd variations are significantly negative, and since the second reform enforcement, in 1991, strongly and significantly negative. The second reform piled up with the first in the benefit reduction and led to a significant decline of non-artisan firms entering the program.

Fig: 6. Percentage of firms which entered CFL program by kind



²⁹ Odd is the ratio between probability: $\text{odd} = \frac{\Pr(Y=1)}{\Pr(Y=0)}$. The logit estimation coefficients can be interpreted as the coefficients of an hazard model

Tab. 10: Log-odds variation of probability to enter CFL program

Year	1986	1988	1989	1990	1991	1992	1993	1994	1995	1996
<i>DID</i>	.038	-.136	-.475	-.221	-1.097	-1.074	-1.452	-1.657	-1.477	-1.644
P-Value	0.697	0.173	0.000	0.056	0.000	0.000	0.000	0.000	0.000	0.000

Note: Baseline 1987 and artisan firms.

Treated firms willing to maintain the relative employment level can transform the CFL contract in an open-end contract (and enjoy one-year bonus after 1994) or can separate the CFL coming to maturity and draw a new CFL or draw an open-end contract. The first option is preferred. The second and the third options have a similar payroll cost. The firm choice is between half of the benefit (two years later declined to one quarter) for a new CFL contract and a full payroll tax for an open-end contract, but a new CFL implies a new training project approved by the commission and the choice towards a new CFL is much less convenient now that at the program launching: many firm have out-passed the threshold given by the contract fixed costs that make it no more worthwhile to enter the program, and no surprise if the number of treated firms declined markedly over time in comparison with non treated firms, that maintained full benefit (fig. 6).

In other words the total CFL reform impact at the aggregate level is made up by two components. The *ATT* estimate shows that the marginal effect of the reform on the employment level by the firms that entered the program, is insignificant. On the other hand the number of non-artisan firms entering the CFL contract has declined rapidly in respect to that of artisan firms and the contract is mainly used by artisan firms. Artisan and non-artisan firms react to the reforms resorting to different labour contracts.

5. Conclusions

Matching-*DID* estimates suggest that the CFL program had a positive impact on the employment rate of eligible individuals. At the introduction, in the mid-eighties, at the firm level the CFL impact was positive, significant and homogenous for the age group 21-29, although firms entered the program mainly to hire people in their twenties. The CFL program reached its aim: granting a large payroll tax reduction and several other benefits to the firms entering the program, CFL made easy the young workers' search process, and increased permanently the youth employment level. In such a way our study supplements the work by Contini, Cornaglia, Malpede, Rettore that measured the impact of the program exploiting the

temporal and territorial variation of the relative labour costs for the eligibles, and found no evidence of a significant policy impact (Contini, Cornaglia, Malpede, Rettore, 2002).

At the end of the eighties the CFL program underwent two reforms, characterised by a significantly reduced benefit for non-artisan firms: the payroll tax rebate was halved in 1988 and halved again in 1991 and other side advantages were also generalized to other form of labour contracts. Our estimate exploits the difference between artisan and non-artisan firms that had entered the program in order to assess the impact of the reform after controlling for firms heterogeneity. The estimates do not point out significant and reliable changes in the outcome for artisan in relation to the non-artisan firms. Firms that had changed their productive organization and adapted it in order to hire an additional worker under the program continued without a significant employment decline when the benefit was reduced. In this respect granting full benefit to artisan firms had no a significant differential return in terms of a positive variation of employment for the eligibles³⁰.

The number of non-artisan firms entering new CFL program nonetheless declined in the nineties due to the reduced benefit that was introduced by the 1988 and 1991 state budget laws. The number of non-artisan firms hiring workers under the program declined drastically, in respect to artisan firms that went on instead with full fiscal benefit and the relevance of the program in supporting youth employment rapidly declined through time.

³⁰ To asses the impact effect of a program exploiting its variations through time amounts to assume a *ceteris paribus* assumption and, in the case of a reduction of the previously granted benefit, a rather unreasonable assumption on the reversal of the cost curve.

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