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Can employment subsidies and greater labour market flexibility increase job opportunities for youth? Revisiting the Italian On-the-job Training Programme[§]

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Abstract. The CFL (On-the-job Training) programme was introduced in Italy in 1985 with the aim of reducing youth unemployment. The new programme offered employers two main advantages: it exempted them almost completely from payment of payroll taxes and it provided them with virtually the only opportunity to employ people on a basis of fixed term contracts.

The paper looks at the employment impact of the programme among a subgroup of eligible workers and finds that firms taking part increased employment more than non-participating firms by almost 5%. Employers had a strong positive reaction to the tax subsidies and to the softening of the rigid employment code.

The overall effect of the programme on youth employment was however limited, registering only a 1% increase, mainly because about 80% of firms never participated.

Keywords: Labour Policy Evaluation, Youth Unemployment, Occupation and Intergenerational Mobility, Unemployment

JEL classification: J38, J62, J64.

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Can employment subsidies and greater labour market flexibility increase job opportunities for youth? Revisiting the Italian On-the-job Training Programme

1. Introduction

The CFL, on the Job Training Programme (*Contratto di Formazione e Lavoro*), was a scheme designed to integrate more young people into employment. It was typical of what was happening in many European countries after the mid seventies when there was an increase in policies aiming to promote youth employment through active labour market policies (ALMP) at a time of very high levels of unemployment. (O'Higgins, 1997, p. 54; Blanchard, 2006).

The schemes have varied somewhat in form and content over the years and for different countries, but are generally characterised by the use of two basic mechanisms 1) increasing the skill levels of young workers through training 2) lowering labour costs, by reducing wage expectations, and/or reducing the cost of hiring, and/or increasing flexibility for targeted groups of workers, particularly in countries with rigid labour markets. High income taxes and over protective labour legislation are frequently asserted to be two of the main causes of the high rates of unemployment experienced in Europe in recent decades (OECD, 2006).

In the eighties, the Italian labour market was characterised by strong protection regulation and a rigid system of wage determination. The Charter of Workers' Rights (*Statuto dei lavoratori*) had established various employment restrictions (in firms with more than 15 employees) on hiring and firing procedures, and on the use of temporary labour contracts. Also, a minimum wage, generally laid down in sectoral agreements, had been collectively agreed, and applied to all workers, with an almost total coverage.

The CFL programme was introduced in Italy in the last month of 1984 in an attempt to tackle youth unemployment (eligible participants should be less than 29 years old). The new contract had two major advantages: it exempted employers (almost completely) from the payment of payroll taxes, and it was virtually the only form of fixed term contract available for use. The contract provided a partial reform of the employment protection legislation, introducing an automatic termination after a maximum of two years, and a wage subsidy (as well as reduced taxes) that might ease the distortions caused by the minimum wage legislation (Nikell and Bell, 1997).

Several papers have found that this type of partial reform is inefficient (Blanchard, Landier, 2002). In particular, it has been claimed that it induces firms to replace high cost workers with

low cost workers, and encourage high turnover and a reduction in skill levels, and that this can lead to a waste of know how and intellectual resources.

On the whole, the impact of programmes targeted at young workers in Europe has been mixed. Training programmes in particular have had poor results. However, wage subsidies and tax reductions have provided more encouraging findings, but have been accompanied by potential displacement and substitution effects that have seldom been taken properly into account (Kluve and Schmidt, 2002; Ilo, 2005; Kluve, 2006).

By considering the introduction of the CFL programme as an exogenous innovation, this paper evaluates its employment impact on a subgroup of eligible workers, those aged 21-29, in two provinces in the North Eastern part of Italy (Treviso and Vicenza). The experiences there are politically relevant as they show that the programme was conducive to job creation net of possible substitution and displacement effects. Firms participating in the programme increased employment of eligible workers more than non-participants by almost 5 per cent, although the overall impact in the provinces was small, producing about a 1 per cent employment increase, because only one fifth of firms participated.

This paper is structured in the following manner. Section 2 describes the CFL programme, its initial structure and subsequent changes. Section 3 sets out the evaluation problem to be addressed, formulates the micro-econometric analysis and describes a matching approach. Section 4 presents the empirical results, and a set of sensitivity analyses to assess the robustness of the estimated employment effects. This section also devotes particular attention to exploring the linkages between treatment effects, substitution possibilities and different workers' abilities. Section 5 describes CFL reforms that occurred and discusses the programme's lasting consequences. Section 6 examines the causes for the programme's limited diffusion and provides some suggestions for formulating active labour market programmes in Europe to day, and then concludes.

2. The CFL Programme

The CFL was a fixed-term contract introduced at the end of 1984 in order to facilitate the engagement of young workers in the labour market. The CFL programme was aimed at public and private firms hiring young people aged between 15 and 29 years, and which at the date of

application had no recent record of mass redundancies. The programme included the provision of training under a scheme endorsed by the Regional Commission for Employment¹.

The CFL contained a number of benefits. First of all, Social Security contributions paid by participating Italian firms were reduced to 2,5 Euros per capita, a substantial reduction, as the payroll tax in Italy amounted to around 40% of a person's salary, and was one of the largest taxes of its kind in Europe (Contini, 2000). Further, the participants were exempted from the usual firing costs associated with the widely used alternative of the open-ended contract, while the 24-month "fixed term" duration of the CFL allowed a significant increase in flexibility². The programme offered workers specific on-the-job training so that those hired through the CFL were expected to develop a stable and qualified career with the firm. The firm was allowed to hire a worker at a lower contract level (two grades down) than would normally be proscribed by his/her qualifications according to the national contract. Entrepreneurs were also able to hire workers directly, without applying to the Italian Ufficio di Collocamento (employment office)³, which was the usual practice and which meant hiring from a pool of declared unemployed, basically on a first-come first-served basis. In 1991, the opportunity to engage in direct hiring was extended to all Italian firms and one of the comparative advantages of the programme was lost.

At the beginning, workers eligible for employment in enterprises under the CFL programme were young people aged between 15 and 29 years⁴. In 1994, the eligible age range was changed to between 16 and 32 years and eligible workers were extended to include self-employed professionals and workers hired by associations and by research centres⁵. The range of eligible workers was thus extended over the years in response to demand, but at the same time the financial benefits connected to the contract were progressively reduced. The payroll tax rebate was reduced from 98% to 50% and subsequently to 25%. From 1991, firms applying to the programme, and that had participated in the past, were required to have hired at least 50% of the

¹ The training would subsequently turn out to have been largely ineffective, as shown by several studies. (Breda 1993, p. 167).

² Fixed-term contracts were introduced in Italy in 1962, but were very strictly limited (covering seasonal activities and temporary replacements). CFL was the first widely diffused fixed-term contract. It could not be renewed with the same training specification. In fact, the contract was seldom renewed.

³ With exceptional conditions for apprentices (Law 25, 19.01.1955 art.3).

⁴ The upper boundary was 32 years old for the South of Italy and also for those Northern Italian regions with an unemployment rate higher than the national average.

⁵ The latter were included by Law n.196, 24.06.1997. Law n.299, 16.05.1994 added a new job training contract to the previous one. It offered a reduced training period of 12 months, and allowed employers to be eligible for the tax rebate for one year after the CFL contract was transformed into a permanent contract. Employers undertaking the 12 month CFL were eligible for the rebate only if the contract was transformed. Employers in the southern regions continued to enjoy the full rebate.

employees that had come to the end of their CFL programme over the previous two years. This percentage rose to 60% in 1994⁶. CFL terminated in 1995 (Table 1)⁷.

The progressive limitation of the incentives attached to the programme resulted from two factors: firstly, the precarious condition of Italian public finances, and secondly an attempt to make the programme more selective, encouraging its use by firms with favourable employment prospects while discouraging firms that were only interested in the financial bonus (Contini et al., 2002).

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Tab. 1: CFL Rebate on Social Security Tax Payable by Employers.

Year	Employers:	
	Artisans; Southern Italians; Employers from high unemployment areas	All others
from 1.11.1984 to 30.5.1988	About 98%	About 98%
from 1.6.1988 to 29.12.1990	About 98%	50%
from 1.1.1991 onward-...	About 98%	25%

The present study is based on VWH (Veneto Worker Histories), a data-set built up at the University of Venice from social security files. VWH contains information on all participants in the CFL programme in the private sector in the two provinces of Treviso and Vicenza, for the years 1975-1997. It includes register-based information on all establishments in the two provinces, and on all the employees that were hired by those establishments - even if for just one day during the observation period - independent of the worker's place of residence⁸. The observation unit is the employer-day and is used to build a monthly history of the working life of each employee. The choice of location was constrained by data availability, but the two provinces make an interesting case study because of the wide diffusion of the scheme there.

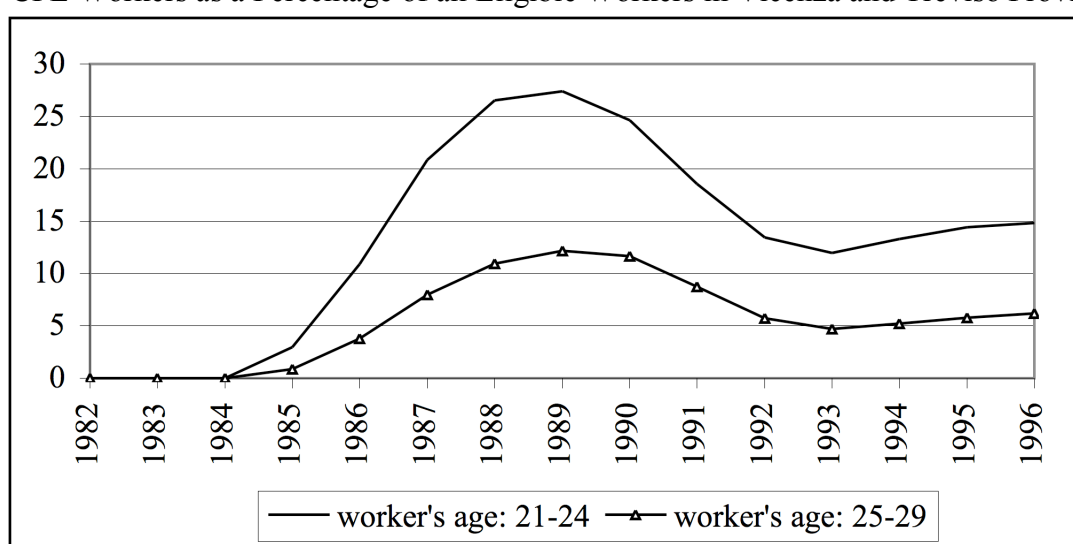
⁶ The percentage was raised to 60% in May 1994. D.L. n.299, 16.05.1994. Lay-offs and firings for misbehaviour (*per giusta causa*) were not considered when calculating the hiring percentage.

⁷ It was declared illegitimate by the European Commission on the grounds of unfair competition because the level of subsidies were differentiated by territory (firms in the Mezzogiorno had higher incentives) and by type of firm. As of today, targeted payroll tax rebates are legitimate only to the extent that new jobs are created by the same employer, in addition to existing ones.

⁸ The entire working life of all employees that worked at least one day in Treviso and Vicenza, was reconstructed, including periods of work away from Treviso and Vicenza.

The paper focuses on young workers of 21 to 29 years of age. For workers aged 21 to –29, the CFL was the only alternative available to the “normal” open-ended contract, and so its impact can be more accurately measured. The selected group is split into two to take into account different characteristics. A 21-24 cohort includes workers that had finished high school or military service (if males). A 25-29 cohort includes workers with a college education, and also workers with higher skills and work experience. Younger workers aged 16 to 20 are not considered because they could take advantage of two “entry contracts”, the CFL and the apprentice contract, the effects of which overlapped and this makes identification of the CFL impact difficult.

Fig. 1: CFL Workers as a Percentage of all Eligible Workers in Vicenza and Treviso Provinces



After 1985, when CFL was launched, the programme was immediately and unquestionably successful. For many firms, particularly small firms, CFL became a common way to hire young people. In Italy, on average about 25% of hiring in the eligible age cohorts was through the programme. In Treviso and Vicenza, workers hired under the programme were almost 15% of the eligible employment stock in December 1987, rising to 24% for those aged 21 to 24 (See Figure 1). The number of CFL workers continued to increase until the first programme reform in 1988, then a more modest growth in 1989 changed into a drastic decline, especially after the second reform of December 1990; then it increased once again in the mid nineties, in response to a recovering economy.

Table 2 presents the probability of a young worker from Treviso or Vicenza provinces moving from unemployment to employment in the eighties⁹. Four age cohorts are examined, the 30-32 cohort acts as a control.

Table 2. The Probability of Transition from Unemployment to Employment in Treviso and Vicenza.

Age cohort	1982	1983	1984	1985	1986	1987	1988	1989
16-20	0,58	0,53	0,58	0,71	0,78	0,84	0,86	0,79
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

The probability of transition from unemployment to employment is computed as the ratio between associations from unemployment (VWH data) and active population (interpolated from the population censuses of 1981 and 1991 according to the employment rate, marginally adjusted to fit the proper age class).

The transition probabilities for the four age cohorts are very different in absolute numbers, which reflects the progressive decline of young people entering the labour market and the effect of increased participation in education. In the pre-CFL years (1982-1984) the four age cohorts are fairly stable. The age class 16-20 has a distinct pattern of its own, with a rapid decline beginning in 1989 which reflects decisions to attend high school in large numbers and is not of much interest to our analysis. The cohort (21-29) shows in the first year of the programme (1984-85) a positive increase in the probability of transition to employment which is greater than that of the age group 30-32. This difference is reinforced in 1986 and 1987 (During the period 1985-87 there was an increase of +53% in transition numbers for the 21-24 cohort, but only +13% for the 16-20 group and +43% for the 30-32 age class), but then declines in the following couple of years.

Such evidence is at the root of the success claimed for the CFL programme, but it is very difficult to assess the extent to which it the CFL program was responsible for the increase in employment among those eligible to participate. A simple comparison between pre-programme and post-programme outcomes may be distorted by other parallel events, particularly increasing participation in education which tends to delay the entrance age in the labour market - such that this in itself might in part be the cause of reduced unemployment levels, implying thereby that

⁹ In the eighties the structure of labour supply was strongly influenced by the baby boom in the early sixties and the subsequent rapid decline in the population birth rate; by the rapid increase in school attendance (Canu, Tattara, 2004); and by the increase in average life expectancy.

the effect of CFL might not be as great as would first appear. In order to control for such potential biases we set the evaluation problem in an econometric framework.

3. The CFL Evaluation Strategy

3.1. The evaluation problem.

The CFL was introduced all over the country at the same time. Firms were not forced to participate in the programme and only “the good ones”¹⁰ - firms with a credible training plan, and which had not fired substantial numbers of workers during the previous year - were accepted.

The control group includes firms that were interested in hiring eligible employees but not interested in CFL; firms that applied to the CFL Commission and received approval but did not take up the programme for various reasons; and firms that applied and whose application was rejected. These three categories are not separately identifiable.

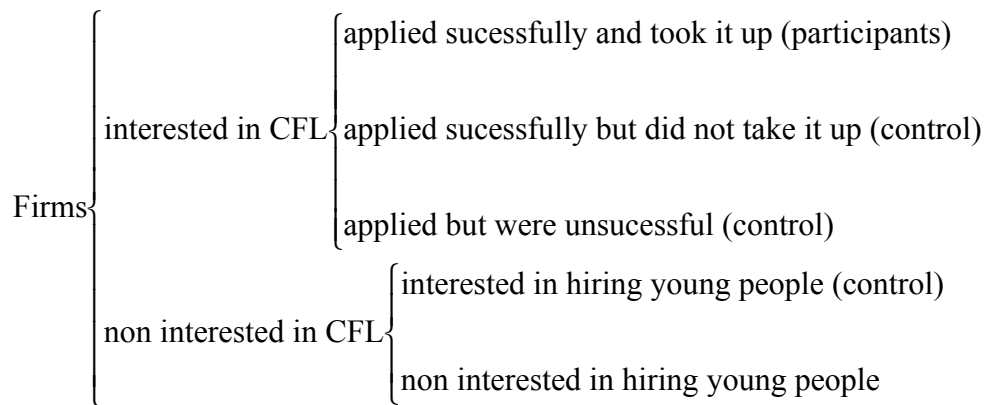
Many applications were rejected, mainly because the training plan presented by the applicant was not considered satisfactory¹¹. These firms are particularly good candidates for our counterfactual experiment as they presumably share many of the characteristics of the participating firms. Both groups tried to engage in the programme, although, possibly, with different intensity.

Some firms were declared eligible but did not take up the offer of engagement in the programme for reasons unknown – possibly there was either a change in the firm’s plans, or maybe it had a lack of workers with the required skills - which makes it difficult to devise an appropriate control. To our knowledge they are very few, particularly during the first years of the programme. The remaining firms were not interested in hiring eligible workers and were discarded¹².

¹⁰ The concern with spending money on “the bad and the ugly” was politically understandable as CFL was supported by the trade unions under a socialist government, so active policies from entrepreneurs was considered an essential counterpart to the expenditure of public money.

¹¹ According to several interviews with former members of the Regional Commission for Employment, almost half of the applications were rejected.

¹² The approach was suggested to us by Barbara Sianesi.



To assess if the CFL programme improved labour market conditions for young people, an Intention to Treat framework (ITT) is necessary. The ITT measures the CFL impact on youth employment in general, while the Average Effect of Treatment on the Treated (ATT) measures the effect on youth employment in just those firms that took part in the programme. The ATT is useful for studying the reaction of those firms that took part in the programme to monetary incentives and flexibility in labour legislation. At the final stage, the ITT is measured by multiplying the ATT value by the fraction of firms that took part in the programme.

The key question we want to answer is what is the effect of the treatment on the magnitude of youth employment, compared to what it would be if the treatment had not taken place. Let Y_{1i} and Y_{0i} be random variables representing the employment stock of eligible workers for cases of treatment and non-treatment respectively. The impact of participating in the programme for any firm i is given by $\Delta_i = Y_{1i} - Y_{0i}$, a measure that is not directly observable as no firm can experience both treatment and non-treatment at the same time.

Let D_i be a binary variable that takes the value 1 if a firm i took up the programme and 0 if it did not, and let X_i denote the characteristics of firm i . What is observed is:

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

The Average Treatment Effect on the Treated is the average gain due to the programme for firms that actually choose to participate: $E(Y_1 - Y_0 | D=1) = E(Y_1 | D=1) - E(Y_0 | D=1)$.

The employment effect of firms in the control group is assumed equal to zero, i.e. firms that did not participate in the programme show no effects from its availability.

A large majority of firms had the option to participate in the programme, but some firms declined. Firms choosing not to participate may differ systematically from participants in respect of their levels of motivation and other important characteristics. The difference in motivation (or other characteristics) may itself lead to different results for the two groups, and thus bias the

study estimates. For example, if the firms entering the programme are more prone to growth, or are bigger in size, thus allowing more people to be hired and to take part in the programme, any employment increase would be the result of selection and not of the programme.

3.2. The matching assumptions.

A way out of this dilemma is to address the problem directly by making the two groups, treated and untreated firms, comparable on observed characteristics through a process of matching. A counterfactual analysis 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 programme outcomes are independent, and conditional on a vector of observable characteristics X

$$(Y_0, Y_1 \parallel D) | X \quad (2)$$

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

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

Rosenbaum and Rubin (1983) demonstrated that if the CIA holds, matching on the propensity score results in an unbiased estimate of ATT.

Matching methods, by choosing and reweighing observations within the common support, eliminate biases arising from comparisons of the wrong units (comparing units outside the common support region), and biases arising from comparisons of the right units in the wrong proportions (differences in the densities of observable characteristics between treatment and comparison units), but not biases arising from unobservables. The CIA requires that, conditional to X , there be no unobserved heterogeneity left that affects the decision to join the programme. The CIA thus requires detailed knowledge of the factors that drive participation. In this paper the choice of a matching approach is motivated by the richness of the available information related to firms’ heterogeneity and to job seekers’ characteristics.

In general, after having run logit, probit or semiparametric estimations on pre-participation variables X , the fitted values, $p(X)$, are used in order to match participants with control units.

Following Heckman, Ichimura and Todd (1998) the form of the matching estimator can be set in the following framework

$$ATT = \sum_{i \in T} \omega(i) [Q_{1i} - \sum_{j \in C} W(i, j) Q_{0j}] \quad \text{for } X \in \bar{X} \quad (4)$$

where Q_{1i} is function of the treatment T outcome, Y_{1i} ; Q_{0j} is function of the comparison C group outcome, Y_{0j} ; $W(i, j)$ 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 heteroscedasticity and scale; \bar{X} is the common support on the treated and untreated units: $\bar{X} = X_1 \cap X_0$.

t^0 is the date at which the participants entered the programme, t^1 is the earlier period and t^2 the period from t^0 onward. Exploiting the panel data information, the ATT estimate is obtained by the difference between matching estimates before and after treatment (Heckman et al., 1998). In terms of expression (5) the programme outcomes are defined as $Q_{1i} = (Y_{1it^2} - Y_{1it^1})$ and $Q_{0j} = (Y_{0jt^2} - Y_{0jt^1})$.

The identification assumption is the usual conditional independence assumption CIA, in differences:

$$E(Y_{0t^2} - Y_{0t^1} | X, D = 1) = E(Y_{0t^2} - Y_{0t^1} | X, D = 0) \quad (5)$$

i.e. no difference in the outcome trend between non participants and participants when there is no treatment. In a context of a linear model with an additively separable error term, u_{0t} , this condition is

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

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

The estimation strategy assumes that the programme does not affect the control group - a reasonable assumption because although engagement in the CFL programme in the eighties was widely diffused, a large number of young workers were available for hiring by untreated firms.

4. Empirical Findings.

4.1. The employment impact.

The important questions are how much did youth employment increase in firms that participated in the programme, ATT, and how was this reflected in an increase in overall youth employment, ITT?. Eligible workers are those aged 21-29, and the age range has been split into two classes, 21-24 and 25-29¹³, in order to separate the programme influence on the less skilled workers from its influence on the more skilled workers, likely to have a college or university education¹⁴. Treated firms are firms that at the stated month in 1986 hired people under the CFL (table 3). Untreated firms are defined as firms that did not enter the CFL programme in the same month; or they are non- participants or they are not-as-yet participants but might enter the programme at a later stage (see page 7)¹⁵.

¹³ The age class 15-20 was excluded for two reasons. Firms can exploit the apprenticeship programme and this blurs the results; by excluding the age class 15-20 we get rid of any contamination between the two contracts. Second, the extension of school attendance drastically reduced the number of eligible workers over time.

The panel excludes firms that are established or that failed during the study years, so the possible variation in stock due to this natural process is ruled out

¹⁴ The two Veneto provinces are in full employment, so that the termination of full-time education, most of the time, matches with the entrance into the labour market.

¹⁵ In terms of the schema on p. 7: firms that applied successfully but did not take it up, that applied without success, and firms interested in hiring young workers outside the programme. The probability of hiring a worker under CFL for firms hiring a eligible worker in 1986 is assumed to be independent from hiring a worker under CFL in the near future. See the following par. 4.4.

Table 3. Treated Firms and Eligible Worker Cohorts

Period of observation			Number of firms entering the programme (participants)	
Pre-treatment	Treatment	After-treatment	Workers 1961-1964 birth cohort*	Workers 1957-1960 birth cohort ^o
1985/1-1985/12	1986/01	1986/02-1986/12	153	49
1985/2-1986/1	1986/02	1986/02-1987/1	87	39
1985/3-1986/2	1986/03	1986/03-1987/2	77	26
1985/4-1986/3	1986/04	1986/04-1987/3	77	27
1985/5-1986/4	1986/05	1986/05-1987/4	88	32
1985/6-1986/5	1986/06	1986/06-1987/5	74	23
1985/7-1986/6	1986/07	1986/07-1987/6	100	33
1985/8-1986/7	1986/08	1986/08-1987/7	30	11
1985/9-1986/8	1986/09	1986/09-1987/8	98	37
1985/10-1986/9	1986/10	1986/10-1987/9	118	53
1985/11-1986/10	1986/11	1986/11-1987/10	109	47
1985/12-1986/11	1986/12	1986/12-1987/11	61	19
Total			1072	396

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

^oWorkers' age is 25-26 for birth cohort 1960, 26-27 for birth cohort 1959, ... 28-29 for birth cohort 1957.

A firm can be present in both groups. Firms that hire only in a single age cohort cannot be used as a control for the other age cohort, so that any possible substitution effect is avoided.

Observable characteristics in the last pre-treatment month are used for selecting for the p-score matching.

Workers are clustered according to birth date. The ATT is estimated through a *DID*-p.score-matching estimator and identifies the average effect of the programme as the difference between the average outcomes in the participating group and those in the control group. This is achieved by comparing the two groups in terms of the changes in the average monthly employment levels of eligible workers over 12 months before, and a similar period after, entering the programme, given the appropriate control variables. The entrance date slides through time, from 1.1986 to 12.1986¹⁶ and table 3 presents the sliding observation window used in the estimate, with the number of firms taking up the programme. The observation window is centred in the entrance month, is 24 months long, and slides month after month. The period of 12 months before and after entrance into the programme proves to be long enough so that the result is not blurred with extemporaneous spikes.

Firms choose to participate and this causes an important bias in our estimate. Treated firms' past employment records show that they employed many more young workers in the pre-treatment period than untreated firms. In addition, treated and untreated firms are remarkably different in size and employee composition as reported in table A1 of the appendix.

¹⁶ CFL started in 1985 but only a few firms participated, so the programme is evaluated from 1986.

Consequently, there is a need to construct a control by identifying a proper ‘match’ in the control group for every treated firm. As indicated earlier, the employment effect of the programme is estimated through a two-stage procedure. First, the matching process sets up a treated and an untreated group similar in terms of their propensity scores (table A1 appendix). Second, for each firm, the variation in employment stock over time is computed (the difference between pre-treatment and post-treatment). The ATT estimator (*DID* p-score matching estimator) is computed as the average difference between the matched treated and untreated firms.

Firms are pooled according to the month in which they joined the programme and a logit model for each of the 12 groups is estimated on the following pre-participation exogenous variables (measured one month before entering the programme): size, sector, industrial area dummies, firm age, number of males, blue-collars (proxy for capital or labour intensity), apprenticeships (proxy for the firm’s inclination to use fixed-term contracts and training contracts), eligible workers and the yearly variation of eligible workers. In order to capture non-linearity, interaction and second order terms are allowed (see table A2 Appendix).

Yearly variations in the employment of eligible workers in the pre-treatment period are included among the variables in order to control for the firms’ different rates of growth. The lagged employment variable is a good forecaster for future behaviour of participants as far as employment is concerned. The autoregressive estimate of the yearly variations in eligible workers in the most recent past is estimated by OLS over the period January 1982 - December 1984: the coefficient of the lagged employment 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 the eligible workers (std. err.)	.9591724 (.0005126)
Adj R-squared	0.9049

We matched participant and non-participant firms with the closest p-scores and from the same month observation (symmetric difference in difference matching). The calliper nearest-neighbour matching with a tolerance under 1% imposes a common support and excludes less than 5% of the participant population¹⁷. Overall, matching on the estimated propensity score balances the X’s in the matched samples extremely well. Seasonality, which is particularly relevant in the hiring of young people, is dealt with by matching treated and control firms month by month and

¹⁷ In some cases we imposed not only the nearest neighbour but the 5 nearest. By doing this the estimate improved but the variance increased. The tolerance and the number of comparisons were chosen in order to minimise outcome differences between the two groups during the pre-treatment period.

comparing the 12 month pre-treatment period with the 12 month post-treatment period. Twelve \hat{ATT}_t are computed, one for each month of treatment, but only the weighted (according to the observed number of treated firms in a given month) average of the \hat{ATT}_t is reported:

$$\hat{ATT} = \sum_t \hat{ATT}_t \cdot P(D_t | D = 1) \quad t: \text{January, } \dots, \text{December 1986} \quad (7)$$

Control group firms are the same across the repeated procedure (14.308 firms), but the specific observations used in the logit estimate vary for each group, depending on the specific application date.

Figure 2 shows the monthly employment stock of 21-24 year-old workers (dependent variable), both treated and untreated, measured with respect to the average firm's size (20 employees), centred in the month in which, for the first time, the firm hires a CFL worker. The horizontal axis measures the distance in months in relation to the month that the firm joined the programme, which is labelled 0.

Table 5 reports the ATT estimates from equation 4, summarized according to equation 7. Over the treatment period, the number of eligible workers employed by treated firms differed significantly from the number of eligible workers employed by untreated firms, and the difference is close to 1. The introduction of CFL, evaluated through the *DID*-p-score matching, had a fairly strong effect in increasing employment of the eligible age cohort in the firms participating in the programme. On average, the treated firms each hired one extra young worker more than the untreated firms. The outcome is very similar for the two age cohorts. On average, participating firms increased their eligible employment stock by 5%¹⁸.

Tab. 5: Estimated Average Treatment Effect on the Treated (*DID*-p.score-matching).

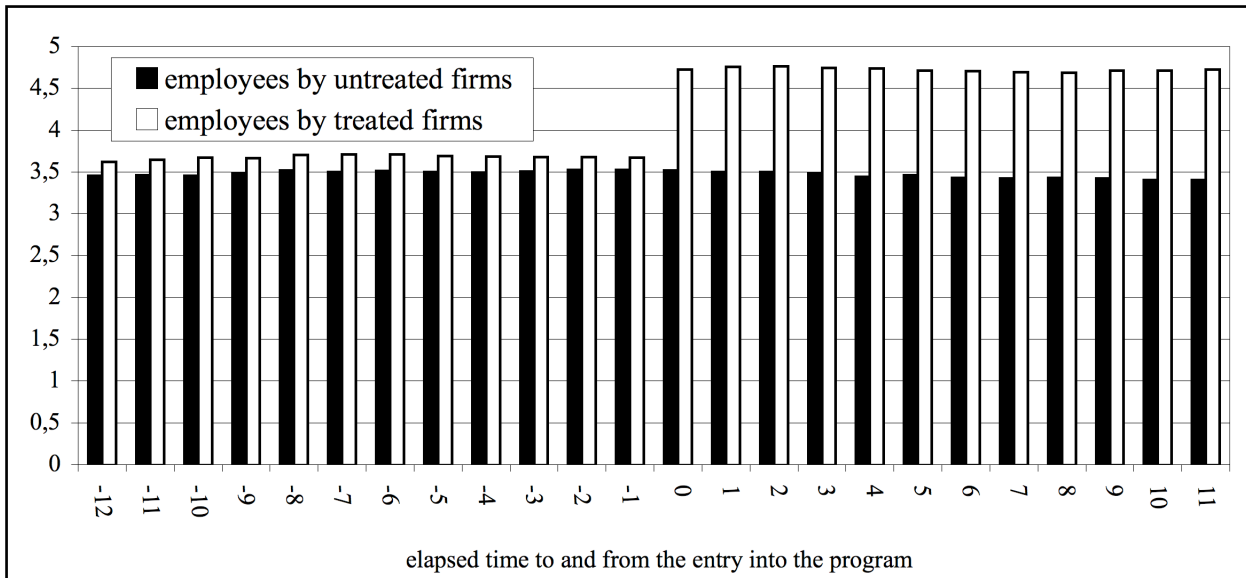
Eligible age cohorts	21-24	25-29
\hat{ATT} (bootstrap std. Err.)	1.101183 (.0228328)	.9996684 (.0235885)

Matching allows us to remove the main differences between participating and non-participating firms in the 12-month pre-participation period. Some heterogeneity, which is fairly constant over time (about 0,2), is still present (fig. 2), and is dealt with successfully by

¹⁸ If people hired under the programme were already employed, does a positive coefficient for the programme imply an increase in employment? The answer is affirmative. All other things being equal, workers who terminate their previous jobs (whether through quitting or being fired), are probably replaced by new workers, and so the situation for the firms that lose their workers remains the same. Sooner or later the system fills the vacancy caused by the new CFL hiring with an unemployed worker.

DID.

Fig. 2: Number of 21-24 Year-old Workers by Firm Treatment Status, after P-score-matching



Many studies report a decrease in the probability of employment before participation in a programme like CFL. This effect was first observed by Ashenfelter and is therefore referred to as Ashenfelter's dip. The most popular explanation for it is that firms that are planning to take part in the programme in the future anticipate their participation and therefore reduce their recruitment. Such a dip in employment would cause an over-estimate of the CFL impact¹⁹ but this possibility is ruled out by the stable pattern of employment shown in figure 2.

Admission to the programme was limited to firms whose training plans had been approved. In fact, several applications were rejected and about 50% of potentially eligible firms did not apply²⁰. To assess the programme's overall impact on youth employment, we must take into account the probability that a firm will participate in the programme, which is 1/4, on average, for the years 1986-87 (table 6 last row). The impact effect on total net employment for participating enterprises is positive and statistically significant, although the overall general impact on youth hiring is limited, since it is only a small fraction of the estimated ATT, i.e., 1.2%.

¹⁹ The 'dip' typically occurs if firms are selected which experienced a decline in the number of their employees prior to engaging in the programme such that a subsequent increase in employment might be attributable to the 'dip' and not to the programme.

²⁰ Breda (1993, p. 161 ff)

Tab. 6: The Number of Firms in Vicenza and Treviso Provinces that Joined the Programme, and the Number of Eligible Firms: 1985,1986,1987.

	Year			
	1985	1986	1987	1986-87 average
New firms that joined the programme	1044	3912	5514	4713
Number of eligible firms	24612	22298	18675	20487
Percentage of firms that joined the programme	4.24%	17.54%	29.53%	23.01%

4.2. A sensitivity analysis

The lagged yearly variation in employment of eligible workers plays an important role in identifying the programme's impact. A sensitivity analysis helps to understand whether firms with different growth rates performed differently in respect to this variable. A simple sensitivity analysis looks at the estimated effects on employment in relation to various rates of growth in employment experienced by the treated.

Participating firms are grouped into firms with decreasing, constant and growing employment one month prior to treatment²¹ (-1, 0, +1), and for each set the matching procedure is replicated. It is found that the employment gains are virtually the same across the sub-samples (table 7). After joining the programme, firms with decreasing employment increased their stock of eligible workers by an average of one additional worker over the pre-treatment stock; the same is true for the firms that previously had constant employment; and also for those that had increased their stock of employees before joining the programme.

Tab. 7: Estimated Average Treatment on the Treated According to Pre-treatment Growth Rates in Employment (*DID*-p.score-matching).

Employment growth	< = -1		= 0		> = 1	
	21-24	25-29	21-24	25-29	21-24	25-29
\hat{ATT} (bootstrap std. err.)	1.250839 (.065365)	1.077297 (.069879)	.9748806 (.013941)	.9732956 (.017048)	1.28522 (.048300)	.9856224 (.076903)

4.3. Income and substitution effect.

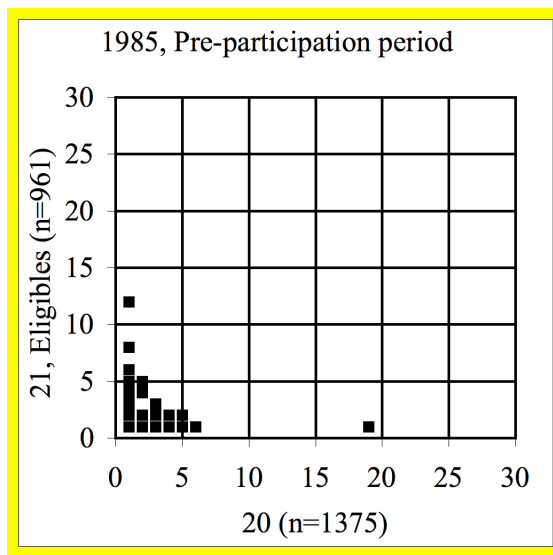
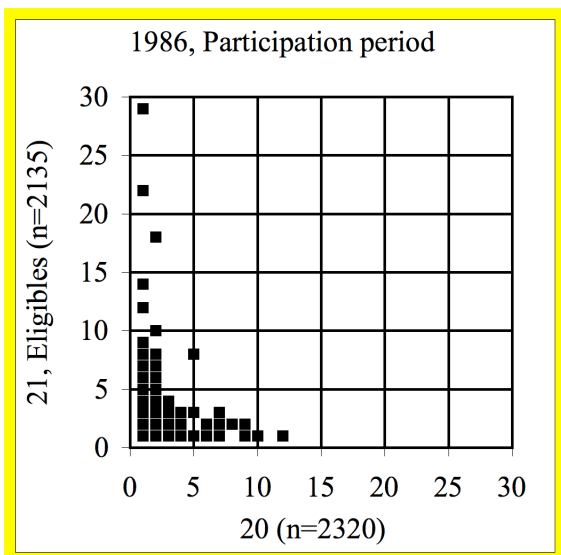
Did the increase in employment of eligible workers lead to the displacement of other workers so that the overall result was less satisfactory than assumed? The employment increase in the age

²¹ Most of the participating firms had a growth rate equal to zero, which is also the median. The extension of coverage to more months previous to treatment altered the relative number of firms belonging to the three groups but did not alter the estimate results.

cohort 21-29 might be at the cost of a parallel employment decline in younger or older workers. An assessment of whether the programme did have such an impact needs to proceed net of the so-called “bookkeeping” effects.

Figure 3. Hiring by Participant Firms

a.



b.

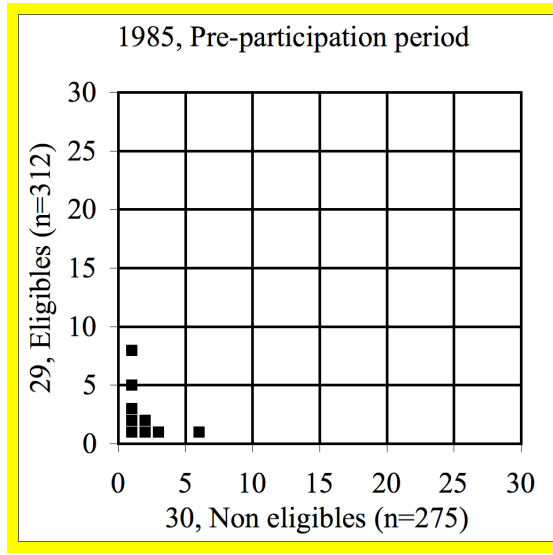
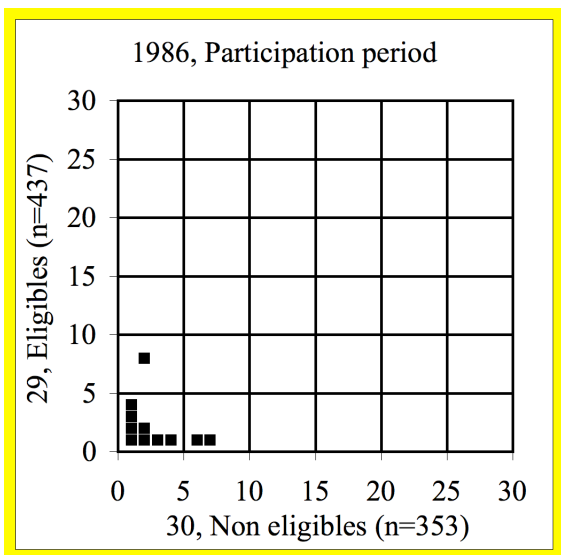


Figure 3a and 3b provide a first descriptive answer. The behaviour of treated firms in the participation period (1986), and in the pre-participation period (1985), is compared in relation to the hiring of eligible and non eligible workers. Participating firms in 1986 hire more 21 year old relative to 20 year old workers than in the pre-participation period (See fig. 3a left and 3a right, vertical axes), and roughly the same number of 29 year old relative to 30 year old workers (See fig. 3b left and 3b right, vertical axes). This indicates that in the participation period treated firms have recruited a greater proportion of eligible workers in relation to non-eligible workers particularly at the lower age margin; a sign of a possible substitution effect that is absent at the upper age margin. But a definite conclusion about a possible displacement effect would require a more sophisticated analysis.

A reduction in the cost of a group of workers has two standard effects: a substitution effect, and an “income” effect - whereby firms may save money and so “increase their income” which could be spent on hiring more workers – both eligible and non-eligible. The fact that eligible firms might have hired non eligible workers at the upper margin might simply reflect a strong “income” effect rather than the absence of substitution effect. In order to check for an “income” effect we extended the analysis to workers in the age group 35-40. They are less prone to the substitution effect but might have been influenced by an “income” effect. Mature workers have different experiences and skills compared with those participating in the CFL programme, who are mainly new entrants, and are liable to play a very different role in the firm’s organization. A substitution of workers in the 35-40 age group by eligible (younger) workers participating in CFL is probably very limited, while a positive “income” effect for the firm will be reflected in a general increase in hiring in all age classes.

In order to test the magnitude of the substitution and “income” effects we computed the Average Effect on the Treated in respect of employment variations for the age classes, 15-20, 30-32, 35-40 experienced by participant and non-participant firms (with participating firms being defined as those who employ under CFL workers from eligible age classes: 21-24; 25-29). (Table 8)²².

²² We have limited ourselves to 40 years of age because older workers would introduce much heterogeneity in the sample.

Tab. 8: Estimated Average Treatment Effect on the Treated Computed with Respect to the Age classes 15-20; 30-32; 35-40. (*DID* p.score-matching)

Eligible age class: 21-24		Eligible age class: 25-29			
$\hat{ATT}_{15-20 \text{ cohort}}$ (bootstrap std. err.)	.1576029 (.0226783)	$\hat{ATT}_{30-32 \text{ cohort}}$ (bootstrap std. err.)	-.0031796 (.0158533)	$\hat{ATT}_{35-40 \text{ cohort}}$ (bootstrap std. err.)	.0022628 (.0133871)

The relative difference in employment for the age class 35-40 between treated and control groups is insignificant and confirms the absence of a positive “income” effect for participating firms (Table 8). There is a positive, but minimal CFL effect on the employment of workers aged 15-20 (negative substitution or complementarity between workers of the two age classes 15-20 and 21-24) measured by 1 more employee for every 5 firms of average size, i.e. approximately 1 in 100 workers, but the CFL net effect on the 21-24 years old workers is 5 times as big. The relative difference in employment for the age class 30-32 between treated and control groups is insignificant and confirms the absence of a positive substitution effect (Table 8).

4.4. Selection bias.

Matching estimators mimic a randomized experiment ex-post by constructing a comparison group as similar as possible to the treatment group. Even with the most informative data, matching can only control for observable differences between the treatment and comparison groups, and the method assumes that there is no remaining unobserved heterogeneity between the two groups that could possibly bias the treatment effect estimate (Kluwe, Schmidt, 2002, p. 427). Eligible workers were not only hired by participants through a CFL contract, but also by participants through an open-ended contract, and also by non-participants. Why would any firm hire an eligible worker without using CFL? Possibly different firms hire different kinds of young people and this can make control firms different in their hiring policies.

Different propensities to hire eligible workers on CFL contracts suggest the possibility of unobserved heterogeneity between the two groups. The obvious explanation for varying propensities to employ eligible workers on CFL contracts is that in order to employ better young people – i.e. high-quality employees who want a long-term relationship with the firm - an open-ended contract is required. Some firms may have targeted "the cream of the crop"; other firms maybe went for "bulk purchase and discount"²³. If this is true, our control group would prove inappropriate. Firms with growing employment needs chose to participate while firms more prone to high quality growth, and requiring a limited number of workers, went into the control group, and the positive outcome for the participants is the result of selection.

In fact, participants hired a substantial number of eligible young workers on open-ended contracts in the same month that they hired CFL workers. Among all workers hired by participants, 49% were CFL employees and the rest were hired on open-ended contracts, a clear sign that many participants choose to use the two contracts in parallel, with selection in mind. Adding a new worker to the CFL project would not have proved particularly expensive, but entrepreneurs had other reasons to choose high quality workers. Firms use the new CFL contract in parallel with the open-ended contract, with its higher wages, for selection purposes, but this behaviour seems not to affect crucially the composition of our control group. Both types of firms, treated and untreated, hired eligible workers on open-ended contracts; both were sensitive to the labour supply portion that could be attracted by offering better employment conditions; and both hired a mix of ordinary and high quality workers²⁴.

²³ The segmentation of the labour market due to the diffusion of fixed-term contracts in recent years, is considered, for example, by Boeri, 1999; Blanchard and Landier, 2002; Cipollone and Guelfi, 2005.

²⁴ Firms in the control group include a substantial number of firms whose application was rejected, and their demand for labour was presumably very similar to the demand for labour expressed by the treated firms.

5. The CFL Reforms

The CFL reforms, in June 1988 and January 1991, reduced the financial rebate for participating non-artisan firms by 50%, twice, leaving the benefit intact for artisans (table 1). The benefits related to contract flexibility were basically untouched. Very few firms joined the programme after the reform and the CFL was mainly used by artisan firms, and had a limited effect on total employment.

The effect of the reforms on the firms that chose to join is now the most interesting question. Given that, when launched, the programme had many adherents and that the programme significantly altered firms' behaviour²⁵, one would expect that those firms that lost the financial benefits (non-artisan firms) would return to their starting situation. Surprisingly, non-artisan firms that originally took up the programme carried on with the same employment as before.

The ATT estimates the average employment variation due to the programme reforms on firms with ongoing CFL participation. Treated and control groups are non-artisan and artisan firms. The parameter estimate is an ATT conditional to those firms who joined the CFL programme at baseline.

Treated and control firms are exogenously identified (They belong to the non-artisan or artisan set), and the basic problem is heterogeneity between the treated and the control groups, i.e. a problem of diversity, not a problem of selection such as the one we encountered in the previous sections.

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. Even after matching and balancing the observable variables - the stock of eligible workers for artisan and non-artisan firms - in the period before the reforms artisan and non-artisan firms exhibit different patterns through time. This heterogeneity²⁶ can be addressed by using regression-adjusted matching²⁷. Heckman, Ichimura, Todd (1998) combine the matching method and regression adjustment on the X . Their method extends beyond the classical matching process by utilizing information about the functional form of the outcome equation and by directly confronting the problem of the difference in behaviour over the pre-treatment period.

²⁵ Empirical observations show that the benefit reduction was accompanied by a drastic fall in the number of non-artisan firms joining the programme.

²⁶ Blundell et al. (2003) observe that "for the evaluation to make sense with heterogeneous effects, we must guarantee that the distribution of the relevant observable characteristics is the same in the four cells defined by eligibility and time". They suggest using two propensity scores: one for eligibility and one for the time period.

²⁷ We do not use this technique in the previous analysis because the lagged employment variation of the eligible workers accounted for any self-selection. Thus, after matching there was no a-priori reason to expect a different response to the cycle from treated and untreated firms (table 5 and figure 2).

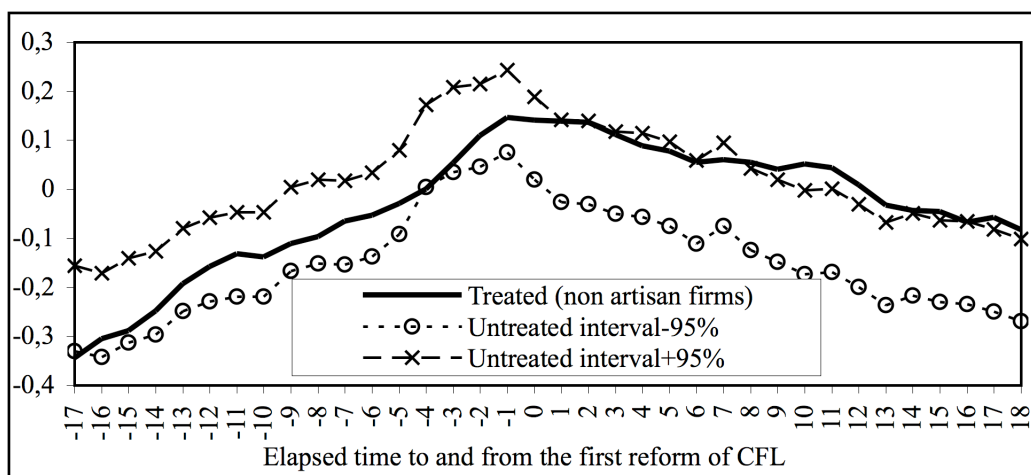
Regression-adjusted matching is performed through the following procedure (Heckman et al., 1998). Assume a conventional econometric model for outcomes in the untreated state that is additively separable in the observable X and unobservable characteristics U :

$$E(Y_0 | X, D = 0) = X\beta_0 + E(U_0 | X, D = 0) \quad (8)$$

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

In order to account for the different reactions to the business cycle by the treated and the untreated firms, the amount of non-CFL recruitment is considered. Indeed, treated (non-artisan) firms and control (artisan) firms have a different cyclical behaviour through time – previous controls given – and a focus on non-CFL recruitments for the two categories of firms captures a picture of the variability that derives from the cycle effect.²⁸ Figure 4 plots the residuals of the regression of the employment variable for the treated for the age class 21-24 after regression-adjusted matching, once observable factors have been properly controlled for. The horizontal axis measures the time, in months, in relation to the beginning of the first reform in 1988, which is labelled 0.

Fig.4: Residuals of the Regression $Y_{1i} = X_i\beta_0 + u_i$ and Confidence Interval Constructed Around the Residuals of the Regression $Y_{0j} = X_j\beta_0 + u_j$



²⁸ Observables are quite balanced between treated and untreated firms. Results are available on request.

Tab. 10: Estimated Average Treatment Effect on the Treated in Relation to the Two CFL Reforms (*DID* p.score-matching).

Reform period examined	from 6.1988 to 5.1989		from 1.1991 to 12.1991	
Eligible age cohort	21-24	25-29	21-24	25-29
\hat{ATT} (bootstrap std. err.)	.107756 (.017400)	-.020937 (.029064)	.043564 (.010108)	.025117 (.013646)

Firms that were faced with a much reduced financial benefit, did significantly cut the number of their new CFL contracts²⁹, but youth employment in the eligible age classes did not decline.

One possible explanation for this lies in the rigidity of labour demand by the firms. Once a firm joined the programme, its structure adapted to the new situation (in terms of production volumes, market positioning, and other aspects). The enterprise was faced with a choice between retaining the CFL worker as soon as the contract came to maturity (50% on average were retained³⁰), or shifting to a new worker, but it was unwilling to decrease the employment level, all other things being equal, despite the fact that the financial benefit had been reduced. So treated and control firms do not behave differently as far as the ongoing employment level of the eligible workers is concerned, although new CFL recruitment was only undertaken by artisan firms³¹.

6. Conclusions

The CFL programme was introduced in Italy in 1985, to relieve youth unemployment. The programme provided firms with a substantial reduction in social security contributions and a wage reduction; it was virtually the only fixed term contract available for employers, having a maximum duration of two years; it allowed firms to hire directly without having to go through a government agency. This paper has argued that CFL was conducive to net job creation by firms that took it up because employers that took part in the programme strongly reacted to the fiscal benefits, the opportunities for direct hiring and the determinate duration of the contracts.

Our results complement the study by Contini, Cornaglia, Malpede and Rettore, which measured the impact of the programme on youth employment at the national level through the analysis of temporal and territorial variations in relative labour costs for eligible employers, but found no evidence of a policy impact (Contini et al., 2002). Our richer dataset allows us to evaluate the programme's consequence on for the number of young people hired by the

²⁹ In a couple of years new CFL in Treviso and Vicenza almost halved, Breda (1993, p. 167). If artisan firms did not change their attitude, CFL by non artisan firms were reduced to very small numbers, indeed.

³⁰ Breda (1993, p. 167).

³¹ The result is only in part due to the fact that about 50% of CFL contracts were transformed into open end contracts: the employee stock of untreated firms is stable independent of such transformations.

participating firms and so produce a more informative estimate. Firms reacted promptly to the programme, and the average treatment effect on the treated is statistically significant and positive. The indirect effect of the programme on workers outside the targeted group has been scrutinized and no sizeable substitution effect has emerged, leading us to conclude that the impact of the intervention on the targeted group produced a positive net effect for the economy. In contrast to Contini et al. (2002) our study provides new evidence supporting active labour market interventions, also in the face of many sceptical findings for other European countries (Kluwe, Schmidt, 2002).

The size of the overall effect in Treviso and Vicenza provinces was lessened by the programme's limited diffusion, and this had at least two causes. First, in a very rigid labour market, where terms and conditions of employment were negotiated in detail at the national level, firms took advantage of the new contract form – to separate the wheat from the chaff. Better educated workers or, generally speaking, workers with higher productivity, were offered open-ended contracts and higher entrance wages, while low productivity workers were hired through CFL, on fixed-term contracts and lower wages. Thus, CFL aided the process of screening workers for their job suitability. The occurrence of the programme intervention in a very rigid situation fostered competition among workers, and among firms, and improved the economy's employment performance. This argument is one of the main policy directions identified by the Manpower and Social Affairs Committee of the OECD (Kluwe, Schmidt, 2002, p. 421). Second, the programme was burdened by a series of restrictions aimed at selecting "good" employers. Employers were selected on the basis of an employment history free of recent substantial firings in order to avoid subsidizing firms which might have reduced employment prior to engaging in the programme with the intention of re-hiring previously employed youths on a temporary employment basis. Also, many applications were rejected on the grounds that the proposed training programmes they offered were poor.

The main lesson we can derive from the Italian experience is that the CFL programme had net, but limited, positive results on youth employment. To be an effective programme entry contracts must be framed in a sufficiently general and flexible framework, be able to take account of various forms of labour heterogeneity and skills, and, in order to reach a wide audience, must be relatively simple and not burdened by excessive bureaucratic impediments.

Appendix

Table A1 presents statistics used in the matching procedure, comparing the treated and untreated firms on a number of variables before and after matching for 21-24 year-old workers in 1986. Matching makes the observable characteristics of treated and untreated firms very close.

Tab. A1: Selected variables before and after matching in 1986 for 21-24 year-old workers (standard errors in brackets)

Variable	Before matching		After matching	
	Untreated	Treated.	Untreated	Treated.
Artisan	0.486674 (0.499824)	0.309595 (0.462555)	0.319580 (0.466542)	0.322266 (0.467572)
Size	9.402192 (28.54818)	40.77052 (191.3818)	20.31966 (36.94924)	21.61426 (43.51913)
Firm's age	7.939438 (7.523244)	3.240337 (3.298095)	7.356364 (3.291211)	7.347656 (3.294926)
Number of males	5.768556 (26.83482)	20.05857 (136.6268)	13.57259 (29.23727)	13.89453 (30.03775)
Number of Blue Collar workers	7.505811 (28.44313)	22.18906 (121.1931)	16.27197 (30.34577)	16.85449 (35.37205)
Number of Apprentices	0.934651 (1.529179)	1.750039 (3.037047)	1.355062 (2.407525)	1.34082 (2.261941)
Number of Workers under 20 years old	1.437891 (3.192878)	2.481867 (5.5.35429)	2.376709 (3.554734)	2.453125 (3.455307)
Number of Workers between 21 and 24 years old	1.649114 (6.086053)	3.303712 (10.47834)	3.458745 (5.189902)	3.662109 (4.863844)
Number of Workers between 25 and 29 years old	1.621119 (6.869436)	4.527697 (30.31506)	3.666585 (6.653488)	3.891602 (7.507505)
Number of Workers between 30 and 32 years old	0.768825 (3.503462)	3.284795 (18.13499)	1.730469 (3.90936)	1.900391 (4.673697)
Number of Workers between 33 and 35 years old	0.645664 (3.163205)	3.616331 (19.0575)	1.47583 (3.606509)	1.523438 (4.436523)
Lag Eligible Stock Variation	-0.01924 (1.355094)	1.049493 (2.652389)	0.065104 (2,005692)	0.081055 (1.330987)

Before matching some variables are highly significant and these are the most important variables for selection into the program. When running the same estimation after matching no variable is significant, and this is clear evidence that the balancing procedure worked very efficiently.

Table A2: Coefficients of the logit estimation (p-values in brackets)

Variables	Before-matching	After-matching
Artisan Firm	-1.1502 (0.000)	-0.16381 (0.688)
Size	0.023211 (0.042)	0.07114 (0.102)
Firm's age	-0.08273 (0.551)	0.05029 (0.812)
Number of males	-0.0079 (0.187)	-0.00805 (0.513)
Number of Blue Collar workers	-0.01327 (0.073)	-0.04522 (0.055)
Number of Apprentices	0.072632 (0.196)	0.03765 (0.716)
Number of Workers under 20 years old	0.014904 (0.565)	-0.01115 (0.854)
Number of Workers between 21 and 24 years old	0.027207 (0.246)	-0.0238 (0.66)
Number of Workers between 25 and 29 years old	-0.00094 (0.967)	-0.01043 (0.791)
Number of Workers between 30 and 32 years old	0.0095 (0.828)	0.01247 (0.848)
Number of Workers between 33 and 35 years old	0.035217 (0.429)	-0.06893 (0.430)
Lag Eligible Stock Variation	0.112311 (0.022)	0.03138 (0.721)
Territorial Dummies	Yes	Yes
Sector Dummies	Yes	Yes

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