formula for finite populations[7] (Cochran, 1977). The sample represents the 16.0% population and shows a satisfactory level of precision, particularly with respect to similar evaluation exercises (Lalla *et al.*, 2004; Centra *et al.*, 2007; IRPET, 2011).

Secondly, individuals are split by through the Cochran formula across the 6 strata obtained. Such design allows the researcher to focalize on the peculiarities of the training actions, accounting for other labour market policies. In order to obtain representative subsamples, the smaller subpopulations are oversized, thus reducing the sampling error associated to the critical strata[8]. Once the subsample size is defined, individuals are randomly extracted, following the proportional allocation design.

The overall response rate is 52,4%, showing a consistent *"hard-core"* (Cochran, 1977) of individuals systematically refusing to be interviewed, which could affect the estimates. Moreover, the 9,0% non-respondents are displaced by other individuals from the same stratum (Levy *et al.*, 2008), maintaining the designed representativeness, but possibly enhancing the non-sampling error.

3.3 The counterfactual sample

In the present work, the net impact evaluation is realized by properly identifying a comparison sample as much homogeneous to the main sample as possible. In fact, in the counterfactual analysis the main and comparison groups should theoretically differ solely with respect to the treatment, in this case the attendance to VT courses. Hence, the counterfactual impact evaluation should answer the question "what if the (training) policy would not have been supplied?". But this is far from being a simple task (White, 2010). Mostly, it is an arduous whenever the comparison group has not been designed *ex-ante*, as in experimental design (e.g. randomized control trial experiments), but it has to be identified *ex-post*, as in the present case (Ciravegna *et al.*, 1995). Moreover, in the present case the size of the control group is necessarily limited by other evaluation objectives (see sec. 3.2). A careful analysis of the evaluation contest suggested to extract the control sample from the no-shows (Bell *et al.*, 1995), i.e. from the students who did not complete the course (treatment) and that were not employed at enrolment. Such individuals are highly homogeneous with the main group.

Some alternative strategies were aborted for many unfeasibility constraints. In particular, the pass-list strategy would be quite desirable, since it attenuates the selection bias by comparing the placement outcomes between the last-admitted and first-excluded individuals. However, pass-lists are not available for VT policies. Finally, an approach based on employment agency lists was neglected, since the counterfactual sample would be too heterogeneous with respect to the main group. In fact, these lists collect a particular group of unemployed individuals, who presumably differ from the main group for several unobservable characteristics (e.g. motivation, proactive attitude, individual abilities, background), which substantially influence their placement (selection bias).

[7] The formula is $n_e = \frac{\frac{z_{1-\alpha/2}^2 P(1-P)}{e^2}}{1+\frac{1}{N}\left(\frac{z_{1-\alpha/2}^2 P(1-P)}{e^2}-1\right)}$, where $\boldsymbol{e}$ is the absolute error in estimating the unknown proportion $\boldsymbol{P}$ of the target population $\boldsymbol{N}$; $\boldsymbol{z_{1-\alpha/2}}$ is the abscissa when the normal distribution function equals ***(1-α/2)***; $\boldsymbol{\alpha}$ is the desired significance level. The chosen values are e = 2.31, P = 0.5, α = 0.1.

[8] The absolute error for the whole sample is 2.3%, while each stratum lies underneath 7%.