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Skills training programmes for unemployed workers in mature economies*

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Abstract

This paper studies the effects on long term labour market success of the provision of intensive skills training courses offered in 2013-14 to adult unemployed workers in the north-east of Italy. We find a substantial effect, which persists well into the fourth year after the beginning of the course. From a methodological viewpoint, we argue, as proposed by Angrist and Rokkanen (2015) that the set-up is equivalent to a randomised controlled trial, in view of the fact that the criterion which determines admission to the course is unrelated to the outcomes of interest.

JEL Classification: J24, J68, M53, C21

Keywords: Active labour market policies, Trento, Unemployment training, Randomised control trial, Regression discontinuity design

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1 Introduction

A vibrant economy requires a flexible workforce, capable to adapt knowledge and skills to take advantage of novel opportunities and to address the threat to existing jobs arising from innovations and technological advances and shifts in demand. This need pushes governments across the world to invest large sums in policies that come under the broad label of "active labour market policies".[2] These lubricate the labour market by reducing search frictions, and so helping workers find suitable employers, or by improving some relevant characteristics of the workers, so that, following a match, employment is more likely and subsequent separation less probable. The latter group consists mostly of the provision of job training programmes for unemployed workers. Such programmes are very expensive, and for this reason it is very important to understand precisely how effective they are. To gain this understanding, an experimental methodology, extremely useful in general, is of crucial importance here. This is so because, as extensively discussed in Heckman et al.'s early survey (1999), the outcomes for those who undertook a training programme are not easily comparable with the outcomes for those who did not. And indeed RCT or quasi-experimental methods are commonly used to study these programmes: the meta-analysis by Kluve et al. (2017) considers 107 interventions directed at young people in 31 countries. They find mixed results, especially for developing countries. Recent works confirms these earlier findings (Alfonsi et al., 2020; Bratti et al., 2022).

The intervention studied in these works focus on youth workers, and there are relative few interventions, and consequently few studies, where the age range of those eligible for assistance is extended to older adults. Fewer still are carried out in advanced economies, where such projects as there are consist of small scale interventions which are shown to achieve some success as long as they are targeted to the specific needs of local employers (Martin & Grubb, 2001; Meager, 2009). This paucity makes the present paper, which studies an intervention in a mature economy, aimed at all workers, irrespective of age and experience, useful in filling this crucial gap in the literature: young unemployed, whose

[2] Table 1 in Crépon and Van Den Berg's (2016, p 522) gives an idea of its size; other recent surveys are Card et al. (2018) and Vooren et al. (2019).

work experience is mainly in casual short-term or part-time jobs, may differ in fundamental manners from older workers, who may have transitioned into unemployment following several years of holding a regular job. These differences imply that any lesson learnt by identifying the effects of youth training programmes may teach us little on the effectiveness of active labour market policies intended to assist older workers to acquire any new skills they need to re-enter the labour market.

We find that the training courses for skilled manual and intermediate office and technical jobs run by a public body in the north-east of Italy in 2013-14, which provided over 400 hours of training and work experience and were open to anyone who was unemployed irrespective of their age and experience, had a strong positive causal effect on the participants' chance of being employed in the years following the course. The over five hundred individuals admitted to the courses are on average roughly 6-9% more likely to be in work in the period from 12 to 54 months after they begun the course. In principle, this is a lower bound, both because it is the intention to treat effect, and so its treatment subsample includes those admitted to the course, who, for whatever reason, did not attend, and because it constitute the average of the period beginning at the start of the course, attendance to which made it impossible for those admitted to be in work at the same time. In practice, Table 2 shows that the results obtained are robust to relaxation of both these assumptions. Importantly, and unlike the few existing studies of training programmes targeting adult unemployed workers, we are able to study the medium term effects of attendance to the training course: we find that the effect is persistent as it remains positive well into the fifth year, though as time goes by the effect loses statistical significance, likely in consequence of the accumulation of other random events affecting participants and non-participants alike.

The paper begins with a description of the intervention, in Section 2. After that, Section 3 details our empirical approach. Results are presented and discussed in Section 4.

2 The training programme

Between 2013 and 2014, the Labour Bureau (Agenzia del Lavoro) of Trento Autonomous Province in the north-east of Italy run a programme of occupational training courses aimed at unemployed workers. The programme comprised 18 courses providing training for skilled manual jobs (such as sous-chef and carpenter), seven training in higher-grade non manual occupations (among them accountants and office clerks) and five for intermediate technical jobs (such as web and computer network technicians). Each course admitted a maximum of 15-20 individuals, and provided between 400 and 450 hours of classroom teaching, and a work experience internships.[3] The various courses lasted between 11 and 32 weeks, averaging around 18 weeks. Eligibility was restricted to those registered as officially unemployed, with a basic knowledge of the Italian language, and, depending on the course, possession of a high-school qualification, availability to work weekends and evenings, and ability to work with people. Admission was subject to a standardised written test, typically a mixture of general knowledge and course specific questions, either as multiple choice quizzes or as closely structured questions. The pre-determined capacity was filled for each course by the applicants with the highest scores.[4]

Our analysis uses two data sources. The first is the information gathered as part of the administration of the training course programme. The second is obtained from firms' mandatory communications to the Italian public employment agencies: we utilise information on each individual's employment history in the period from three years before to four and a half years after the beginning of the course they applied to join.

Table 1 collects summary statistics of the variables characterising the individuals in the sample. The dataset contains information on the course applied for, the dates when it started and when it finished: this information is used to determine the "pre-" and "post-" variables included in the table. In addition to those listed, we some have information on the household composition, the number of earners, the main sources of the household's income,

[3] This was either unpaid, or attracted a nominal, low, pay, or the reimbursement of some of the expenses incurred.

[4] The following table breaks down the type of the courses by demand and attendance, and hints at the lack of strong differences in their relative desirability.

	Not adm.	Admit.	Total	% admit.
Courses targeting intermediate technical jobs	54	74	128	57.81
Courses targeting skilled manual occupations	383	309	692	44.65
Courses targeting routine non-manual employees	198	164	362	45.30
Total	635	547	1182	46.28

Table 1: Summary Statistics

Variable	Not admitted mean	sd	Admitted mean	sd	Difference of means
Female	0.37	0.48	0.34	0.47	0.04
Age	32.53	10.95	34.87	10.36	2.34***
Italian citizen	0.6	0.49	0.78	0.42	0.18***
High school diploma	0.48	0.5	0.65	0.48	0.1***
Employment one year before	55.57	90.14	89.97	107.43	34.41***
Employment two years before	96.01	129.54	151.06	148.89	55.06***
Would accept any job	0.425	0.495	0.305	0.461	-0.12***
Admission test score	-12.69	11.346	8.32	6.797	21.01***
Post-course employment, Y_i	0.373	0.311	0.451	0.324	0.08 ***

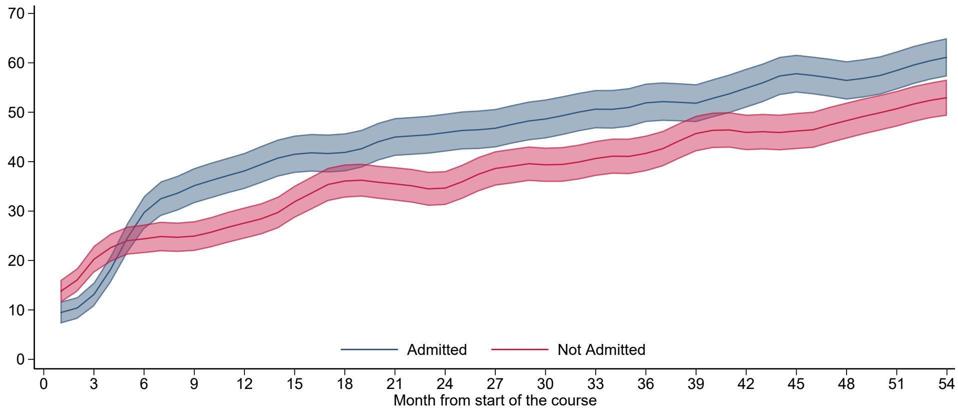
Note: Summary statistics for applicants to the programme. The rows labelled "Employment one/two years before" measure the number of days that the individual worked in the period from 0 to 12/13 to 24 months prior to the day when the course they applied for started. The last two rows report the test score obtained in the admission test, relative to the admission threshold, and the fraction of working days in employment in the period beginning 12 months after the start of the course, and ending at the end of the 54-th month, defined as Y_i in the text. All variables have 1182 observations. The columns reports the mean and standard deviation of the variable, the last column is the result of a t-test, with *** indicating significance at the 1% level, $p \leq 0.01$.
Sources: Agenzia del Lavoro, Trento, see text for details.

the availability to commute, and the willingness to relocate in order to find employment. These variables are highly correlated with those in the table, and the results are unchanged when they are included in various combinations in the regression. Table 1 separates the two subsamples of those not admitted to the course they applied for (the control group) and those admitted (treatment). The last column reports the result of a t-test for the difference between the two means. It highlights the unsurprising difference in the composition in the two groups: those admitted are older, better educated, more likely to be Italian citizen,[5] less willing to accept any job, and have a better employment history at the start of the course.

We focus on two types of measures for the effectiveness of the training courses. The first is Y_i, the percentage of time spent in employment in the 42-month period beginning twelve months after the beginning of the course, when short terms adjustment effects can be assumed to have petered out, and ending four and half years later. The second measure disaggregates the period beginning at the start of the course into its component months. We do so in recognition of the fact that the overall potential effect of a training

[5] Recall that holding a right-to-work permit is a condition for eligibility for admission to the course.

Figure 1: Employment following the start of the training course.



Note: The blue line is the percentage of each month which on average those admitted to the course were in employment, the treatment group. The red line is the corresponding measure for the control group, those not admitted to the course. The shaded areas are the 95% confidence intervals.

course may spread itself across time in an uneven manner, and the temporal pattern of this manner matters both from a positive and a policy viewpoint. So we construct a vector of 54 output variables y_{it}, where y_{it} measures the percentage of time that individual i holds an employment contract in month t, $t = 1, \ldots, 54$. To smooth out the time series, we replace y_{it} with its three-month moving average.

Figure 1 summarises employment after the course for the two groups of individuals, those who were admitted to the course, and those who were not. Given the substantial differences in the composition of the two groups, including in the pattern of employment prior to the course, we should not be surprised to see substantial differences in their pattern of employment following the course. However, as we show below, the score obtained in the test, and hence the determinants of attendance to the course, are not correlated to the subsequent employment history. To document the robustness of our findings, we show that controlling for alternative sets of observable characteristics does not affect results.

3 Econometric approach

Our empirical analysis compares the outcomes of those who were assigned to attend the training course with those who applied for admission, met the eligibility requirements, but were excluded from participation as their scores in the entry test were not high enough. Since the admission rule is based *only* on the score of the applicant at the selection test, under mild conditions we can use the standard regression discontinuity design (RDD, see for instance Cattaneo & Titiunik, 2022), whereby comparing the average outcome for the applicants on either side of the admission cut-off credibly identifies the average causal effect for those assigned to the course whose score is close to this cut-off. In the following, s_i is the score relevant for selection into the programme as observed on individual i. Normalising to zero the cut-off value, the binary treatment status of individual i is:

$$D_i = I(s_i \geq 0) \tag{1}$$

where $I(s_i \geq 0) \in \{0, 1\}$ is the indicator function equal to 1 if $s_i \geq 0$ and to 0 otherwise.

Besides the substantial reduction in the number of observations used to determined the causal effect, the standard RDD design estimates the average effect only for the very specific sub-population of those whose score is near the cut-off. Angrist and Rokkanen (2015) overcome these drawbacks. They prove that, if there is a set of predetermined observable co-variates, $\mathbf{X}$, such that, conditional on $\mathbf{X}$, the correlation between the test score and the outcome is zero both for the treatment group and for the control group, then the regression of the outcome on the treatment status controlling for $\mathbf{X}$ eliminates the selection bias. In detail, following Angrist and Rokkanen (2015), we write the score as a function of individual i's observables $\mathbf{X}$ and of individual i's unobservable characteristics, e_i, that is, $s_i = g\,(x_i, e_i)$. Then, in the set-up we consider, the rule for selection into the programme is:

$$D_i = I(s_i \geq 0) = I\,(g\,(x_i, e_i) \geq 0) \tag{2}$$

In words, conditional on $\mathbf{X}$, the variability of the treatment status D_i is due only to e_i. Angrist and Rokkanen (2015) note that if e_i is uncorrelated to the outcome Y_i both for the treated and for the controls, then, conditional on $\mathbf{X}$, it is as if D_i were randomly determined. In other words, we can extend the estimation of the average causal effect to the entire sample, instead of having to restrict the regression sample to a suitably small neighbourhood of the threshold. Formally, in Section 4, we begin by estimating the regression:

$$Y_i = a_0^G + \alpha_1^G s_i + \alpha_2^G \mathbf{X}_i + u_i, \qquad G = T, C, \tag{3}$$

where Y_i is the outcome of interest for i, and u_i is the error term with standard properties. We run the regression in (3) separately for individuals assigned to take the course, $i \in T$, and those who did not $i \in C$ to test whether the coefficients for s, α_1^T and α_1^C are both zero. This procedure amounts to test whether e_i, the part of the score s_i left after controlling for $\mathbf{X}_i$, is unrelated to the outcome Y_i for both groups. Doing so is equivalent to testing that, controlling for $\mathbf{X}_i$, s does not affect the outcome Y_i either for those below the threshold or for those above it.

Therefore, our main regression, to be estimated on the whole sample, to obtain the overall effect of admission to the course is simply:

$$Y_{ic} = a_0 + \alpha_1 D_{ic} + \alpha_2 s_{ic} + \alpha_3 s_{ic} D_{ic} + \alpha_4 \mathbf{X}_{ic} + f_c + \varepsilon_{ic}. \tag{4}$$

In (4), f_c is a set of course fixed effects, and ε_{ic} is a standard error term.

The sample, that is the total number of people taking the admission tests for these courses, is 1182. Of these 547 obtained a score sufficient to ensure admission to the course they had applied for, the remaining 635 did not. As we pooled all the training courses together, our estimates measure the overall average causal effect.

Of those admitted, 66 individuals did not start the course, and eight attended the course even though they were not initially admitted. Overall, in approximately 6% of the observations, attendance to the course is not determined by the result in the admission test. This implies that we identify the "Intention to Treat" (ITT) effect, namely the causal effect of offering participation, which in principle is likely to be a lower bound on the causal effect of actual participation, even though it could be argued that if those who passed the test and did not attend the course did so because they obtained a job after they passed the test then the treatment group average employment is biased upward by the high values of these high employment individuals. In either case, the small number of observations where the result of the admission test does not correspond to actual attendance ensures that the difference between the ITT and the ATT is also small. At any rate, as the fourth column in Table 2, where these observations are dropped from the sample, shows negligible differences.[6]

4 Results

We begin by checking that the condition proposed by Angrist and Rokkanen (2015) to ensure that the extension of the sample away from the admission threshold does not introduce selection bias holds in our case. To this aim we estimate (3). We do so in two ways, the same two by which we present the main result measuring the causal effect of the training course. First with the outcome variable as the aggregate employment in the three and a half years following the twelfth month after the beginning of the course. Estimation of (3) for the treatment and the controls groups yields:

$$Y_i = a_0^T + \underset{.223}{.114} s_i + \alpha_2^T \mathbf{X}_i + u_i, \qquad i \in T, \tag{5}$$

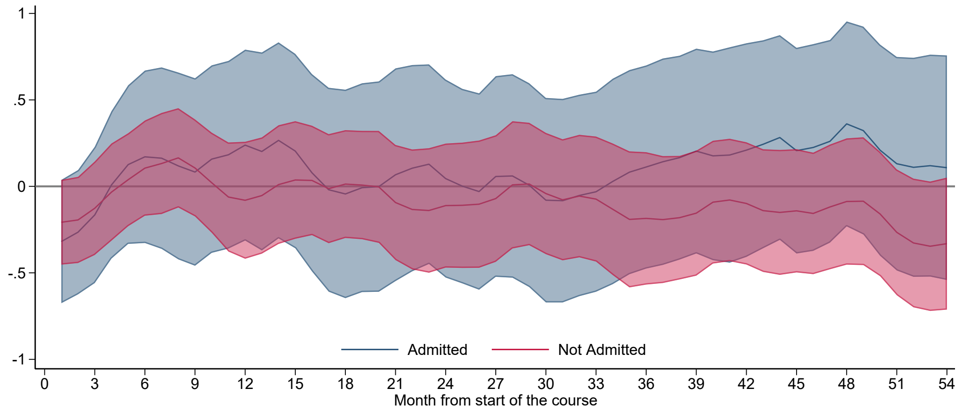
$$Y_i = a_0^C - \underset{.129}{.108} s_i + \alpha_2^C \mathbf{X}_i + u_i, \qquad i \in C. \tag{6}$$

The vector $\mathbf{X}_i$ contains all the control variables presented in Table 1. In both estimations, we cannot reject the hypothesis that the coefficient for the admission test score α_1^G is zero.

Next we show that this is the case not just for the average employment in the four and a half year period following the start of the course, but also in every month since the start of the course. That is, we estimate (5) and (6) with the monthly employment y_{it} in each month t from the beginning of the course, $t = 1, \ldots, 54$. The results of these 54 pairs of regressions are presented in Figure 2: note that the scale on the vertical axis is percentage points, and is therefore comparable with the scale in Figures 3 and 4. This confirms, that controlling for other confounders, the test score is uncorrelated with the ability to find

[6] A further, even smaller, unevenness is the presence of 26 individuals who failed to gain admission at their first attempt, but were admitted at a subsequent second attempt. These trained individuals were included in the control group, thus creating a bias toward zero of our estimate of the ITT effect. Nevertheless, the very small size (about 2% of the sample) of the group of individual with this characteristics makes it likely in this case as well that any bias is negligible.

Figure 2: Effect of the test score on employment.



Note: The vertical coordinate of the blue (red) line is the value of the coefficient a_{1t}^T (a_{1t}^C), that is the effect of the admission score on the percentage of time spent on employment by those who were (who were not) admitted to the course in the t-th month after the beginning of the course for which the worker had applied. The coloured bands are the 95% confidence intervals.

employment in the t-th month after the start of the course for all the months $t = 1, \ldots, 54$. That is, none of the 108 coefficients estimated is statistically significant at the 95% level, and only five are statistically significant at the 90% level.

Relying on these two related pieces of evidence, (5)-(6) and Figure 2, we can argue, following Angrist and Rokkanen (2015), that there is no selection bias conditional on $\mathbf{X}_i$, and so we can use the entire sample as our control and treatment groups in the estimation of (4) to identify the average causal effect of being assigned to taking part into the course.

In it worth noting that, as shown below in the first column in Table 2, these coefficients would remain not significantly different from 0 even if we excluded from the estimation the controls $\mathbf{X}_i$, that is if we restricted $\alpha_2^G = 0$, $G = T, C$, in (5) and (6).

Table 2 reports the results we obtain when estimating (4). We begin with a regression which includes only the admission indicator variable, the test score and their interaction. We expect, and find, that we cannot reject the hypothesis that the coefficient for the admission test score is zero both on its own and when interacted with the admission dummy, that is both for the control and for the treatment group. The admission dummy on its own is instead significantly different from zero, establishing a causal link from attending the course and the probability of finding employment in the three and a half years since the first anniversary of the beginning of the training course. This effect remains when course fixed

Table 2: Effect of admission to the training course on employment.

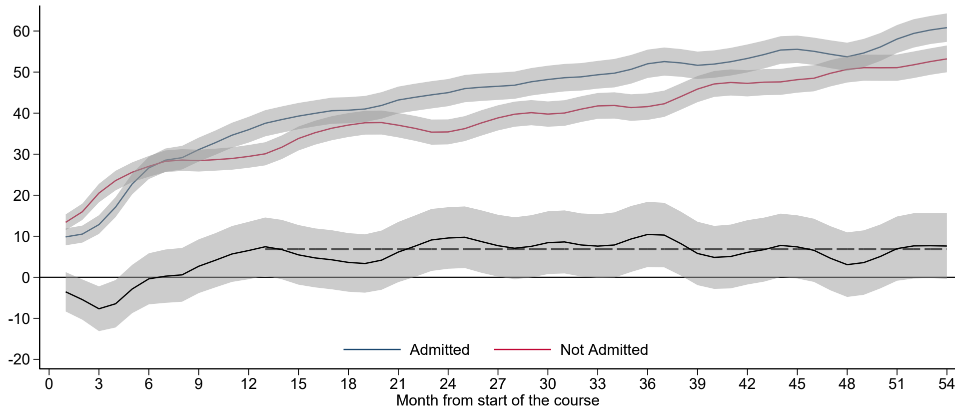
Variable	(1) No controls	(2) Add course fixed eff.	(3) Main: Add controls	(4) Only partecip.
Admitted to the course	7.101**	7.643**	5.923*	5.491*
	(3.221)	(3.196)	(3.040)	(3.152)
Admission test score	0.053	-0.064	-0.151	-0.149
	(0.114)	(0.123)	(0.122)	(0.122)
Admitted × score	0.092	0.392	0.321	0.301
	(0.256)	(0.252)	(0.236)	(0.245)
Age			-1.328*	-1.403**
			(0.691)	(0.696)
Age^2			0.012	0.013
			(0.009)	(0.009)
Female			-4.677*	-4.008
			(2.798)	(2.859)
Italian national			5.785**	6.049**
			(2.515)	(2.562)
High school graduate			0.415	0.053
			(2.615)	(2.697)
Would accept any job			5.726***	5.192**
			(2.169)	(2.224)
Days worked year before			0.076***	0.079***
			(0.013)	(0.013)
Days worked two years before			0.040***	0.036***
			(0.010)	(0.010)
Constant	42.08***	41.83***	58.98***	62.07***
	(2.119)	(6.609)	-14.05	-14.15
Observations	1,182	1,182	1,182	1,119

Note: Column (1) estimates (4) with no course fixed effects, and α_4 restricted to 0. In column (2), course fixed effect are added, and (3), our main regression, we add the vector of controls. The robustness test in the last columns exclude the observations where the worker was admitted to the course but did not attend. * , **, *** denote significance levels, $p < 0.1$, $p < 0.05$, $p < 0.01$, respectively.

effects are included, column (2), and settles at 6% when controlling for relevant individual characteristics, column (3). This is just over a day additional employment per month. When we exclude from the treatment group individuals who, though admitted, did not attend the course the effect changes little relative to the main regression in Column (3). Unsurprisingly, given the limited change in the sample, the divergence from the main regression is slight. Table 2 shows that the controls do have some explanatory, as introducing them reduces marginally the coefficient for having being admitted to the training course. Moreover the sign of their effects is plausible and in line with one's expectations.

We also estimate how this effect is distributed across time. The difficulty of finding a job while attending the course, and the possible adjustment in the initial period from the

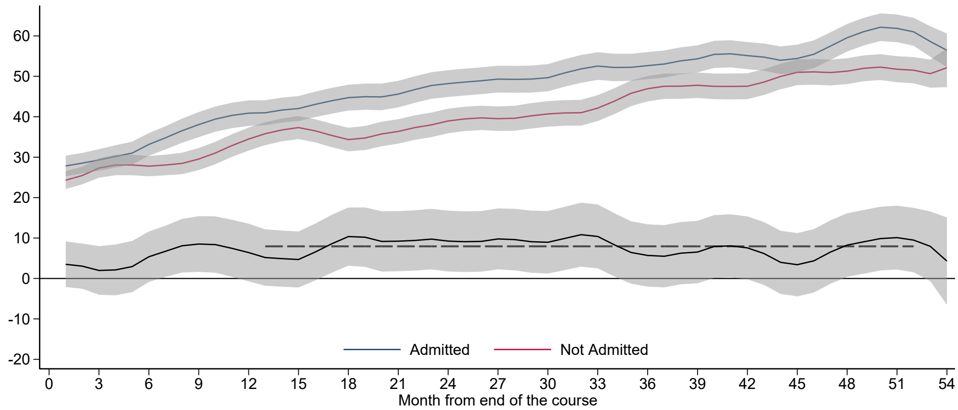
Figure 3: Time trend of the employment coefficient.



Note: The blue line is the sum of the coefficients $\alpha_0 + \alpha_1$ in (4) for the month measured on the horizontal axis, the red line is the coefficient α_0, and the solid black line the difference between them. The height of the horizontal black dashed line is the coefficient α_1 in the third column of Table 2, measuring the average employment effect across the period.

beginning of the course is accounted for by our construction of the regression outcome variable Y_i in (4), which compute the total employment in a period which starts one year after the beginning of the course, when short terms adjustment effects will have petered out. But is the positive effect we find in Table 2 an average of an initial brief spike which fades soon out as the training received recede in the past, eventually becoming obsolete, or does it provide a more limited, but persistent boost? To answer the question we run (4) 54 times, to measure the effect on employment τ months from the start of the training course, with $\tau = 1 \ldots, 54$, and calculate the ITT on the probability of employment in each month τ. The horizontal axis in Figure 3 measures the months from the beginning of the course. The vertical coordinate of the three lines at each point $t = 1, \ldots, 54$ are the coefficients for the admission dummy in the regression in (4) where the output measure is the average employment at time t, $t = 1, \ldots, 54$ in the following three cases: the blue line is the probability of employment of the treated, that is the sum $\alpha_0 + \alpha_1$ in (4) for month t, the red line is the probability of employment of the controls, α_0, and the solid black line the difference between them, measuring therefore our sought ITT: how much the probability of being employed in the n-th month after the start of the course increased as a consequence of having gained admission to the course. The confidence interval for this line is adjusted according to the analysis in Goldstein and Healy (1995). After the

Figure 4: Employment following the end of the training course.



Note: This is the same as Figure 3, except that output is measured from the end of the training course. The height of the horizontal black dashed line is the coefficient α_1 in the third column of Table 2, measuring the average employment effect across the period.

negative effect of the initial period, attributable to the impossibility of holding a job and simultaneously attending a course, the positive effect of attending the training course is evident, with no suggestion that the trainees' success in finding and keeping jobs tapers quickly as time goes on. For reference we have also depicted, as the black solid line, the overall ITT effect for the relevant period, namely the coefficient α_1 in (4). The starting point of this line reflects the fact that we have averaged the post-course employment over the period from months 12 to months 54. Its height is the coefficient α_1 in the third column of Table 2, measuring the average employment effect across the period.

In Figure 4, we study the time trend of the model when the outcome variable y_{it} is individual i's employment in t-th month after the training course. The figure is similar to Figure 3, if anything with a slightly larger employment effect.

5 Conclusion

We study in this paper the effects on employment of a large scale training programme provided in 2013-14 in the north-east of Italy. Our findings of a relatively strong effect, equivalent to approximately one day per person per month of additional employment is a contribution of notable interest in view of the relative scarcity of programmes, and consequently of analyses, where the target is the set of all unemployed workers, not just the young ones, and which are offered in advanced economies.

Our work also offers a novel methodological contribution, as it constitutes a textbook example of the application of the recent theoretical contribution of Angrist and Rokkanen (2015), who show how it is possible to extend the window of applicability of the classic regression discontinuity design approach to the whole sample while avoiding the sample selection problems which often beset this approach by dramatically reducing the sample size, and casting doubts on the validity of the results for units of observation substantially different from those close to the discontinuity.

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