An Illustration of the Returns to Training Programmes: The Evaluation of the "Qualifying Contract" in France*

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Abstract

We evaluate the labour market outcomes of a French training programme for youth, using a non-experimental sample of individuals who completed their studies (or dropped out) in 1998 and were observed until 2003. We use propensity score matching to estimate the impact of participation on three outcome variables: the net monthly wage, the monthly income and the probability of employment. We find a positive impact of participation on all three outcome variables. Non parametric robustness checks confirmed our results. We explain these results, which contrast with those of previous French studies, by the very strong training content of the programme.

Keywords

active labour market programmes, training programmes for youth, propensity score matching

JEL classification

J68, I28, C14, C21

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The empirical evaluation of active labour market programmes for youth has given rise to a fairly large body of economic literature. Many of these studies focus on European countries, where youth unemployment remains an important (and, as yet, unresolved) issue. One of the explanations put forth for high youth unemployment in contemporary knowledge-based economies is the lack of specific human capital, leading to poor matches on the labour market. This line of explanation has led researchers to dedicate specific attention to training programmes.

In the United States, the evaluation of training programmes often relies on controlled experiments, in order to isolate the causal effect of participation on labour market outcomes. However, this practice is still far from widespread in Europe, as public authorities and public opinion are only beginning to accept them. Many European studies have to rely on non-experimental data, and use alternative estimation techniques to try and isolate the causal effect of training policies.

In this paper, we evaluate the impact of participation in a French training programme for youth on wages, income and the probability of employment, using (non-experimental) survey data. The data is a representative sample of all individuals who completed their initial training in 1998 (including drop-outs). Individuals are observed until 2003, and we can identify those who participated in the programme during this period. Using this data instead of Labour Force Survey-type data has at least two advantages: First, it allows us to focus on youth entering the labour market for the first time, which reduces selection problems. In particular, it allows eliminating problems arising from differences in labour market histories. Second, it allows us to focus on the targeted training scheme, and to isolate its precise effect, which previous French studies failed to do for lack of appropriate data.

The paper is organized as follows: Section 1 states the objective of our research and emphasizes its relevance in the context of the existing literature. Section 2 describes the training scheme we are interested in. Section 3 details our econometric approach, while Section 4 presents the data. Section 5 gives the results of the analysis, together with some robustness checks. Conclusions are given in the final section.

1 Objective of the Research

We want to evaluate the impact of participation in a French training programme for youth on wages, income and the probability of employment, using a large non-experimental dataset. While the use of controlled experiments to evaluate training programmes is commonplace in the United States (e.g., Manski and Garfinkel, 1992; Heckman et al., 1999), it is still not the case in Europe, where the legal context is less appropriate. Therefore, studies focusing on European countries rely primarily on non-experimental data. They use various estimation techniques on various outcome variables to assess the impact of training policies (see Smith (2000) for a survey). On the whole, evaluations of training policies for youth have yielded mixed results.

For instance, Larsson (2003) evaluates two training programmes for youth in Sweden: youth practice and labour market training. Using propensity score matching, she evaluates their impacts on three different outcome variables: earnings, the probability of employment, and the probability of a transition from unemployment to studies. She finds negative short-term impacts (one year after participation) on earnings and employment, and no significant medium-term impacts (two years after participation).

Using the same econometric methodology for Finland, Hämäläinen & Ollikainen (2004) evaluate the impact of labour market training, youth practice and employment subsidies on six outcome variables, and in particular employment and earnings. They find positive and persistent impacts of employment subsidies on both employment and annual earnings, up to four years after participation. Labour market training has a positive and persistent impact on annual earnings, but its impact on the probability of employment is not persistent. Finally, the authors do not find any significant effect of youth practice on any outcome variable in the period considered.

In Norway, Hardoy (2005) has studied employment programmes, training programmes (classroom training), vocational programmes (combination of on-the-job training and off-the-job training) and combinations of these. Using maximum-likelihood estimators, she estimates the impact of these various programmes on the probability of employment and on the transition to education, five years after participation. Her conclusions are rather pessimistic: training programmes always lower the probability of employment, irrespective of gender or age group. Vocational programmes increase the probability of part-time (versus full-time) employment for females, and lower the probability of employment for the 16-20 years old (precisely the age group which they target).

In France, there have been relatively few studies dedicated to the evaluation of training programmes, and especially training programmes for youth (see Fougère *et al.* (2000) for a survey). Bonnal *et al.* (1997) study the impact of training programmes on transitions to employment and unemployment using a sample of male youth aged 16 to 26 in 1986, and observed from 1986 to 1988. This non-experimental, longitudinal dataset comes from the French survey on the unemployed conducted by the National Institute for Statistics and Economic Studies (INSEE). Estimating a mixed proportional hazard model on these data, the authors find that training programmes (as a whole and contrasted to other policies) have positive impacts on transitions from unemployment to open-ended employment, especially for low-skilled workers. They also find that training programmes increase the expected average duration of subsequent employment spells. However, their research interest (which lies in confronting different types of policies) and the broadness of their data lead these authors to pool together different existing training programmes.

Using the same data source under a different sample selection rule, Brodaty *et al.* (2000) study the impact of training programmes on the transition to employment for youth (both male and female) aged 27 or less in 1986 (and observed until 1988). Again, these data do not allow the authors to distinguish between different types of training programmes, or to focus on a specific training scheme. They do, however, distinguish between different types of active labour market policies, classified in four broad categories: "Workplace training" (which regroups different training programmes), "Courses for preparation to the working life", "Community jobs" and "Other programmes". They arguably consider jobs with fixed-term labour contracts as a fifth programme type. The authors analyse the effects of each type of programme relative to the other types using propensity score matching estimators. However, the small number of transitions to workplace training in the authors' dataset does not allow them to make inference (with matching estimators) on the relative effectiveness of these programmes.

Magnac (2000) uses non-experimental, monthly longitudinal data from the 1990-92 wave of the French Labour Force Survey to study the labour market transitions of youth aged 18-29 at that time. Estimating a dynamic multinomial Logit model, he evaluates the impact of training policies (an aggregated category which includes different training programmes) on the probability of employment. Failing to find a significant impact, the author concludes that "training schemes do not seem effective in promoting access to *stable* employment" (Magnac (2000), p. 808, we emphasize). According to him, this result contradicts the argument that training schemes impact on labour market histories through an increase in human capital investment: they would rather appear to be a temporary refuge against unemployment (especially for young people), that protect the unemployed against losses in human capital, until better times come.

This interpretation might explain why training programmes often have weak (and sometimes negative) impacts on employment or earnings. According to Magnac (2000), programmes that have a large training content might be an exception to this rule. In the remainder of this paper, we want to examine this assertion further, by focusing on one specific training scheme in which training is sanctioned by a diploma. This scheme, which relies on part-time work in a firm and part-time training in a teaching institution, is not designed to merely prevent the loss of human capital: it specifically targets the acquisition of human capital. By granting diplomas (similar to those obtained in teaching institutions) to participants, it also makes the acquisition of human capital observable by potential employers.

Indeed, it is generally admitted in the literature that firms tend to consider diplomas as a reliable indicator of job seekers' human capital. By contrast, participation in a training programme in the past does not provide an employer with accurate information on an applicant's level of human capital. The programme we study in this paper may not suffer from such a flaw, because it provides participants with well-known diplomas. It may therefore have positive effects, where many other training schemes fail to do so.

2 Qualifying Contract

The training programme on which we focus in this paper is the "Qualifying Contract", hereafter CQ[1]. The CQ programme is a widely-used training programme for youth in France. Its most important aspect, for the present study, is that it has a large training content and can lead to a formal diploma. It is therefore the programme of choice for an empirical investigation of the impact

[1]This abbreviation stands for the French denomination of the programme: "Contrat de Qualification".

of training programmes with important training contents, as suggested at the end of the previous section[2].

The CQ programme was created in 1984 and is addressed to individuals aged 16 to 25, who either *(i)* did not acquire any diploma during their schooling, or *(ii)* acquired a low-level diploma that does not help them in finding a job. The primary objective of the CQ is to allow youth to acquire a higher (or more "recognised") diploma through formal training. It naturally targets the less educated, which results in the following eligibility conditions in terms of education level: all unemployed youth are automatically eligible except (i) those holding a vocational/technological High School diploma and (ii) those holding a Higher Education diploma. Youth belonging to the last two categories can nevertheless be allowed to apply for a CQ programme, if they manage to prove severe difficulties in finding a job.

Legally, the CQ training programme takes the form of a fixed-term labour contract between a youth and a private employer (i.e., a firm: there is no CQ training scheme in the public sector). The contract lasts from 6 to 24 months, and can be renewed one time only. The specific feature of this contract is the emphasis put on formal training (as opposed to informal or "on-the-job" training). As a rule, this training usually occurs in a private or public teaching institution (such as a high school) with which the firm has signed a training convention. Training can only take place within the firm if the latter has a training facility certified as a training institute. Besides being formal, the training received during a CQ programme also represents an important amount of time: training duration has to be at least 25% of the total duration of the contract. Last but not least, the training received during a CQ is sanctioned by a diploma, which can be: (1) a vocational or technological diploma similar to those delivered in French teaching institutions, like a BTS or a DUT (see Appendix 1 for a review of these diplomas); or (2) a vocational diploma recognised in the collective wage agreement that prevails in the industry where the CQ programme took place.

The selection process into a CQ in 1998 was as follows: an applicant willing to participate in a CQ had to turn to the local employment agency of her area to inform them of her interest. Employers interested in hiring youth on a CQ did the same thing. It was then up to the local employment agencies to match youth and employers. The final decision to accept or reject an application did belong to employers. With the development of Internet, the role of local agencies has gradually decreased: they ended up simply publicising vacancies offered as CQ, without sorting nor matching the applications. In that context, it is up to youth to apply, and to employers to decide about who they hire.

Participants in the CQ programme are evaluated in order to determine which type of diploma (and which education level) they will be granted at the end of the programme. At the beginning of the programme, a tutor, recruited among the personnel of the firm, is allocated to the participant. The tutor's mission is to act as a mentor for the youth, to help her and advise her for the duration

[2] Apprenticeship contracts also have an important training content. However, being part of initial education, they can be set apart from active labour market policies, and will not be examined here. The returns to apprenticeship training have been evaluated by McIntosh (2004) in the UK and by Sollogoub & Ulrich (1999) in France.

of the programme. The tutor intermediates between the firm, the participant, and the teaching institution, and participates in the youth evaluation. To be designated as a tutor, an employee must have at least two years of labour market experience. The tutor is generally an experienced worker, with a certain length of job tenure within the firm. He/she supervises the participant in a more or less informal manner, at no extra cost for the firm. A tutor cannot supervise more than three CQ participants at a given time.

Applicants for a CQ programme expect to accumulate valuable human capital. This appears to be their prime motivation: in a sample of 1631 individuals who participated in a CQ in 1999 (and were surveyed in 2002), eight youth out of ten declared they applied because they wished to acquire labour market experience and/or a formal training (DARES (2004)). However, these youth expected more than just the acquisition of knowledge and know-how. According to DARES (2004), more than 50% of the sample also applied in order to improve their financial situation. Indeed, participants in a CQ programme are paid a given percentage of the minimum wage[3], according to their age and tenure within the firm. This compensation scheme is summarized in Table 1.

Table 1 about here

The CQ programme also provides a number of financial incentives to firms. First, employers who hire a young person on a CQ training programme are exempt of social security contributions for this particular job. They can also ask for the reimbursement of training costs, up to 9.15 euros per hour (or deduct these training costs from their annual mandatory training expenses). In addition, employers who hire less-educated youth (i.e. youth who did not study beyond Junior High School) receive a premium of (i) 1050 euros if the duration of the contract is 18 months or more, or (ii) 750 euros if the duration of the contract is less than 18 months. As a labour contract, the CQ programme is therefore useful for firms that have low-skilled vacancies to fill, especially for a short period of time. With a CQ, labour costs are kept low (thanks to the exemption of social security contribution), and training is conducted at no extra cost for the firm (since training costs are refunded). Moreover, this contract can provide firms with skilled workers that may be hired afterwards (on other types of labour contracts) if need be[4].

Let us conclude this section with a few important remarks. First of all, in spite of official regulations, CQ programmes have been offered to youth who were older than 25 at the beginning of the contract. According to DARES (2000), 7.8% of participants in a CQ programme in 1998 were aged 25 or more at the beginning of the contract. This fact has been acknowledged by public authorities, and has led to a change in the regulation in 2004. The new regulation allows to hire either youth aged 16 to 25, or unemployed persons aged 26 or more. Our analysis concerns the original CQ programme, as it was designed until 2004, and not the post-2004 modified version.

[3]In France, the minimum wage is set at the national level, and not at the industry level.

[4]Surveys conducted by DARES (2000, 2004) suggest that roughly one third of participants found a job in the firm which granted them the CQ, although that figure cannot be measured accurately.

Rules regarding the initial education level of the participants were even easier to bend than those regarding age: although the CQ programme targets youth with low education levels, it leaves room for the recruitment of educated youth. As mentioned previously, young persons with a High School or University diploma can access a CQ programme, provided they can prove that they encountered severe difficulties in finding a job. According to DARES (2000), 20.2% of participants in a CQ programme in 1998 had a higher education diploma before starting their contract. For the majority of them, however, it was a 2-year higher education diploma[5] (only 3.2% had 3 years of higher education or more).

Finally, even though the primary target of the CQ programme was originally *unemployed* youth, recruitment into the programme directly after graduation from initial education has gradually become widespread. According to DARES (2000), 31.5% of participants in a CQ programme in 1998 were recruited directly after graduating. These various observations are interesting for the econometrician, as they reduce the scope for selection bias in the analysis. Indeed, a CQ programme can be offered to almost any young school leaver, whatever her age and education level, and regardless of whether she experienced unemployment after graduating.

3 Econometric Analysis

3.1 Estimation strategy

We evaluate the impact of the CQ on individuals' earnings and labour market situation using propensity score matching, and perform (non-parametric) robustness checks. We conduct our analysis in three steps. The first step consists in estimating the probability of participation in a CQ. We define an indicator variable D_i, equal to 1 if individual i has participated a CQ, and to 0 otherwise. Our data tell us whether an individual has participated in a CQ between the end of her studies in 1998 and the date of the survey in 2003. We do not, however, know the duration of the CQ, or the precise starting and end dates of the contract. To estimate the probability of participation, we specify a binary Probit model[6]:

$$p_i = Pr(D_i = 1|X_i) = \Phi(\beta X_i) \tag{1}$$

where X_i is a set of explanatory variables characterizing individual i during the course or at the end of her studies. The estimated probability $\widehat{p_i}$ defines individual i's *propensity score* (see Section 3.2).

The second step of the analysis consists in matching treated individuals with individuals from the control group, on the basis of their propensity scores, in order to estimate non-parametrically the Average effect of the Treatment on the Treated (hereafter ATT). We consider three outcome variables: net monthly wage in 2003 (set to zero for unemployed individuals), monthly income in 2003 (i.e. net monthly wage for employed individuals, and benefits for the non-employed individuals), and a binary indicator of employment in 2003. We implement various non-parametric matching algorithms: *(i)*

[5]This includes DEUG, BTS and DUT. These diplomas are described in detail in Appendix 1.

[6]This specification is the most frequently used in the literature, although some papers rely on a binary Logit model.

the nearest-neighbour estimator, *(ii)* the n nearest-neighbours estimator with n = 5, 10, and *(iii)* the kernel estimator. We enforced a common support by dropping control observations for which the propensity score was either higher than the maximum or less than the minimum propensity score of the treated group. We give more details about the principles of propensity score matching in Section 3.2.

The third and final step of the analysis consists in testing the robustness of our results to possible failures of the main assumption of matching, the Conditional Independence Assumption (CIA). The idea of this non-parametric robustness check (presented in Ichino *et al.* (2008)) is to consider that this assumption does not hold given our observable variables but that it would if we could observe an additional binary variable. We then include this variable in the set of variables used in the matching process and re-estimate the ATT. The comparison between this estimate and the baseline estimate gives an idea of the robustness of our results to possible failures of the CIA. The details of the procedure are given in Section 5.3

3.2 Propensity score matching

Propensity score matching allows for the non-parametric estimation of the effect of a "treatment" (participation in a training programme, for instance) on an outcome variable, such as earnings. Economic studies dealing with evaluation problems have come to rely increasingly on this methodology in recent years. In this framework, the main problem is that of missing information. Let D be a random variable taking two values: 1 if the individual has been treated and 0 if she has not. Let X be a vector of observed covariates and Y be the outcome variable. In the data, we only observe Y_1 if the individual has been treated ($D = 1$) or Y_0 if she has not ($D = 0$).

In that framework, the ATT is defined as the expected difference between Y_1 and Y_0, conditional on $D = 1$, i.e.:

$$\Delta^{TT} = E(Y_1 - Y_0|X, D = 1) = E(Y_1|X, D = 1) - E(Y_0|X, D = 1) \tag{2}$$

The missing information problem concerns $E(Y_0|X, D = 1)$: we never observe Y_0 in the data when $D = 1$. In other words, we never observe the outcome variable of a treated individual had she not been treated. This counterfactual will have to be estimated. To do so, it is common to use the information given by $E(Y_0|X, D = 0)$. However, the use of this information without precautions may cause biased estimations of Δ^{TT}. Hence, it is necessary to guarantee that, given X, the treated outcomes would be what the non-treated outcomes are had they not been treated (this is the conditional independence assumption, or CIA, and is written $Y_0 \amalg D|X$).

The CIA implies that selection occurs only on observables and is eliminated when accounting for X. Under the CIA, $E(Y_0|X, D = 1) = E(Y_0|X, D = 0)$ and thus we can use the latter to estimate the counterfactual. However, as conditioning on X would be a computational burden (due to the dimensionality of the vector of covariates), another approach is normally used: following Rosenbaum & Rubin (1983), if the CIA is valid for X, it is also valid for any given function of X, which can be written: $Y_0 \amalg D|b(X)$. A commonly used function is the one-dimensional propensity score P(X), which is simply the probability of being treated conditional on X:

$$P(X) = Pr(D = 1|X) \tag{3}$$

Rosenbaum & Rubin (1983) show that, if $P(X) < 1$, the matching between treated and control individuals can be done solely on the basis of their propensity scores, because:

$$E(Y_0|P(X), D = 1) = E(Y_0|P(X), D = 0) \tag{4}$$

For matching individuals on the basis of their propensity scores, several non-parametric algorithms are available. In this paper, we use: *(i)* the nearest-neighbour estimator, which matches treated observations to the closest control; *(ii)* the n nearest-neighbours estimator, which matches treated observations to the n closest controls (we first set $n = 5$, then $n = 10$); *(iii)* the kernel estimator, which attributes weights to control observations according to their relative proximity to the treated observation, the relative proximity being based on the propensity scores P(X).

Heckman *et al.* (1997) emphasize that the performance of propensity score matching as an econometric estimator depends on both the quality of the data and the properties of the estimator. Regarding data, it is crucial that treated and control group members answer the same questionnaire: this ensures that outcome and explanatory variables have the same definitions across groups. Moreover, when the application concern the effect of a treatment on labour market outcomes, it is preferable that treated and control group members be located in the same labour market. Without anticipating on the next section, let us remark that our data fulfil both criteria: both groups answered the same questionnaire, and belong to the same labour market (the labour market for youth leaving initial education for the first time in 1998). We also have a fairly large number of observables to control for further differences between both groups. This pleads for the use of propensity score matching as the methodology of choice in the present paper.

However, the quality of the data is only half of the story: the estimation method also matters. In particular, Heckman *et al.* (1997), state p. 607 that “simple balancing of observables in the participant and comparison group samples goes a long way toward providing a more effective evaluation strategy”’. More precisely, these authors show that evaluation biases include (i) the selection bias as such, (ii) the bias due to non-overlapping supports of X in the two groups, and (iii) the bias due to different distributions of X within the two groups. If matching is performed on the common support of the distributions, and if the distributions of X within the two groups are less different after matching (balancing property), the bias due to selection on unobservables becomes negligible. The quality of our estimations in that respect will be thoroughly discussed in Section 5.

4 Data

4.1 The Generation 1998 survey

Our data comes from the "Generation 1998" national survey, conducted by the Cereq[7] (Centre for Research and Studies on Employment and Skills, based in Marseilles), by means of telephone interviews. This survey yielded a representative sample (by gender and diplomas) of all individuals aged below 35 years old who left the French education system in 1998 and *did not go back* to schooling during that year. There were two waves of the survey: the first one, conducted in 2001, yielded 55345 answers, and the second, conducted in 2003, yielded 22021 answers out of the initial 55345.

Our study will focus on the 22021 individuals who were present in both waves of the survey. This sample provides a longer observation period, which is preferable for our analysis. Since a CQ programme can last as long as two years and can occur at any time during a individual's labour market history, it is better to have a 5-year (rather than a 3-year) period to measure the effects of participation. The strong attrition observed between 2001 and 2003 induces a selection bias, which is apparently not too severe. A cursory look at Table 2 suggests that the distributions of key variables are extremely close in both waves of the survey: mean values are generally equal at the 2-digit level, although standard deviations do vary. A formal test confirms that first impression, by showing that differences in means are never significant.

Table 2 about here

The Generation 1998 database contains very detailed information, which is important for matching. It primarily includes information on individuals' initial education. In particular, the type of diploma obtained in 1998, and the discipline (or specialization in case of vocational training) in which each individual graduated are both described in details. It also includes information on individuals' characteristics such as age, gender, number of children, geographical location in 1998, and family background. Finally, the Generation 1998 database includes a monthly calendar covering the years 1998-2003 and describing each individual's labour market history.

Although periods of training are reported in this calendar, the information is too general for our purpose: it does not allow us to identify participation in a CQ programme. Indeed, training reported in the calendar does not focus on training schemes, but aggregates very different situations, from on-the-job training to an individual's decision to go back to regular education (and temporarily leave the labour market)[8]. In order to identify participation in a CQ programme, we had to rely on another question, specifically devoted to participation in that training scheme. Unfortunately, this question does not give us the precise dates of beginning and end of the CQ programme, although we can identify individuals who were still in a CQ programme in 2003, at the time of the latest survey.

[7]For a description of the Cereq in English, go to: http://www.cereq.fr/publicpoleofexpertise.htm

[8]Note that given the sampling scheme of the Generation 1998, the decision to go back to regular education can only occur after January 1999.

We focus on the 22021 individuals who were observed in both waves of the Generation 1998 survey. We eliminated individuals who were participating in a CQ at the time of the survey (i.e. in 2003), as they could neither be considered as treated nor as controls in our analysis. We also eliminated some outliers with aberrant or extreme monthly wage values. This selection process left us with 17873 individuals, but did not lead to a strong selection bias. Indeed, Table 2 shows that the distributions of key variables (as characterized by mean values and their standard deviations) are similar in the second wave of the survey and in the selected sample. A formal test confirms that differences in means are not significant.

Among the 17873 selected individuals, 732 (i.e., about 4%) did participate in a CQ training programme. As was said above, we were able to identify individuals who participated in a CQ between 1998 and 2003, but we do not know the duration of their participation. We will therefore be able to evaluate the impact of participation in a CQ on our various outcome variables, but not the impact of *participation duration*. This has the following implications for our analysis: given that individuals can participate at any time during 1998 and 2003, the treated group is more likely to consist in individuals who had greater difficulties in finding a conventional job during that period. By contrast, individuals in the control group may not have faced such difficulties. Therefore, our estimate of the ATT could be considered as a "lower bound" of the actual ATT, as suggested in Fredriksson & Johansson (2008).

4.2 Definition of variables

We retained three outcome variables: first of all, the net monthly wage reported at the time of survey, in 2003. We set this wage equal to zero for individuals who were unemployed or inactive at that time. Our second outcome variable is the monthly income in 2003. This variable is equal to the net monthly wage if the individual is employed, and to net social allowances of she is not. "Net social allowances" is a survey variable reporting (as an aggregate) all benefits received by non-employed individuals, including not only unemployment benefits, but also all other social benefits (such as invalidity pensions or children allowance). It can therefore be considered as a source of income for inactive as well as for unemployed individuals. Our third and final outcome variable is an indicator of employment at the time of the survey, i.e. in 2003.

The variables we use to explain participation in a CQ training programme include, first of all, individual characteristics: gender, age in 1998, and nationality (coded as a binary variable: French/non-French). We also considered the diploma obtained in 1998, using a very detailed variable. We give more information on French diplomas in Appendix 1.

We use three proxy variables to control for differences in family background: father's labour market situation in 1998 (coded in six categories), mother's labour market situation in 1998 (coded in the same categories as the father's situation) and an indicator of the father's nationality (coded as French/non-French). Two variables give information about individuals' family situation in 1998. The first variable distinguishes between four mutually exclusive situations in 1998: living at one's parents (with or without a partner), being single (and living in one's own home), being part of a couple (and not living at one's parents), and "unknown" situations. The second variable indicates whether an individual had children in 1998.

We also include a series of five binary indicators of reasons why an individual did not go on with her studies. These indicators are not mutually exclusive, since an individual may decide to stop studying for several reasons (and individuals were not asked to rank their reasons). The reasons we are able to identify are: tiredness, financial difficulties, success in finding a job, attaining a desired education level, and failing to enrol in a higher-level training track (e.g., failing to enrol in a Master after a B.A.). Four indicator variables allow us to control for vocational experience during an individuals' initial training: internships, regular job[9], occasional jobs during holidays, and occasional jobs while studying.

Finally, we included two additional control variables: the first is an 83-categories indicator variable that gives precise information about the specialization of an individual's education or initial training. Specialization is formally defined as a disciplinary field (in the case of general training), or as a vocational/professional area of expertise (in the case of vocational training). Examples of disciplinary fields include "Chemistry", "Geography", "Law", or "Economics", while examples of vocational areas of expertise include "Animal Production and Cattle Raising", "Woodcraft", "Construction Work", "Cuisine", or "Retail and Sales". The categories of specialization used in the Generation 1998 survey are the NSF categories[10] that were formally defined in the French Decree of June 21st, 1994.

The other variable is a 350-categories indicator of an individual's geographical location in 1998, at the end of her studies. The geographical location is defined in the Generation 1998 at the "employment area" level. An employment area is a geographical unit defined by the French National Institute for Statistics and Economic Studies (INSEE) as a "geographical area within which employed individuals both reside and work". This partition of the French territory is based on the distance between residences and working places, and is therefore more relevant for labour market studies than administrative divisions. It is also more detailed, as there are currently 348 employment areas in France (each one comprising at least 25000 employed individuals). Our indicator accounts for all 348 employment areas, and adds two categories: one for individuals located in overseas territories or abroad in 1998, and one for individual whose employment area in 1998 is unknown (there are only 158 such individuals).

4.3 Description of the treatment and control groups

Summary statistics for all of our outcome and explanatory variables are presented separately for the treated and control groups, in Table 3, along with a test for differences in means. This table suggests that treated individuals have, on average, lower earnings and income than controls; however, both groups have the same proportion of employed individuals. The treated and control groups differ

[9] A regular job is defined in the survey as a job taken while studying and used as a source of income (sometimes to finance an individual's education, although we do not know the proportion of individuals financing their studies in this way). These are generally low-skill, low-pay jobs, such as the so-called "Mac jobs".

[10] NSF is the French acronym for "Nomenclature of Training Specialization". The French Centre INFFO (http://www.centre-inffo.fr/international/) gives a list of the NSF categories, although this list is so far available in French only: http://www.centre-inffo.fr/Nomenclature-des-specialites-de.html.

in terms of diplomas (most differences are significant). There are more unskilled individuals and individuals with general *baccalauréats*, as well as more individuals with low-level vocational diplomas (such as CAP/BEP) among the treated[11]. Conversely, there are fewer individuals having completed 3 to 5 years of higher education among the treated. Since the CQ training programme offers a "second chance" of attaining a desired diploma or education level, this does not come as a surprise.

Table 3 about here

On average, treated and control individuals have similar family backgrounds, but differ in terms of family situation in 1998: there are fewer treated individuals who are part of a couple, and more treated individuals living at their parents'. The reasons for terminating initial education in 1998 differ across groups: there are, among the treated, more individuals who failed to enrol in a higher-level training track, and fewer individuals who reached the education level they desired. This may have resulted in dissatisfaction, and the CQ programme may have been a way to attain the educational objectives that some of the treated individuals had to forego in 1998. Finally, treated and control individuals do not differ much in terms of vocational experience during their initial training or education: none of the differences is significant. This observation suggests that the CQ programme may be used primarily by individuals who want to reach a given education level, rather than to acquire additional labour market experience.

Since the control variables "training specialization" and "employment area" both have a very large number of categories (83 for the former and 350 for the latter), we do not present detailed summary statistics about these variables[12]. Instead, we give a graphical representation of the distribution of these variables in the treated and control groups. Figure 1 shows the trends in the distribution of "training specialization" in the treated and control groups (as dotted and continuous lines respectively). The distribution of this variable is very similar in both groups, with two noticeable exceptions for categories number 60 and 72. These two peaks mean that there are more individuals specialized in "Sales" in the treatment group, whereas there are more individuals with a specialization in "Health" in the control group. Figure 2 shows the trend in the distribution of the "employment area" as a black surface for the control group, and as a white, dotted surface for the treated group. Overall, the distribution of this variable looks rather similar in both groups.

Figure 1 about here

Figure 2 about here

5 Results

5.1 Estimation of the propensity score

As explained in Section 3.1, the first step of our empirical analysis was to estimate the propensity score, i.e. the probability to participate in a CQ training programme conditional on the observable

[11]See Appendix 1 for a more thorough description of French diplomas such as *baccalauréat* and CAP/BEP.
[12]Detailed frequency tables for these two variables are available upon request from the authors.

variables. We estimate the propensity score using a Probit model, with the vector of explanatory variables described in Section 4.3 (and summarized in Table 2 and 3). The results of the estimation are presented in Table 4, in the form of marginal effects. Before commenting these results, three remarks are in order. First, we have tested for the correct specification of the model, using the method presented in Wooldridge (2002) (p. 464-465) and we cannot reject the assumption that the model is correctly specified. Second, for the sake of concision, we do not report detailed results for the two control variables "training specialization" and "employment area", which are included as fixed-effects in our Probit model. Instead, we report an overall test of statistical significance for each one of these fixed effects. We find that specialization globally affects the probability of participation, whereas the location in 1998, measured at the employment area level, does not. Yet, we do not exclude this variable because we consider it to be important to account for local labour markets. Third, one might ask if the inclusion of such detailed variables, along with variables that are statistically insignificant, is not explaining "too well" the propensity score. If it were the case, the estimated propensity scores of the treatment and control groups would be very close to, respectively, 1 and 0 and the common support of these propensity scores would be very narrow. In our case, only 20% of the observations of the control group are outside the common support, which means that we do not have to worry about the specification of the Probit model being "too good".

Table 4 about here

According to Table 4, the probability of participation depends strongly on diplomas: the *baccalauréat* (the French diploma that concludes High School and grants access to higher education) being the category reference, almost all other diplomas are associated with a lower probability of participating in a CQ programme. This result makes perfect sense, as the *baccalauréat* is a diploma which opens the doors to higher education, but does not give the recipients any vocational skill (see Appendix 1). Individuals who leave education at this level may therefore have poor perspectives on the labour market, and may be willing, after a few months of unsuccessful job search, to enter a CQ programme. Another result reinforces this interpretation: the probability of participation for recipients of a 2-year general university diplomas (a.k.a. DEUG) is not significantly different from that of general *baccalauréat* recipients. Again, DEUG are diplomas which allow recipients to pursue further higher education, but do not give any vocational skill.

Individual characteristics also affect the probability of participation: men are more likely to participate in a CQ, whereas individuals who were older in 1998 are less likely to participate. The impact of family background seems somewhat less important: individuals who had a deceased parent in 1998, as well as those whose mother was inactive in 1998, are less likely to participate, whereas individuals whose father was retired in 1998 are more likely to participate. Interestingly, the father's nationality does not influence the probability of participation, and neither does the individual's nationality: therefore, discrimination on the basis of nationality does not seem to be an issue here. Finally, individuals who were single (and living in their own home) in 1998 are more likely to participate in a CQ.

Individuals who ended their studies because they were tired of studying, because they faced financial difficulties, because they found a job or because they were satisfied with the education level they had reached all have a lower probability of participation. However, the probability of participation is not significantly higher for individuals who had to terminate their studies because they failed to enrol in a higher-level training track. Among our four proxies for vocational experience acquired during an individual's initial training, only one significantly affects the probability of participation: occasional jobs during holidays are associated with a higher probability of participation.

5.2 Estimation of the ATT using propensity score matching

The second step of our analysis consisted in estimating the ATT (i.e. the impact of participation in a CQ on the outcome variables) by matching individuals on their estimated propensity scores. Table 5 presents the results of these estimations, for each outcome variable: the net monthly wage in Column (I), the monthly income in Column (II), and the probability of employment in Column (III). In each column, four estimates are given, together with standard errors and the absolute value of Student's t. These estimates correspond to the following non-parametric matching algorithms: nearest neighbour, 5-nearest neighbours, 10-nearest neighbours, and kernel[13]. Abadie & Imbens (2006b) show that bootstrap is not a valid method to obtain standard errors for nearest neighbour estimators with a fixed number of matches. For that reason, we use the estimators proposed in Abadie & Imbens (2006a) and Abadie *et al.* (2004). For kernel, we present bootstrapped standard errors.

Table 5 about here

When the outcome variable is the net monthly wage, Table 5 shows a positive ATT that is always significant at the 1% level. This means that participation in a CQ between 1998 and 2003 results in a higher net wage in 2003. When the outcome variable is the monthly income, Table 5 also shows a positive ATT with different levels of significance. The use of income instead of wage as the outcome variable always leads to a smaller ATT (for a given matching algorithm), which is not so surprising, since the monetary resources gap between the employed and the non-employed becomes smaller in that case. When the outcome variable is the probability of employment, Table 5 also shows a positive ATT, which suggests that participation in a CQ programme after 1998 and before 2003 increases the probability of employment in 2003. However, the ATT is not significantly different from zero when estimated with the nearest neighbour algorithm, whereas it is significant at the 1% level when estimated with any of the other three algorithms. It is also interesting to note that, for the three outcome variables, the value of the kernel estimator is always between that of the nearest neighbour and that of the 5-nearest neighbours and that the magnitudes of the estimates increase with the number of matches, for the nearest neighbour algorithms.

Overall, our propensity score matching analysis leads to the following conclusion: participation in a CQ training programme has a positive impact on the net monthly wages, on the monthly income

[13] We use an Epanechnikov kernel function with a bandwidth of 0.06. Other kernel functions were tested but all yield very similar results.

and on the probability of employment. Moreover, given that our estimate could actually be a lower bound of the "true" ATT (as stated in Section 4.1), we can reasonably conclude that participation in a CQ strongly improves the labour market prospects of participants. These results are in line with the analysis of Magnac (2000) mentioned in Section 1: the CQ programme, which undoubtedly has a strong training content, also leads to favourable labour market outcomes.

Everything indicates that the analysis is reliable and that the performance of the matches is good. First, the Probit model used to estimate the propensity score is correctly specified. Second, the common support includes 80% of the control group (for the kernel estimator). Finally, the balancing property is verified, as can be seen in Table 6. For all algorithms, there is a reduction in the average absolute standardised bias[14] before and after matching.

Table 6 about here

Although the four algorithms presented in Table 5 yield similar results, the magnitudes of the ATT are not the same and, furthermore, some of the estimates obtained with nearest neighbour matching are not significant. In this situation, it seems necessary to choose one preferred estimator out of these four. This is essential if one wants to provide a quantitative (rather than merely qualitative) answer to the question of the effects of participation in a CQ programme.

Our preference goes to the kernel estimator, for several reasons. One reason is that, as mentioned in Abadie & Imbens (2006a), the large sample properties of nearest-neighbour estimators with a fixed number of matches have not been established in many cases. Moreover, these authors have shown that some of these large-sample properties are not very attractive: for instance, these matching estimators are not $N^{1/2}$-consistent in general (N being the sample size). By contrast, Heckman *et al.* (1998) have developed a sampling theory for kernel-based matching estimators, and established their large sample properties. They have shown in particular that these estimators are convergent and $N^{1/2}$-consistent.

Focusing on our kernel estimates, we can say that participation in a CQ programme increases the net monthly wage by 84 euros and the net monthly income by 51 euros (both estimates being rounded to the nearest integer). Participation also increases by 4.5% the probability of being employed in 2003. However, these estimates are only valid insofar as the CIA holds. In the next section, we examine the robustness of our preferred estimates to possible failures of the CIA.

5.3 Non parametric robustness checks for possible failures of the CIA

We have been very careful to conduct our estimations under conditions where the CIA is most likely to hold. We ensured that the same questionnaires were used for both treated and control groups and controlled for selection on observables using a fairly large number of very precise variables. Nevertheless, unobservable variables might still be influencing our results. While we cannot control

[14] The average absolute standardised bias is formally defined as the difference between sample means in the treatment and control groups as a percentage of the square root of the average of the sample variances in both groups.

for unobserved heterogeneity in our matching framework, we can examine the robustness of our results to possible failures of the CIA, using the non parametric robustness check introduced by Ichino *et al.* (2008).

The idea of this methodology is to consider that the CIA does not hold given our observable variables but that it would hold if we could observe an additional binary variable U (called confounder from here onwards). Formally, we have that $P(D = 1|Y_0, Y_1, X) \neq P(D = 1|X)$ but $P(D = 1|Y_0, Y_1, X, U) = P(D = 1|X, U)$. If we write $Y = D.Y_1 + (1 - D).Y_0$ and $U \amalg X$, then the four parameters that characterize the conditional distribution of U are:

$$p_{ij} \equiv P(U = 1|D = i, Y = j, X) = P(U = 1|D = i, Y = j) \quad (5)$$

with $i, j \in \{0, 1\}$. These parameters give the probability that $U = 1$ in each of the four groups defined by the treatment status D and the outcome variable Y. For our two continuous outcomes, wages and income, we adapt this methodology on the basis of a binary transformation of the outcome: $Y = 1$ if earnings (income) are higher than average earnings (income) and $Y = 0$ otherwise[15]. After attributing values to these parameters, it is possible to predict a value of U for each individual in those four groups, which we then include in the set of the covariates and use to estimate the propensity score and the ATT, using an Epanechnikov kernel estimator. We repeat this process several times (here, we will have 99 replications[16]) and the estimated ATT will be an average of the ATTs for all replications. The comparison between this estimated ATT and the original one gives an idea of how robust are our initial results to possible failures of the CIA. As mentioned in the previous section, we only perform this non parametric robustness check on the kernel estimator of the ATT.

The key to this methodology is to set the "right" parameters p_{ij}. As pointed out by Ichino *et al.* (2008), a potentially dangerous confounder for a positive baseline estimate is one which has a positive effect both on the untreated outcome and on the selection into the treatment because the existence of such a confounder could result in a positive ATT estimate, even without a causal effect between treatment and outcome. They analytically show that having $d = p_{01} - p_{00} > 0$ implies a positive effect on the untreated outcome, i.e. $P(Y_0 = 1|D = 0, U = 1, X) > P(Y_0 = 1|D = 0, U = 0, X)$ and that having $s = p_{1.} - p_{0.} > 0$[17] implies a positive effect on the selection into treatment, i.e. $P(D = 1|U = 1, X) > P(D = 1|U = 0, X)$. Hence, we should focus our attention on confounders that have $d > 0$ and $s > 0$, for the positive baseline estimates.

It is important to note that d cannot be interpreted as the measure of the effect of U on the untreated outcome and that s cannot be interpreted as the measure of the effect of U on selection because, to be so, these effects would have to be measured after conditioning for X. In fact, although the definition of the p_{ij} shows that the distribution of U given D and Y does not vary with X, there is, in the data, an empirical association between the simulated U and X, coming from the indirect association of X with D and Y. For this reason, Ichino *et al.* (2008) estimate a Logit model of

[15] We have also performed the estimations considering the median as the threshold and there are no significant differences.

[16] Given the size of our database, performing more than 99 replications would be overly time-consuming.

[17] $p_{i.} = P(U = 1|D = i) = \sum_{j=0}^{1} p_{ij}.P(Y = j|D = i)$

$P(Y = 1|D = 0, U, X)$ in every replication and compute the effect of U on the relative probability of having a positive outcome in case of no-treatment (the observed "outcome effect" of the simulated U) as the average estimated odds ratio of U:

$$\Gamma \equiv \frac{\frac{P(Y=1|D=0,U=1,X)}{P(Y=0|D=0,U=1,X)}}{\frac{P(Y=1|D=0,U=0,X)}{P(Y=0|D=0,U=0,X)}} \tag{6}$$

Similarly, by estimating a Logit model of $P(D = 1|U, X)$, the average odds ratio of U is the measure of the effect of U on the relative probability of being assigned to treatment D (the observed "selection effect" of U):

$$\Lambda \equiv \frac{\frac{P(D=1|U=1,X)}{P(D=0|U=1,X)}}{\frac{P(D=1|U=0,X)}{P(D=0|U=0,X)}} \tag{7}$$

Note that if $d > 0$ and $s > 0$, both the outcome and the selection effect presented above are greater than one. In order to test the robustness of the results, Ichino et al. (2008) propose two exercises, which we implement below.

5.3.1 Robustness checks using "calibrated"' confounders

The first exercise consists in setting the probabilities p_{ij} such as to mimic different observable variables present in the data. As mentioned previously, we should be worried about variables that have $d > 0$ and $s > 0$ but we will also present estimations for variables with different combinations of values of d and s, and hence, of Γ and Λ. We will call these the "calibrated" confounders and we will present them for our three outcome variables, monthly wages, monthly income and the probability of employment in, respectively, Tables 7, 8 and 9.

Table 7 about here

Table 8 about here

Table 9 about here

To make the tables easier to read, we present, for each outcome variable, the estimate with no confounder (which corresponds to the baseline estimate presented in Section 5.2) and the estimate with a neutral confounder. Afterwards, we present the estimates for confounders that replicate observable variables. We first see that the estimate obtained with the neutral confounder is almost identical to the baseline estimate. This is a sign of robustness of the results, which is confirmed by looking at the estimates for the other confounders. Introducing any other confounder only causes very small variations of the ATT estimate when compared to the baseline estimate. This allows to conclude that our results are robust to the possible omission of unobservable variables. As we can see in the tables, we do not present the standard errors for the estimations with confounders. In fact, given the number of individuals in our sample and the number of replications, estimating the standard errors would be extremely time-consuming. Furthermore, Nannicini (2007) comments that the most important in this methodology is not whether the new estimates are significant but rather the comparison between the initial ATT estimates and the estimates obtained with confounders.

5.3.2 Robustness checks using a "killer"' confounder

The second exercise is to set the parameters p_{ij} in order to look for a "killer" confounder, that is, a confounder that, when introduced, would drive the ATT to zero. To do this, we use:

$$
\begin{aligned}
P(U=1) = {} & p_{11}.P(Y=1|D=1).P(D=1) + p_{10}.P(Y=0|D=1).P(D=1) \\
& + p_{01}.P(Y=1|D=0).P(D=0) + p_{00}.P(Y=0|D=0).P(D=0)
\end{aligned}
$$

The parameters $P(U=1)$ and $p_{11} - p_{10}$ do not represent a threat for the ATT estimate and can therefore be fixed. Following Ichino *et al.* (2008), we fix them at $P(U=1) = 0.4$ and $p_{11} - p_{10} = 0$. Using the sample analogues to substitute for the probabilities $P(Y=i|D=j)$ and $P(D=j)$, with $i, j \in \{0, 1\}$, the parameters p_{ij} are uniquely defined once we set the values we want for d and s. We will let both d and s vary from 0.1 to 0.3, hence increasing the outcome and selection effect. We chose this variation interval because it is in agreement with the results we obtained for the "calibrated" confounders. In fact, looking at Tables 7, 8 and 9, we never get outcome effects (Γ) or selection effects (Λ) bigger than three. When both d and s are equal to 0.3, we already obtain outcome and selection effects close or higher than four, so there is no need to increase the variation interval of d and s. The results for the analysis of the "killer" confounder are presented in Tables 10, 11 and 12, for our three outcome variables.

Table 10 about here

Table 11 about here

Table 12 about here

Looking at Table 10, we can see that, as d and s tend to 0.3, the estimated ATT decreases but never gets close to zero. Indeed, even with an outcome and a selection effect close to four, the ATT estimate is still positive and non negligible. The same can be said when the outcome variable is the monthly income, although the estimated ATT gets closer to zero for this variable. Looking now at Table 12, we see that for $d = s = 0.3$, the estimated ATT becomes negative. Nevertheless, we have to keep in mind that, to get to this scenario, we would have to have both a selection effect and an outcome effect greater than four. This means that, in order to have a negative estimate of the probability of employment after controlling for selection on observables, we would have to omit a variable that would have a positive and very high effect on both the relative probability of being selected into treatment ($\Lambda = 4.09$) and the relative probability to have a job ($\Gamma = 5.23$). It is highly unlikely to omit a variable with such important effects. We can therefore be confident in the robustness of our baseline results.

6 Conclusions

This paper evaluated the labour market outcomes of participation in a French training programme for youth, known as “Qualifying Contract” (CQ). An interesting feature of this programme is that it sanctions the acquisition of human capital by a formal diploma. Our evaluation relied on a non-experimental sample of individuals whose initial education ended in 1998, as they either completed their studies or dropped out. We estimated the impact of subsequent participation in a CQ programme on three outcome variables observed in 2003: the net monthly wage, the monthly income and the probability of employment. Our results were obtained using propensity score matching with four different matching estimators: three variants of the nearest neighbour estimator, and a kernel estimator. Although all these estimators yield similar qualitative results, we retained the latter as our preferred estimator, since it has been shown to have better large-sample properties.

With the kernel estimator, we find that participation in a CQ programme has a significantly positive impact on all three outcomes. Participation leads to an increase of 84 euros in the net monthly wage, an increase of 51 euros in the net monthly income, and an increase of 4.5% in the probability of being employed at the end of the period. Since we were able to evaluate the effect of participation, but not of participation duration, we interpret these values as the lower bound of the “true”' ATT. We have every reason to believe that the analysis is reliable and that the performance of the matches is good. We nevertheless examined the sensitivity of our results to possible failures of the CIA, using a recently-developed nonparametric testing procedure. This procedure suggests that our results are quite robust.

Put in a broader perspective, our results stand out as rather optimistic in contrast to previous French (and even some European) studies dedicated to the evaluation of training policies for youth. One plausible explanation is that previous French studies aggregated all training programmes in a single category, which led to the conclusion that training programmes as a whole were at best temporary refuges against unemployment. The authors were aware of this shortcoming, and suggested that programmes with strong training contents may differ from the other in terms of labour market outcomes. Our data, contrary to the Labour Force Survey-type data used in these previous studies, allowed us to focus precisely on one such programme. Our results corroborate the assumption that a programme with a large training content may actually improve the labour market perspectives of participants.

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Table 1: financial compensation for CQ participants based on age and tenure in CQ

	Participant's age		
Tenure in CQ	**Below 18**	**18-20**	**21-25**
1^{st} year of programme	30% of minimum wage	50% of minimum wage	65% of minimum wage
2^{nd} year of programme	45% of minimum wage	60% of minimum wage	75% of minimum wage

Source: DARES

Table 2: comparison of databases Generation 1998 wave1, wave2 and selected sample

Variables	**Wave1 (2001) (55345 obs.)**	**Wave2 (2003) (22021 obs.)**	**Selected sample (17873 obs.)**	**Differences in means (Test statistic)**	
	$\mu_1(\sigma_1)$	$\mu_2(\sigma_2)$	$\mu_3(\sigma_3)$	$\mu_1 = \mu_2$	$\mu_2 = \mu_3$
Outcome variables					
Earnings in euros	–	1122 (4304.4)	1097 (4274.7)	–	0.582
Income in euros	–	1238 (3763.0)	1220 (3677.2)	–	0.470
Employment	–	0.84 (2.15)	0.82 (2.24)	–	0.589
Explanatory variables					
Gender=male (yes/no)	0.51 (1.83)	0.51 (2.90)	0.52 (2.93)	0.003	-0.160
Age in 1998	21.30 (10.73)	21.30 (16.85)	21.21 (16.67)	0.018	0.539
Nationality=non-French (yes/no)	0.04 (0.74)	0.04 (1.15)	0.04 (1.19)	0.246	-0.136
Diploma:					
Unskilled (primary or secondary education only)	0.08 (0.98)	0.08 (1.56)	0.08 (1.62)	0.005	-0.318
CAP/BEP drop out	0.08 (1.01)	0.08 (1.60)	0.09 (1.67)	0.000	-0.351
CAP/BEP in services	0.08 (1.01)	0.08 (1.59)	0.08 (1.61)	-0.005	-0.008
CAP/BEP in manufacturing	0.09 (1.03)	0.09 (1.63)	0.09 (1.65)	-0.006	-0.007
High school drop out	0.04 (0.70)	0.04 (1.12)	0.04 (1.13)	-0.005	0.016
Vocational *baccalauréat* in services	0.08 (0.98)	0.08 (1.55)	0.07 (1.53)	0.000	0.221
Vocational *baccalauréat* in manufacturing	0.05 (0.81)	0.05 (1.28)	0.05 (1.27)	0.004	0.160
Baccalauréat	0.13 (1.23)	0.13 (1.96)	0.13 (1.99)	0.002	-0.071
Vocational diploma in Health/Social sectors	0.03 (0.65)	0.03 (1.03)	0.03 (0.92)	-0.003	-0.749
University (first 2 years completed)	0.03 (0.58)	0.03 (0.92)	0.03 (0.94)	0.002	-0.020
BTS/DUT in services	0.08 (0.98)	0.08 (1.56)	0.08 (1.59)	0.007	-0.118
BTS/DUT in manufacturing	0.05 (0.79)	0.05 (1.26)	0.05 (1.28)	0.003	-0.061
University degree (3 or 4 years) in Social Sciences or Humanities	0.09 (1.04)	0.09 (1.64)	0.09 (1.65)	-0.004	0.062
University degree (3 or 4 years) in Maths or Sciences	0.01 (0.43)	0.01 (0.68)	0.01 (0.71)	0.000	-0.104
Higher education (Master/5 years completed)	0.08 (1.02)	0.08 (1.62)	0.08 (1.61)	0.001	0.133
Father's labour market situation in 1998:					
Employed	0.79 (1.50)	0.79 (2.35)	0.79 (2.37)	-0.524	0.027
Unemployed	0.03 (0.66)	0.03 (1.00)	0.03 (1.01)	0.413	-0.033
Inactive	0.01 (0.33)	0.01 (0.48)	0.01 (0.48)	0.342	0.028
Retired	0.08 (1.01)	0.08 (1.61)	0.08 (1.63)	-0.100	0.003
Deceased	0.04 (0.74)	0.04 (1.17)	0.04 (1.17)	0.072	0.062
Other situation	0.05 (0.77)	0.04 (1.16)	0.04 (1.19)	0.623	-0.104
Mother's labour market situation in 1998:					
Employed	0.59 (1.80)	0.61 (2.84)	0.60 (2.87)	-0.686	0.209
Unemployed	0.03 (0.60)	0.03 (0.93)	0.03 (0.95)	0.240	-0.086
Inactive	0.31 (1.69)	0.30 (2.66)	0.31 (2.71)	0.325	-0.244
Retired	0.03 (0.63)	0.03 (0.97)	0.03 (0.96)	0.159	0.143
Deceased	0.01 (0.43)	0.01 (0.67)	0.01 (0.66)	0.143	0.126
Other situation	0.03 (0.61)	0.02 (0.89)	0.02 (0.92)	0.669	-0.090
Father's nationality=non-French (yes/no)	0.17 (1.39)	0.17 (2.17)	0.17 (2.22)	0.423	-0.224
Has children in 1998 (yes/no)	0.02 (0.45)	0.02 (0.83)	0.02 (0.82)	-0.903	0.141
Family situation in 1998:					
Single (in her own home)	0.15 (1.30)	0.14 (2.04)	0.15 (2.07)	0.356	-0.112
Couple	0.15 (1.29)	0.14 (2.03)	0.14 (2.02)	0.163	0.290
Lived at her parents'	0.68 (1.71)	0.69 (2.69)	0.69 (2.70)	-0.438	-0.274
Unknown	0.03 (0.60)	0.03 (0.93)	0.02 (0.87)	0.130	0.424
Did not go on with studies because:					
was tired of studying	0.41 (1.80)	0.41 (2.85)	0.41 (2.89)	0.292	-0.238
financial difficulties	0.21 (1.50)	0.21 (2.36)	0.21 (2.37)	0.248	0.071
found a job	0.26 (1.61)	0.26 (2.56)	0.24 (2.52)	-0.017	0.839
reached the desired education level	0.45 (1.82)	0.45 (2.89)	0.45 (2.92)	-0.180	0.273
failed to enrol in higher education level	0.10 (1.08)	0.10 (1.75)	0.10 (1.79)	-0.467	-0.165
Did at least one internship during her studies	0.64 (1.75)	0.64 (2.79)	0.64 (2.82)	0.093	0.110
Held a regular job (as source of income) during her studies	0.09 (1.07)	0.09 (1.68)	0.09 (1.65)	0.147	0.285
Had occasional jobs during the holidays	0.56 (1.82)	0.57 (2.88)	0.56 (2.91)	-0.163	0.296
Had occasional jobs while studying	0.22 (1.50)	0.21 (2.38)	0.21 (2.39)	0.116	0.081

Table 3: summary statistics

Variables	**Treated (732 obs.)** μ_1	(σ_1)	**Control (17141 obs.)** μ_2	(σ_2)	**Differences in means (Test statistic)** $\mu_1 = \mu_2$
Outcome variables					
Earnings in euros	1056	(625.8)	1133	(716.1)	-2.851
Income in euros	1157	(525.7)	1241	(623.4)	-3.485
Employment	0.84	(0.37)	0.84	(0.36)	-0.277
Explanatory variables					
Gender=male (yes/no)	0.53	(0.50)	0.51	(0.50)	1.184
Age in 1998	19.95	(2.13)	21.40	(2.87)	-13.492
Nationality=non-French (yes/no)	0.04	(0.20)	0.04	(0.19)	0.672
Diploma:					
Unskilled (primary or secondary education only)	0.13	(0.33)	0.08	(0.27)	4.807
CAP/BEP drop out	0.09	(0.28)	0.08	(0.27)	0.782
CAP/BEP in services	0.11	(0.31)	0.07	(0.26)	3.378
CAP/BEP in manufacturing	0.12	(0.32)	0.09	(0.28)	2.825
High school drop out	0.03	(0.18)	0.04	(0.18)	-0.353
Vocational *baccalauréat* in services	0.11	(0.32)	0.07	(0.25)	4.362
Vocational *baccalauréat* in manufacturing	0.05	(0.21)	0.05	(0.23)	-1.039
Baccalauréat	0.21	(0.41)	0.12	(0.32)	7.133
Vocational diploma in Health/Social sectors	0.001	(0.04)	0.06	(0.23)	-6.418
University (first 2 years completed)	0.03	(0.16)	0.02	(0.15)	0.524
BTS/DUT in services	0.06	(0.24)	0.08	(0.27)	-1.691
BTS/DUT in manufacturing	0.05	(0.21)	0.05	(0.22)	-0.555
University degree (3 or 4 years) in Social Sciences or Humanities	0.02	(0.12)	0.09	(0.29)	-7.175
University degree (3 or 4 years) in Maths or Sciences	0.005	(0.07)	0.02	(0.13)	-2.244
Higher education (Master/5 years completed)	0.004	(0.06)	0.08	(0.28)	-7.768
Father's labour market situation in 1998:					
Employed	0.83	(0.38)	0.80	(0.40)	-1.782
Unemployed	0.03	(0.17)	0.03	(0.16)	0.196
Inactive	0.003	(0.05)	0.007	(0.09)	-1.477
Retired	0.08	(0.27)	0.09	(0.28)	-0.851
Deceased	0.02	(0.15)	0.04	(0.20)	-2.765
Other situation	0.05	(0.21)	0.04	(0.19)	0.886
Mother's labour market situation in 1998:					
Employed	0.65	(0.48)	0.60	(0.49)	-2.467
Unemployed	0.03	(0.17)	0.03	(0.16)	0.353
Inactive	0.27	(0.44)	0.30	(0.46)	-2.022
Retired	0.02	(0.14)	0.03	(0.17)	-1.515
Deceased	0.003	(0.05)	0.01	(0.12)	-2.543
Other situation	0.03	(0.17)	0.02	(0.15)	1.458
Father's nationality=non-French (yes/no)	0.17	(0.38)	0.15	(0.36)	1.708
Has children in 1998 (yes/no)	0.007	(0.08)	0.02	(0.15)	-2.728
Family situation in 1998:					
Single (in her own home)	0.14	(0.34)	0.14	(0.35)	-0.276
Couple	0.08	(0.27)	0.15	(0.35)	-5.18
Lived at her parents'	0.78	(0.42)	0.68	(0.47)	5.635
Unknown	0.01	(0.10)	0.04	(0.19)	-3.794
Did not go on with studies because:					
was tired of studying	0.36	(0.48)	0.41	(0.49)	-2.994
financial difficulties	0.18	(0.38)	0.21	(0.41)	-1.946
found a job	0.21	(0.41)	0.26	(0.44)	-2.673
reached the desired education level	0.27	(0.44)	0.49	(0.50)	-11.53
failed to enrol in higher education level	0.13	(0.33)	0.09	(0.29)	3.21
Did at least one internship during her studies	0.61	(0.49)	0.62	(0.48)	-0.432
Held a regular job (as source of income) during her studies	0.08	(0.27)	0.08	(0.27)	0.091
Had occasional jobs during the holidays	0.55	(0.50)	0.56	(0.50)	-0.153
Had occasional jobs while studying	0.20	(0.40)	0.21	(0.41)	-0.278

Figure 1: Distribution of the “training specialization” variable in the treated and control groups

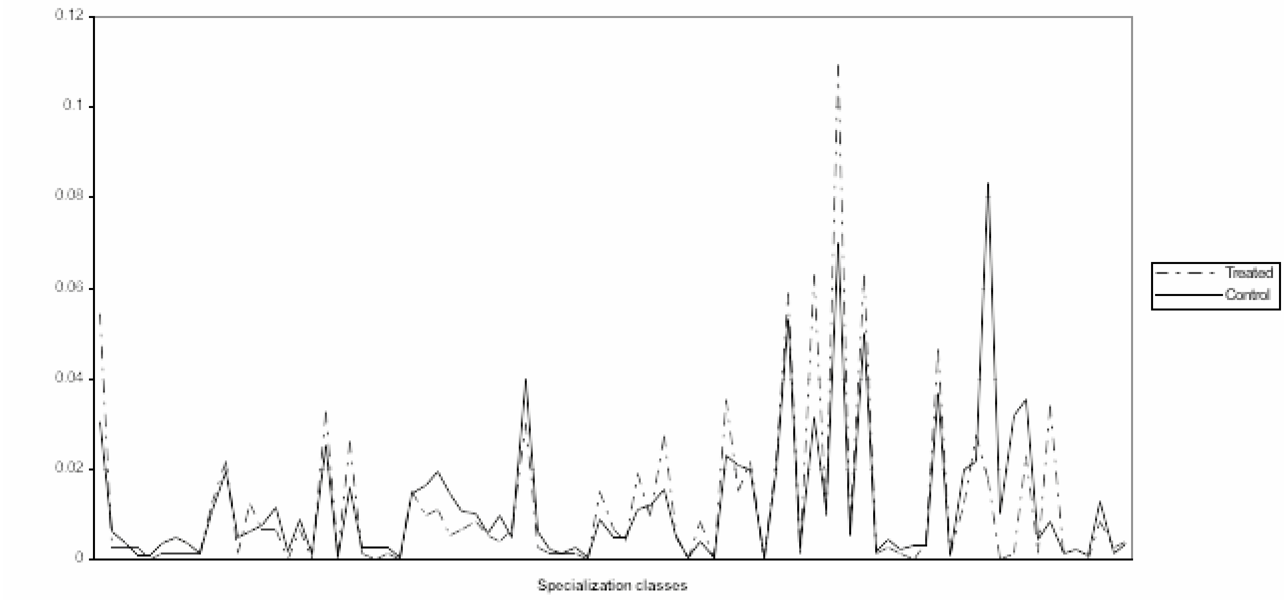

Figure 2: Distribution of the “employment area” variable in the treated and control groups

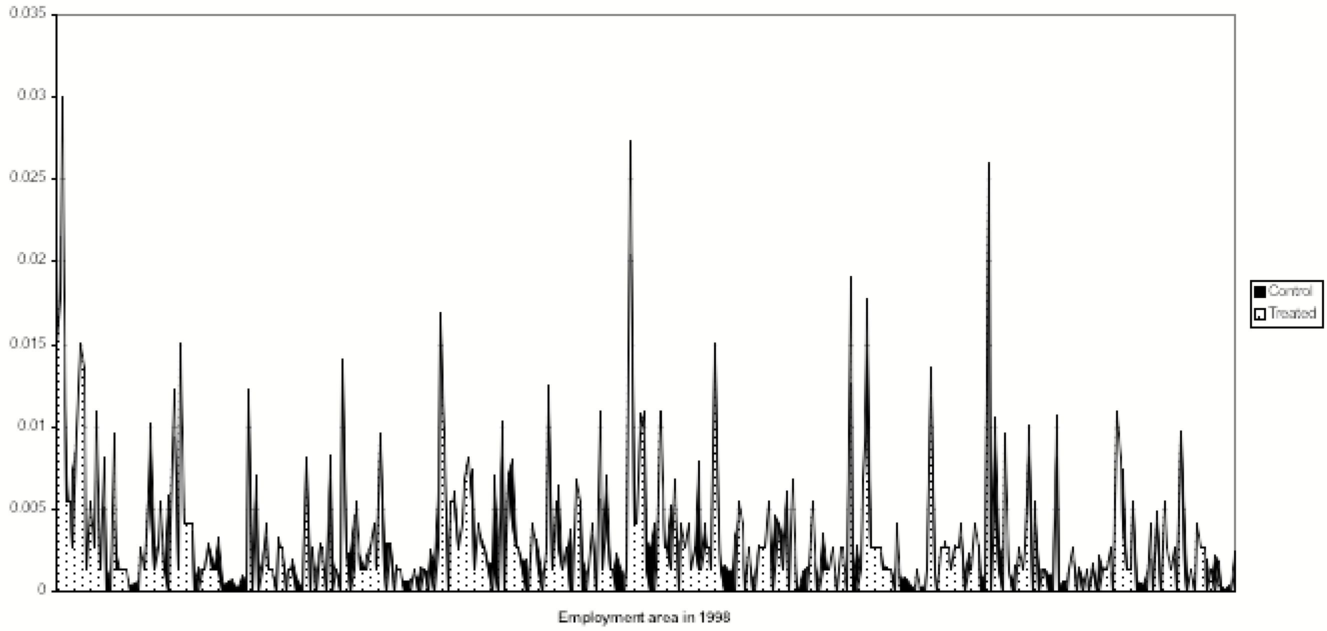

Table 4: Probit model estimates

	Variables	Marg. Eff.	S.E.
Gender	*Male* (ref. female)	0.004	(0.002)*
Age in 1998		-0.005	(0.001)***
Nationality	*Non-French* (ref. French)	0.005	(0.007)
Diploma	*Unskilled*	-0.011	(0.004)**
(ref. *Baccalauréat*)	*CAP/BEP drop out*	-0.014	(0.003)***
	CAP/BEP - services	-0.012	(0.003)***
	CAP/BEP - manufacturing	-0.008	(0.004)*
	High school drop out	-0.013	(0.003)***
	Voc. Bac - services	-0.005	(0.004)
	Voc. Bac - manufacturing	-0.013	(0.003)***
	Voc Health&Social	-0.023	(0.003)***
	University (2 years)	-0.005	(0.005)
	BTS/DUT - services	-0.015	(0.003)***
	BTS/DUT - manufacturing	-0.008	(0.004)
	University (3 or 4 years) Social Sc. & Humanities	-0.023	(0.003)***
	University (3 or 4 years) Maths & Sciences	-0.020	(0.003)***
	Higher educ. (5 years)	-0.029	(0.002)***
Father's labour market situation	*Unemployed*	-0.004	(0.005)
in 1998	*Inactive*	-0.013	(0.007)
(ref. employed)	*Retired*	0.009	(0.005)**
	Deceased	-0.011	(0.004)**
	Other situation	-0.002	(0.005)
Mother's labour market situation	*Unemployed*	-0.001	(0.006)
in 1998	*Inactive*	-0.007	(0.002)***
(ref. employed)	*Retired*	0.005	(0.008)
	Deceased	-0.019	(0.003)**
	Other situation	-0.001	(0.006)
Father's nationality	*Non-French* (ref. French)	-0.004	(0.003)
Children in 1998	*Yes* (ref. no)	-0.011	(0.006)
Family situation in 1998	*Single*	0.009	(0.004)***
(ref. w/ parents)	*Couple*	-0.004	(0.003)
	No answer	-0.008	(0.006)
Ended studies because	*Tired of studying* (ref. no)	-0.012	(0.002)***
	Financial difficulties (ref. no)	-0.005	(0.002)**
	Found a job (ref. no)	-0.005	(0.002)**
	Reached desired education level (ref. no)	-0.009	(0.002)***
	Failed to enrol in higher level (ref. no)	0.001	(0.003)
Internships during studies	*Yes* (ref. no)	0.002	(0.002)
Regular job during studies	*Yes* (ref. no)	0.003	(0.004)
Occasional jobs during holidays	*Yes* (ref. no)	0.007	(0.002)***
Occasional jobs during studies	*Yes* (ref. no)	0.000	(0.002)
Employment area fixed effect	LR test of "no effect"	LR Chi-2 statistic: 201.50	
Specialization fixed effect	LR test of "no effect"	LR Chi-2 statistic: 193.80***	
Wald test of null hypothesis	test statistic: 870.39***		
Goodness-of-fit statistics	log L: -2491.56	Pseudo R^2: 0.15	

* significant at 10%, ** significant at 5%, *** significant at 1%

Table 5: Estimates of the ATT with propensity score matching

Matching algorithm	**(I) outcome=monthly wage**			**(II) outcome=monthly income**			**(III) outcome=employment**		
	ATT	S.E.	$\|t\|$	ATT	S.E.	$\|t\|$	ATT	S.E.	$\|t\|$
Nearest neighbour	82.74	31.72	2.61	19.17	26.79	0.72	0.018	0.020	0.90
5 nearest neighbour	120.34	26.70	4.51	77.24	23.12	3.34	0.048	0.015	3.20
10 nearest neighbour	126.25	27.01	4.67	83.30	23.59	3.53	0.051	0.014	3.64
Kernel	84.21	25.01	3.37	51.39	21.71	2.37	0.045	0.016	2.81

Table 6: performance of match (reduction of average absolute standardised bias)

Matching algorithm	**average absolute standardised bias** *before matching*	*after matching*	**% reduced bias**
Nearest neighbour	1.50	0.77	48.7%
5 nearest neighbour	1.50	1.01	32.7%
10 nearest neighbour	1.50	1.07	28.5%
Kernel	1.50	1.14	23.8%

Table 7: "Calibrated" confounders for monthly wages

	p_{11}	p_{10}	p_{01}	p_{00}	**ATT**	Γ	Λ
No confounder	0.00	0.00	0.00	0.00	84.21	-	-
Neutral confounder	0.50	0.50	0.50	0.50	84.18	1.00	1.00
Confounder like:							
Male	0.66	0.41	0.59	0.43	79.91	2.00	1.19
Foreign father	0.16	0.18	0.13	0.17	84.62	0.68	1.17
Children	0.00	0.01	0.02	0.02	85.01	1.18	0.33
Single	0.16	0.11	0.17	0.10	83.29	1.94	1.06
Couple	0.07	0.09	0.17	0.12	87.45	1.49	0.52
Ended sudies because reached desired level	0.31	0.24	0.60	0.37	109.76	2.61	0.42
Ended studies because tired of studying	0.32	0.39	0.36	0.47	78.89	0.63	0.75
Internship during studies	0.63	0.60	0.65	0.59	84.19	1.28	1.00

99 replications. The matching algorithm is an Epanechnikov kernel. The estimate with no confounder corresponds to the baseline estimate presented in Section 5.2.

Table 8: "Calibrated" confounders for monthly income

	p_{11}	p_{10}	p_{01}	p_{00}	**ATT**	Γ	Λ
No confounder	0.00	0.00	0.00	0.00	51.39	-	-
Neutral confounder	0.50	0.50	0.50	0.50	51.00	1.00	1.00
Confounder like:							
Male	0.65	0.46	0.58	0.46	49.19	1.66	1.15
Foreign father	0.16	0.18	0.14	0.16	50.69	0.84	1.20
Children	0.00	0.01	0.03	0.02	52.23	1.39	0.32
Single	0.16	0.12	0.18	0.10	50.51	1.99	1.06
Couple	0.06	0.09	0.17	0.12	53.72	1.52	0.51
Ended sudies because reached desired level	0.30	0.25	0.59	0.40	67.85	2.23	0.40
Ended studies because tired of studying	0.34	0.37	0.34	0.48	45.32	0.54	0.74
Internship during studies	0.64	0.60	0.65	0.60	51.25	1.26	0.99

99 replications. The matching algorithm is an Epanechnikov kernel. The estimate with no confounder corresponds to the baseline estimate presented in Section 5.2.

Table 9: "Calibrated" confounders for the probability of employment

	p_{11}	p_{10}	p_{01}	p_{00}	**ATT**	Γ	Λ
No confounder	0.00	0.00	0.00	0.00	0.045	-	-
Neutral confounder	0.50	0.50	0.50	0.50	0.045	1.00	1.02
Confounder like:							
Male	0.54	0.49	0.53	0.41	0.043	1.64	1.12
Foreign father	0.16	0.24	0.13	0.24	0.047	0.48	1.17
Children	0.00	0.03	0.02	0.04	0.043	0.45	0.29
Single	0.14	0.11	0.14	0.11	0.045	1.32	1.00
Couple	0.08	0.08	0.15	0.14	0.045	1.04	0.50
Ended sudies because reached desired level	0.28	0.20	0.52	0.29	0.068	2.76	0.40
Ended studies because tired of studying	0.35	0.40	0.41	0.45	0.043	0.82	0.78
Internship during studies	0.62	0.60	0.63	0.60	0.045	1.14	0.98

99 replications. The matching algorithm is an Epanechnikov kernel. The estimate with no confounder corresponds to the baseline estimate presented in Section 5.2.

Table 10: "Killer" confounder for monthly wages

	s=0.1		**s=0.2**		**s=0.3**	
	ATT		**ATT**		**ATT**	
	Γ	Λ	Γ	Λ	Γ	Λ
d=0.1	**76.36**		**73.45**		**72.89**	
	1.53	1.60	1.55	2.47	1.55	3.91
d=0.2	**69.08**		**60.85**		**51.09**	
	2.41	1.69	2.43	2.62	2.43	4.25
d=0.3	**59.08**		**43.73**		**29.76**	
	3.89	1.81	3.89	2.75	3.95	4.45

99 replications. The matching algorithm is an Epanechnikov kernel.

Table 11: "Killer" confounder for monthly income

	s=0.1		**s=0.2**		**s=0.3**	
	ATT		**ATT**		**ATT**	
	Γ	Λ	Γ	Λ	Γ	Λ
d=0.1	**43.70**		**41.86**		**41.70**	
	1.54	1.63	1.55	2.47	1.54	3.87
d=0.2	**38.72**		**32.08**		**25.97**	
	2.39	1.68	2.39	2.56	2.41	4.04
d=0.3	**31.19**		**19.19**		**7.99**	
	3.80	1.77	3.80	2.73	3.82	4.30

99 replications. The matching algorithm is an Epanechnikov kernel.

Table 12: "Killer" confounder for the probability of employment

	s=0.1		**s=0.2**		**s=0.3**	
	ATT		**ATT**		**ATT**	
	Γ	Λ	Γ	Λ	Γ	Λ
d=0.1	**0.037**		**0.032**		**0.028**	
	1.57	1.57	1.58	2.42	1.58	3.82
d=0.2	**0.031**		**0.020**		**0.007**	
	2.62	1.61	2.64	2.51	2.65	4.04
d=0.3	**0.024**		**0.006**		**-0.012**	
	5.05	1.68	5.16	2.55	5.23	4.09

99 replications. The matching algorithm is an Epanechnikov kernel.

Appendix 1: description of French diplomas

This Appendix is dedicated to a further description of the French diplomas presented in Tables 3 and 4. We hope to give the international reader a clearer view of the diplomas existing in 1998, and give some details about their recent evolution whenever necessary. Diplomas are presented according to the order in which they appear in Table 3 (which is based on increasing education levels).

The **Unskilled** category of Table 3 corresponds to individuals who completed only mandatory education. In France, mandatory education ends at 16 years of age, which generally corresponds to the last year of Junior High School (the *Collège* in French), or to the first year of High School (the *Lycée* in French). Pupils facing severe difficulties may however stay down several years in Elementary School, and therefore drop out before the last year of Junior High School.

The **CAP** (*Certificat d'Aptitude Professionnelle*) and **BEP** (*Brevet d'Etudes Professionnelles*) are both "vocational diplomas", in the sense that they give a training oriented towards the acquisition of vocational skills. The CAP is a two-year diploma that can be prepared after the last year of Junior High School, and which targets the acquisition of vocational skills relevant for a specific job (such as baker or mason). Similarly, the BEP is two-year diploma prepared after graduating from Junior High School and insisting on the acquisition of vocational skills. The main difference with the CAP is that the BEP dedicates more time to general teaching (e.g. literature, mathematics), which allows some graduates to enrol in a higher-level training track afterwards (if they wish to do so). In the Generation 1998 database, both diplomas are aggregated in a single category because of their similarities. However, the data distinguishes between pupils who fail to complete a CAP or BEP, pupils who completed a CAP or BEP oriented towards a services job (such as clerk or secretary), and pupils who completed a CAP or BEP oriented towards a job in the manufacturing industry (such as skilled worker in the automobile or electronic industry).

In France, just like in the U.S., high schools train students who are generally between 15-16 and 17-18 years old. The high school training track lasts three years, and is concluded by a national exam, sanctioned by a diploma known as ***baccalauréat***, which is similar to diplomas existing in other countries, such as the German *Abitur*. The *baccalauréat* (affectionately shortened as '*bac*' by the French) is perhaps the most important diploma in France, if only because of its symbolic nature: not only does it conclude High School training, but it also opens the doors to Higher Education. Indeed, the *baccalauréat* is required to study in a University or in a more selective institution (such as a Business School or Engineering School). It sanctions only general training (in literature, history, geography, sciences, mathematics). The Generation 1998 database identifies high school drop-outs and recipients of the *baccalauréat*. Over the years, as the demand for education increased, **vocational *baccalauréats*** have been created. These diplomas are high school level diplomas, but are more oriented towards the acquisition of vocational skills than the usual *baccalauréat*. Training takes place in specific high schools, known as vocational high schools, and maybe involve some internship or apprenticeship in a firm. The Generation 1998 database allows us to distinguish between vocational *baccalauréats* oriented towards services jobs, and those oriented towards jobs in the manufacturing industry.

After passing the *baccalauréat*, pupils may try to enrol in a higher education training track and become students. Our database identifies short vocational training tracks, university training, and business or engineering school training. The most frequent vocational training tracks encountered in higher education are **BTS** (*Brevets de Technicien Supérieur*) and **DUT** (*Diplômes Universitaires de Technologie*). Both are two-year diplomas that can be prepared immediately after the *baccalauréat*. Students are selected on the basis of their previous performance in high school and of the score they obtained when they passed the *baccalauréat*. Although BTS are higher education diplomas, training for a BTS always takes place in a high school (but not all high schools offer BTS training). BTS training is oriented towards a specific industry, and relies on a mix of 2/3 vocational training (including an internship during the second year) and 1/3 of general training. Training for a DUT takes place within a university, and also relies on a mix of general and vocational training (including an internship). BTS training constitutes a logical continuation for a vocational *baccalauréat*, whereas DUT training is more likely to select students with a standard *baccalauréat*. In the Generation 1998 database, BTS and DUT are regrouped because of their similarities, but a distinction is made between BTS or DUT targeting the services industry, and those targeting the manufacturing industry. Vocational diplomas targeting jobs in the **health or social sectors** (such as medical assistant or nurse) are in a category of their own. They are specific diplomas that may require more than 2 years of training. Training generally takes place in specific schools (that is the case for nurses in particular) that often select their students on the basis of a specific exam (in addition to the *baccalauréat*).

French universities offer a large variety of training in different disciplines, and attract the largest number of *baccalauréat* recipients, because of the absence of selection at the entrance. In 1998, there existed five levels of university education: the first **two years** granted a diploma known as DEUG (*Diplôme d'Etudes Universitaires Générales*). Many students drop out during these first two years. **One more year** of successful university studies would grant a *Licence*, while a **fourth year** would grant a *Maîtrise*. After the *Maîtrise*, it was possible to enrol in a one-year Master program. Students had to go through a strong selection process (based on their prior performance and achievements) to enter Master training. Since the Bologna Declaration of June 1999[18], however, university education in France has been gradually reformed to conform to the international standards of a Bachelor after three years of university training and a Master after two additional years. In the Generation 1998 database, however, university diplomas are still those existing in the pre-Bologna era. The first category of university diplomas gathers DEUG of all disciplines. A second category gathers *Licences* and *Maîtrises* obtained in the Social Sciences or Humanities, while the third category gathers *Licences* and *Maîtrises* in Mathematics and Sciences. Similarly, Masters in Management, Social Sciences, or Humanities are regrouped within a single category, whereas Masters in Sciences or Mathematics are regrouped in another category.

Finally, in the French higher education system, there exist, alongside universities, prestigious and highly selective teaching institutions known as *Grandes Ecoles* (similar, in a way, to Ivy League institutions in the USA). These institutions select students either after the *baccalauréat* (on the basis

[18]For more information on the Bologna Declaration and the Bologna Process, see: http://ec.europa.eu/education/policies/educ/bologna/bologna_en.html

of a specific exam), or after two years spent in a specific training track (*classes préparatoires*) that prepares students for *Grandes Ecoles* education. *Grandes Ecoles* deliver a training that is oriented either towards engineering, or towards business and management (in which case they are very similar to Business Schools). After five years of *Grandes Ecoles* training, students are granted a diploma which is equivalent to a Master. The Generation 1998 data puts graduates from engineering schools and graduates from business schools in two distinct categories. However, as explained in Section 4.2, we had to merge all university Masters and *Grandes Ecoles* Masters in a single category, in order to be able to conduct our analysis.