NATIONAL VOCATIONAL EDUCATION
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TECHNICAL PAPER

The effect of a pre-apprenticeship on getting an apprenticeship

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Fellowship Program 2011 participant

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About the research

The effect of a pre-apprenticeship on getting an apprenticeship

Thorsten Stromback, Centre for Labour Market Research, Curtin University

Pre-apprenticeships have been popular for many years with both prospective apprentices and employers — they give prospective apprentices a taste of the relevant trade and therefore should be helpful in ensuring a match between the expectations of a new apprentice and the reality of the trade. However, pre-apprenticeships are by no means the only route into an apprenticeship.

The purpose of this research is to look at the relative impact of pre-apprenticeship programs on the probability of undertaking an apprenticeship.

Any comparison of the effectiveness of pre-apprenticeships in leading to an apprenticeship must be relative to a counterfactual. In this case the counterfactual is students who are undertaking the same courses in Western Australia but not as part of a formal pre-apprenticeship program. In order to take into account that the students undertaking a course as part of a pre-apprenticeship program are likely to have different background characteristics compared with other students undertaking the course, Stromback employs propensity score matching, which matches an individual in the comparison group with an individual in the pre-apprenticeship program. The match is based on factors that explain statistically whether a student is part of the pre-apprenticeship program or not. The most important characteristics turn out to be sex, age and Indigenous status.

Key messages

- For the not-at-school group, the increase in the probability of going on to an apprenticeship the following year is around 11 percentage points if the student is in a pre-apprenticeship program (23% compared with 12% for the comparison group).
- This effect is much smaller than a simple comparison of the two groups, indicating that the differences in personal characteristics are quite important.

The one caveat to these results is that it is likely that those undertaking the pre-apprenticeship program are inherently more interested in the possibility of an apprenticeship than those in the comparison group. Thus the impact of pre-apprenticeship programs cannot really be deduced from the comparisons of the pre-apprenticeship program students with the other students, but they do provide a useful indication of the order of magnitude of the program's effect.

Tom Karmel
Managing Director, NCVER

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Introduction

Using Western Australian data from the National Centre for Vocational Education's (NCVER) VET Provider Collection for 2009 and 2010, this paper uses matching to estimate the effects of pre-apprenticeship activities on student outcomes. Pre-apprenticeship activity is defined as the enrolment in certain courses with the aim of improving a student's prospect of obtaining an apprenticeship. The primary indicator of outcomes is whether a student obtained an apprenticeship during the following year. The point estimates suggests that undertaking a pre-apprenticeship approximately doubles a student's chances of obtaining an apprenticeship, but from a low base of about 10%. These estimates are conditional on an independence assumption that is unlikely to hold. Examining the sensitivity of the results to this assumption indicates that a pre-apprenticeship has a positive effect on students who are not at school, while the effect on students at school is uncertain.

Background

Pre-apprenticeships are courses that provide initial training in a particular trade, which enables potential apprentices to gain an appreciation of the trade and, furthermore, assists them to obtain an apprenticeship. The current form of pre-apprenticeship course has its origin in the prevocational courses that emerged in the early 1970s. At that time youth labour market conditions had deteriorated and many young people were experiencing problems in the transition from school to work (Merrilees 1983). Prevocational full-time courses were thought to be part of the answer, with many such courses developed during the 1970s. These courses took many forms, but pre-apprenticeship courses quickly became the dominant form of prevocational training. Pre-apprenticeships have also proved to be more enduring than other prevocational programs and have continued to play a useful part of the vocational education and training (VET) system. In the early 1970s, New South Wales led the development of pre-apprenticeship courses (Ainley & Fordham 1979). First instituted in 1973 under the name of pre-employment courses, by 1980 enrolments had grown to about 2000 students, who were given training in some 30 occupations (Birkin & Daniels 1981) and who accounted for about 15% of the yearly intake of apprentices. In other states, pre-apprenticeships played a smaller but still significant role (Australian Advisory Apprenticeship Committee 1976; Brindley 1976).

In view of the numerous changes in and expansion of the VET system since that time, the development of pre-apprenticeship training over time is difficult to follow. Generally speaking, its role has been counter-cyclical, expanding at times when school students have experienced a difficult transition and contracting at other times. One estimate put the number of pre-apprentices at 10 000 in 2000 but by 2004 the numbers had fallen to a low of 5500 (Dumbrell & Smith 2007). Since that time there has been a veritable explosion in enrolments, to 64 800 in 2009 (Foley & Blomberg 2011). Most enrolments are in two trade groups: electro-technical and building and architecture. Some of this recent revival of pre-apprenticeship courses has been a response to the economic downturn in 2008—09. During the 12 months ending in June 2009 the intake of new apprentices fell by 15% and consequently many young people found it difficult to gain an apprenticeship in their preferred trade (NCVER 2010). In May 2010 the Commonwealth Government announced that it had allocated $20 million to projects with the aim of increasing pre-apprentice training. This funding was expected to be translated into the partial funding of 5000 pre-apprenticeship places during 2011. But there are also indications that there is a growing preference for the pre-apprenticeship pathway among

employers, students and training providers. In 2010 no less than 28% of completing apprentices in the trade occupations had some kind of prevocational qualification (NCVER 2011).

Reviews of pre-apprenticeship training have been uniformly positive. A 1981 New South Wales review considered that pre-apprenticeship provides basic training that increases students' prospects of obtaining an apprenticeship, with the teachers fully endorsing the value of and need for pre-apprenticeship courses. The students themselves rated the courses as interesting and expected that it will (or has) helped them to get an apprenticeship (Birkin & Daniels 1981). The regular surveys of pre-apprenticeship students during the 1977—82 period lent ample support to these views, showing that 57—71% of completing students obtained an apprenticeship (Putt 1978). An earlier review of the more modest schemes in Queensland found that 53% of completers obtained an apprenticeship (Technical Education Branch & Department of Labour Relations 1978). The many reports and reviews of the apprenticeship system and vocational training at the time were accordingly in favour of expanding pre-apprenticeship training (Williams 1979; National Training Council 1985).

Recent studies of pre-apprenticeships echo the same message. Employers and prospective apprentices regard pre-apprenticeship courses as useful. To employers, they sort the suitable from the less suitable, while prospective apprentices appreciate them and believe they are helpful in getting them an apprenticeship (Dumbrell & Smith 2007). In a review of prevocational programs in New South Wales it was found that 28% of students who began the Trade Start program obtained an apprenticeship (Toner & Woolley 2007). While this figure is much lower than comparable figures of the past, it is nevertheless regarded as a satisfactory outcome. Compared with the 1970s, there is now a multiplicity of options in the VET system, and the apprenticeship path is just one of many alternatives. For example, almost as many, 21%, of Trade Start students undertook further education and training instead of an apprenticeship. The recent studies have also seen a move from using the transition from pre-apprenticeships to apprenticeship as the sole outcome indicator. Thus, Toner and Woolley (2007) note that the apprentice completion rate in New South Wales was 11% higher for students with prior prevocational education and training compared with those without a prior VET qualification. Using regression methods to control for differences between the two groups, Karmel and Oliver (2011) also provide some evidence that doing a pre-apprenticeship might increase the likelihood of completing an apprenticeship. However, the estimates are imprecise and vary considerably between occupations and according to prior educational qualifications.

The role of pre-apprenticeships

In conceptual terms a pre-apprenticeship has three functions. Being a mainly classroom-based mode of training, it provides trade-specific and general skills that prepare a student for an apprenticeship. From the perspective of a potential employer a pre-apprenticeship lowers the cost of training an apprentice. Thus an employer who once rejected an applicant for an apprentice place without a pre-apprenticeship may well accept this same applicant after s/he has done a pre-apprenticeship. In short, pre-apprenticeships shift the cost of training from the employer to the government and the apprentice, which means that individuals can be trained at a cost that makes apprenticeship training viable to employers. In contrast to employer subsidies, it does this at the margin, by converting applicants below the cut-off mark to above the mark. In contrast, subsidies are payable for all apprentices, including those who would have been trained in the absence of subsidies. Pre-apprenticeships expand the pool of suitable candidates for apprenticeship and have the potential to expand the number of apprentices trained.

The second role of a pre-apprenticeship is as a screening device. In that case, it is not the skills or knowledge acquired in a pre-apprenticeship that is the critical factor, but what the successful completion reveals about a prospective apprentice. A screen is useful when the quality of the job match or the quality of the worker cannot be observed by the employer at the time of hiring. The initial probationary period of an apprenticeship fulfils the same role. However, the probationary period is short, and unless the initial training is structured to identify unsuitable apprentices, it can be difficult to do so. Since the offer of an apprenticeship by an employer is in many ways a stronger commitment than permanent employment, a pre-apprenticeship may well be a useful complement to the probationary period. Apart from the wage, the employer incurs significant training costs, and since most apprentices are young and inexperienced, the quality of prospective apprentices and their suitability for a trade can be particularly difficult to judge.

A third role of a pre-apprentice is to bridge temporary gaps between the supply and demand for new apprentices. As is well known, the recruitment of apprentices is highly sensitive to variations in economic activity. In the 1930s depression, apprentice recruitment collapsed and a large cohort of youth missed out on the opportunity to become skilled tradesmen. Subsequent downturns have had a less dramatic effect on recruitment, but even a relatively mild recession has been accompanied by a significant fall in the apprentice intake. Less pronounced, but still important at times, have been variations in the size of the relevant youth cohort.

The main reason why students undertake pre-apprenticeships can be traced to these roles. The majority of students do a pre-apprenticeship because they could not get an apprenticeship but wish to undertake one (Dumbrell & Smith 2007). Their prior skills and knowledge made them too costly to train, employers were not confident about their suitability, or there was an excess demand for apprentice places. It is also the case that pre-apprenticeship courses are designed to meet these roles.

There is, nevertheless, an important caveat. As noted above, a significant proportion of pre-apprenticeship students in the Trades Start program do not move into apprenticeships but choose some other form of further vocational education or training. This reflects a broader pattern, whereby lower-level VET courses, including pre-apprenticeships, are increasingly used for orientation or

enabling purposes (NCVER 2011). Many students are uncertain about what they want and can do and use these courses to inform themselves about the alternatives and their own capabilities. At the same time they gain useful skills that enable them to avail themselves of options that would otherwise not be available to them.

Method

The previous sections established that most students undertake a pre-apprenticeship in order to get an apprenticeship, and pre-apprenticeship courses are designed to fulfil this aim. It was also noted that some students may do a pre-apprenticeship for a variety of other reasons. The possibility of multiple aims is clearly a problem. Ideally, pre-apprenticeship courses should be evaluated with reference to the aims of the participants. This is not possible, however, since far too little is known about the aims at the individual level. The best that can be done is to evaluate the effect of pre-apprenticeships with reference to the principal aim for undertaking a pre-apprenticeship — to get an apprenticeship.

In this paper we use propensity score matching to estimate the effect of doing a pre-apprenticeship, using whether a student obtained an apprenticeship as the indicator of the outcome. In the technical language of matching, doing a pre-apprenticeship is the treatment and the aim of the analysis is to estimate the causal effect of this treatment.

Evaluating the effect of a treatment involves comparing the expected outcome with and without a particular treatment. We define $(Y,^{0}, Y_i^{1})$ as the potential outcomes for the i'th individual if the individual was not treated or treated, respectively, and let D = [0,1] indicate whether an individual received treatment or not. Then the average effect of the treatment on the treated is measured by $E[Y^1-Y^0|D = 1]$. This quantity is not identified since the counterfactual mean, $E[Y^0|D = 1]$ is unobservable. If, however, all the variables that influence the selection into treatment as well as the potential outcomes are observed, then conditional on these confounding variables X, the potential outcomes are stochastically independent of D. This assumption, known as ignorable treatment assignment or conditional independence assumption, means that the counterfactual mean outcome $E[Y^0|D = 1]$ is identified by adjusting the mean of Y among the non-treated for the distribution of X among the treated. Furthermore, Rosenbaum and Rubin (1983) showed that, if the conditional independence assumption is valid, then the potential outcomes are also independent of the assignment to treatment, conditional on a one-dimensional propensity score $p(x) = Pr(D=1|X=x)$. Because of this dimension reduction property, propensity score matching is widely used in evaluation studies. Propensity score matching, however, requires an additional identification assumption, that the probability of assignment is bounded away from zero and one. This is simply a requirement that there is a counterpart for each treated in the population of the non-treated. If all individuals with a certain propensity score are treated, there are no observations of similar individuals who were not treated.

Data

In this paper we use administrative data from the VET Provider Collection to estimate the effects of doing a pre-apprenticeship. Precisely what effects can be estimated is highly conditioned by the limited information in the data. Thus we first describe the data and then explain the effects to be estimated and the method used.

The Western Australian pre-apprenticeship program comprises a set of designated certificate I/II courses that also form part of the ordinary apprenticeship program. This means that courses designated as pre-apprenticeship are also undertaken by apprentices, some trainees, as well as other students for a variety of reasons. For students who are not at school, pre-apprenticeships are generally full-time for six months, with about 60% being classroom-based and the remaining 40% allocated to practical experience and work placements. Students still at school do the course under the School Apprentice Link program over an 18-month period. The pre-apprenticeship course covers two days per week — one day with a VET training provider and a one-day work placement. For school students there are two versions: a family of trades version for those who are not quite sure what to do and a trade-specific version for those who are. Irrespective of how the course is done, there is a mandated three-month time credit towards the completion of an apprenticeship for those who successfully complete a pre-apprentice course. In 2010 there were ten School Apprenticeship Link pathways, 39 certificate I pre-apprenticeship pathways and 31 certificate II pre-apprenticeship pathways listed on the Western Australian Department of Training and Workforce Development's database.

The primary data source is a 2009 unit record enrolment file provided by the Department of Training and Workplace Development. This file includes an additional field distinguishing pre-apprenticeships from apprenticeships, traineeships and other VET enrolments in Western Australia. The file identifies 34 courses with students classified as doing the courses as a pre-apprenticeship activity. Ten of the 34 courses were nationally accredited courses. The remaining 24 courses were training package qualifications. A total of about 16 400 students were enrolled in the 34 courses in Western Australia in 2009. Of these, approximately 3200 students were identified as pre-apprenticeship enrolments. There were 13 200 additional students enrolled in the same courses but not as a pre-apprenticeship enrolment type: 7700 as an apprenticeship, 300 as a traineeship and 5100 as an 'other' enrolment type. These data are linked to the Australian Vocational Education and Training Management Information Statistical Standard (AVETMISS), the national administrative VET database, for 2009 and 2010. This dataset includes information about the students and the observed outcomes. In addition to the Western Australia 2009 enrolment file, we use data from the National VET Provider Collection for 2009 and 2010. These data contain additional enrolment information, including the personal characteristics of the students concerned.

Apprentices and trainees who are enrolled in a pre-apprentice course as defined above cannot be regarded as doing a pre-apprenticeship. The reason they are enrolled in such a course is because it is part of their apprenticeship or traineeship. The course has a dual role: for apprentices/trainees and as a pre-apprenticeship course. The distinction between the two remaining types of enrolment hinges on the motives of students doing the course. Thus students are designated as pre-apprentices (type 'P') if at the time of enrolment they express an interest in progressing to an apprenticeship and as 'other' (type 'O') students otherwise. In many cases, this can be a fine distinction. The interest in doing an apprenticeship can range from weak to strong, and lack of interest among 'other' students can simply reflect an unwillingness to make any form of commitment.

Empirical method

For the purpose of estimating the effect of doing a pre-apprenticeship there are two main limitations to the data.

First, it is not clear who the treated are. Pre-apprentices and other students enrol in the same course, so in a sense both groups are treated. However, their treatment might differ because of associated differences in enrolment status (internal versus external), the support (work placement and mentoring) they receive and differences in their propensity to complete. In addition, their motives for doing the course differ.

The second problem is that the estimation of the causal effect of pre-apprenticeships depends on having a control group from which the counterfactual outcome can be estimated. After matching the treated with this control group on observable variables, the identifying assumption requires that the potential outcomes are independent of selection into treatment, that is, does not depend on any unobservable factors.

For the empirical analysis we have taken pre-apprenticeship students as the treated and the other students as controls. This implies that the treatment is not doing a pre-apprenticeship course per se, but doing the course as a pre-apprenticeship student. Alternative definitions of the treated do not solve the second problem. The selection into treatment, who becomes a pre-apprenticeship or other student, is based on unobservable factors that shape the students' interest in doing pre-apprenticeships. Furthermore, the same unobservable factors shape the students' motivation for obtaining an apprenticeship and their success in achieving this outcome. In short, pre-apprenticeship students are more likely to obtain an apprenticeship than other students due to their greater motivation to secure this outcome. More formally, the conditional independence assumption is unlikely to hold.

The conditional independence assumption is often seen as an all-or-nothing assumption: either it is satisfied and one can then proceed to use matching to estimate a treatment effect, or it is deemed implausible and other methods should be considered. An alternative, which has received much attention in the recent literature, is to relax the assumption and use sensitivity analysis to investigate whether the results obtained are substantially changed by modest violations of the assumption (Imbens & Woolridge 2008). The reason this approach is adopted is that the use of other methods depends on informative data and/or other identifying assumptions that may also be questionable.

In the case at hand, the limited data preclude using another method that does not depend on the conditional independence assumption. Thus we follow the alternative approach: using matching, in conjunction with sensitivity analysis.

Propensity score matching proceeds by first estimating the propensity score, the probability that an individual is treated as a function of the observed confounding variables. For each individual, a matching estimator then imputes the missing outcome by using the outcomes of other individuals with similar propensity scores who were not exposed to the treatment. Averaging over the treated units yields the average treatment effect on the treated. The average treatment effect on the treated is an estimate of the effect of doing a pre-apprenticeship on the outcome, getting an apprenticeship, compared with enrolling in the same course as an 'other' student. Finally, the estimated effect is then subjected to a sensitivity analysis through the introduction of a hypothetical unobserved factor.

Descriptive analysis

The 2009 enrolment file contains observations on 173 447 students enrolled in 216 562 courses. In the analysis we use students rather than courses. To implement this principle: if a student was enrolled in several courses, one course was selected at random to represent the course the student was doing. For courses identified as pre-apprenticeship courses, the enrolments are classified into one of four enrolment types; apart from being a pre-apprenticeship, the enrolment can also be part of an apprenticeship or traineeship. Alternatively, it is classified as being an 'other' enrolment type.

The distribution of enrolment type in pre-apprenticeship courses is given in table 1, distinguishing between students who are doing a VET program while still at school.

Table 1 2009 enrolments in pre-apprenticeship courses

	Apprenticeship	Other	Pre-apprenticeship	Traineeship	Total
Not at school	7 606	2 949	2 253	159	**12 967**
At school	118	2 177	989	161	**3 445**
Total	**7 724**	**5 126**	**3 242**	**320**	**16 412**

Source: AVETMISS Provider Collection data for Western Australia.

Table 2 summarises the information about the principal outcome: whether a student was enrolled in an apprenticeship in 2010. Of the 19 200 apprentice students, the new apprenticeship students comprise the 6325 students who were not enrolled in 2009 plus the transfers from non-apprenticeship enrolments in 2009. The vast majority of students who start an apprenticeship come from outside the VET system. By implication, pre-apprenticeship is not a quantitatively important pathway to an apprenticeship, accounting for only 544 of the 7749 apprenticeship starts.

Table 2 Origins of 2010 apprentice enrolment

	Enrolled in 2009					Not enrolled in 2009	Total apprentice enrolment in 2010
	Apprentice	Other	Pre-apprentice	Trainee	Total		
Not at school	11 353	477	424	70	**12 324**		
At school	98	276	120	57	**551**		
Total	**11 451**	**753**	**544**	**127**	**12 875**	**6 325**	**19 200**

Source: AVETMISS Provider Collection data for Western Australia.

Restricting the attention to the treated and their control (pre-apprentices and other students), the means of selected outcome indicators by treatment category is given in table 3. This table records what happened to students enrolled in pre-apprenticeship courses in 2009 until the end of 2010. The figures show that pre-apprentices did better than the other students for four of the five indicators. For students not at school, 23% of the students who did a pre-apprenticeship progressed to an apprenticeship in the following year. By comparison, only 3% of other students made this transition.

For the other outcome indicators the differences are smaller. Only a very small proportion transitioned to a traineeship. The attachment to VET, as indicated by whether the students were enrolled in 2010, differs little between the two categories. Students who did a pre-apprenticeship are also more likely to have completed their course, either in 2009 or 2010. These figures in particular are

incomplete in the sense that completion is measured up to a point in time only. Of course, all indicators are incomplete in some respects. Not all students in pre-apprenticeship courses in 2009 who progress to an apprenticeship would have done so during 2010. However, the figures permit at least a preliminary assessment of the success of pre-apprenticeship schemes.

Table 3 Students enrolled in a pre-apprenticeship course in 2009: means of outcome indicators by school status and treatment status

		Outcome indicators				
		Had an apprentice training contract in 2010	Had a trainee contract in 2010	Was enrolled in a VET course in 2010	Completed the pre-apprenticeship course in 2009	Completed the pre-apprenticeship in 2010
Not at school	*Pre-apprenticeship students*	0.233	0.001	0.373	0.304	0.061
	Other students	0.032	0.009	0.349	0.166	0.011
	Difference	0.190	-0.008	0.024	0.138	0.050
At school	*Pre-apprenticeship students*	0.159	0.030	0.423	0.219	0.062
	Other students	0.096	0.015	0.382	0.224	0.065
	Difference	0.063	-0.015	0.041	-0.025	-0.003

Source: AVETMISS Provider Collection data for Western Australia.

Any assessment of these figures is necessarily subject to many caveats. The 23% success rate in obtaining an apprenticeship for those not at school is hardly an impressive outcome. We note, however, that the figure is not materially different from the 28% achieved by the New South Wales Trade Start program. The success rate for completers of a pre-apprenticeship course is higher at 30%, but even this figure is modest (table 4). For school-based pre-apprenticeships the relevant proportions are smaller, reflecting the fact that these programs take longer to complete. Overall, 16% recorded a successful outcome (table 3).

Table 4 Proportion with an apprentice contract in 2010: completers and non-completers of 2009 course

		Completers	Non-completers
Not at school	*Pre-apprenticeship students*	0.301	0.187
	Other students	0.035	0.031
	Difference	0.266	0.156
At school	*Pre-apprenticeship students*	0.232	0.129
	Other students	0.123	0.085
	Difference	0.109	0.044

Source: AVETMISS Provider Collection data for Western Australia.

Estimating a treatment effect

The AVETMISS client file records a limited set of personal characteristics, including age, sex and level of education. In total there are 13 personal characteristics, of which 11 are included in the analysis. Several of these variables have a large number of missing values. Omitting all observations with at least one missing value would reduce the sample to almost one-half. Three methods are used to deal with missing covariate values to avoid discarding much of the sample. In some cases, missing values are included as a separate category; in other cases, missing values are assigned the default value of the variable. If neither of these two methods is appropriate, observations with missing values are deleted from the analysis. Table 5 provides the details and also lists the values the covariates take. To make the sample more homogenous we also deleted observations on individuals aged 50 and above and those who completed their highest level of schooling before 1990. These exclusions reduced the sample by less than 1%.

Table 5 List of covariates and their values

Covariate	Values	Missing values
Highest school level completed	Four levels from Year 9 and below Year 12	Separate category for missing values
Year highest school level completed	Five-year intervals	
Sex	Male/female	
Date of birth	Age in five-year intervals	
Postcode	Not included	
Indigenous status	Indigenous/not Indigenous	Missing coded as non-Indigenous
Language other than English spoken at home	Other language/English only	Missing coded as English only
Labour force status	Employed/unemployed/not in the labour force	
Country of birth	Born in Australia/overseas	Missing coded as born in Australia
Disability	Has disability/no disability	Missing coded as having no disability
Prior educational achievement	Post-school qualification/no qualification	
At school	At school/not at school	
Proficiency in spoken English	Very well/less than very well	Coded 'very well' if born in Australia
Address location	Not included	

The first step in propensity score matching is to estimate the propensity scores: the probability of being treated (a pre-apprenticeship student) as a function of the observable personal characteristics.

Notwithstanding our reservations about predictors of selection into treatment, the observable personal characteristics turn out to be very good predictors. For students not at school the pseudo-R^2 for the probit regression is high, at 0.309, and the overall predictive accuracy is 79%.[1] Almost all the included variables are statistically significant at the 1% level. The largest influences are sex, age and Indigenous status. Only the two language variables (language other than English spoken at home and proficiency in spoken English) have no predictive influence. For students still at school, the personal characteristics have a much smaller effect on the selection into treatment, and the fit and predictive accuracy of the model are correspondingly smaller.

[1] Predictive accuracy refers to the correct classification of cases into treated and controls based on the predictive probabilities.

The above findings are reflected in the distribution of the propensity scores depicted in figures 1 and 2. The controls among the not-at-school students are concentrated in the lower end of the distribution and the treated in the higher end. Pre-apprentices and other students are evidently very different with respect to the observable variables. This also means that there are very few controls for the treated in the upper end of the distribution, resulting in some of the treated being matched by the same non-treated observation. Precisely how this works out depends on the matching method. When using calliper matching, which the graphs depict, the limited overlap in the distribution means that some of the treated are not matched with a non-treated counterpart. They are 'off' the common support and excluded from the computation of the matching estimate, which then refers to the sub-population that meets the common support requirement. For students at school there is a greater overlap of the distribution of the propensity scores in the two samples and only two of the treated are off the common support.

Figure 1 Distribution of the propensity score: not-at-school students

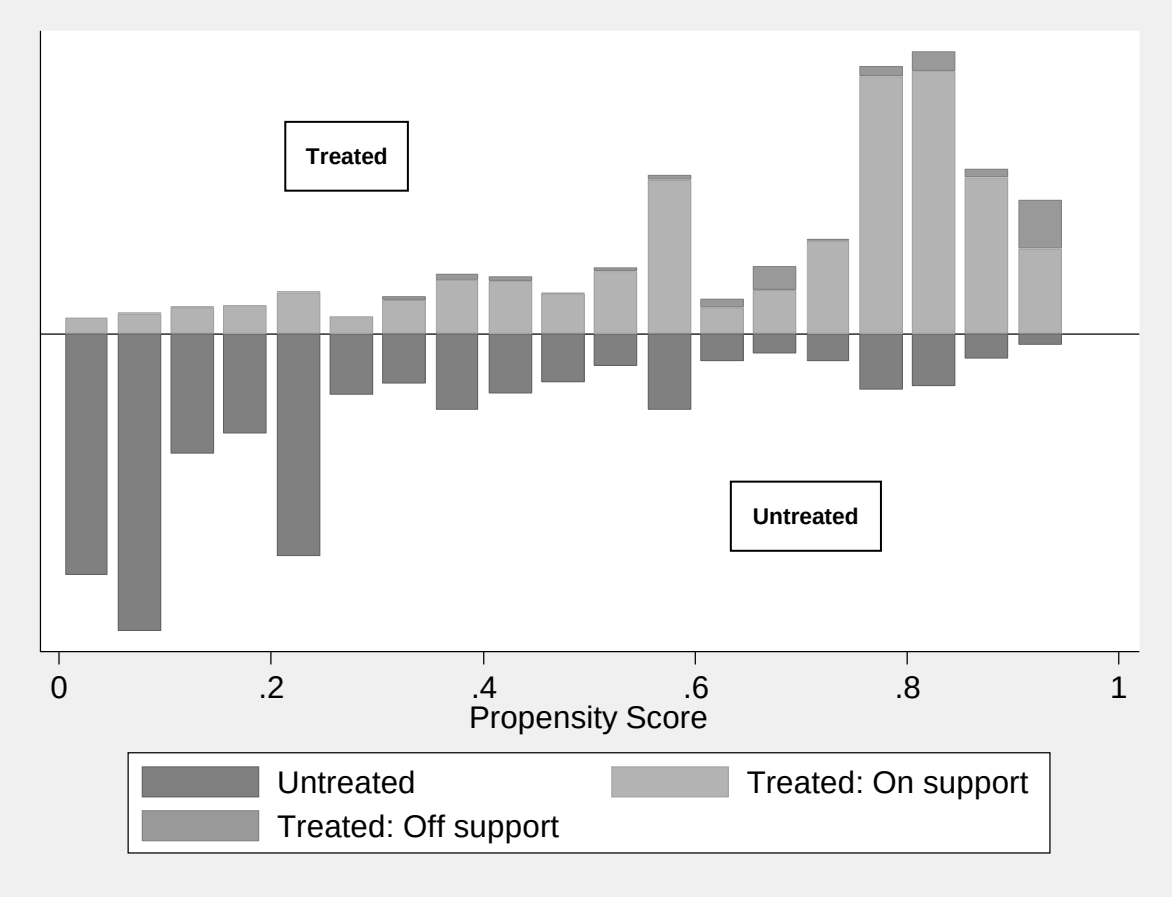
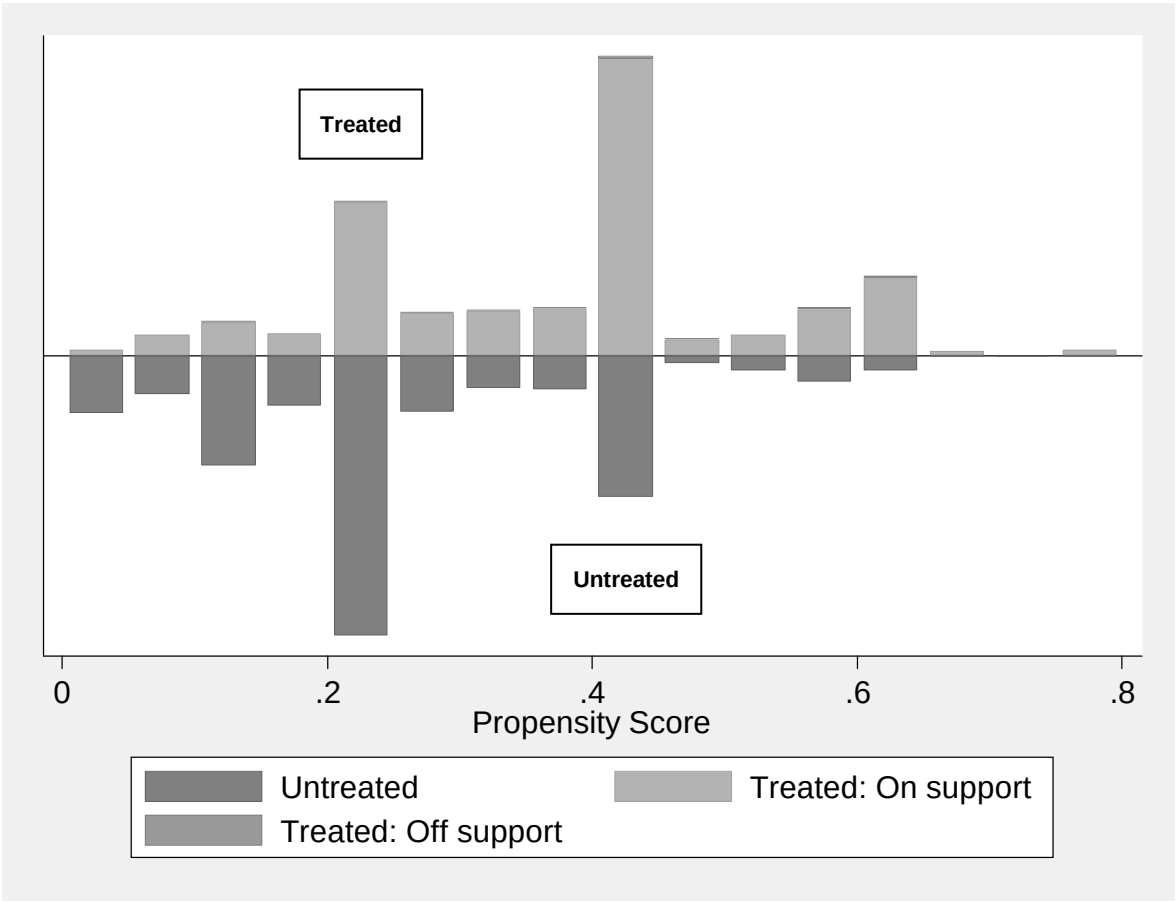


Figure 2 Distribution of the propensity score: school students



The quality of the matching procedure is assessed by the standardised mean difference in the means of the treated and control observations after matching. Before matching, the median standardised bias is 38% for the not-at-school students. After matching, this bias is reduced to 2.4%. Further details are given in table 6, which gives the percentile distribution of the standardised bias before and after matching. As regards not-at-school students, the treated and controls are very different with respect to most of the covariates, but matching reduces most of the differences. Students at school are more similar to start with, but even so, matching reduces the median difference to only 1.41%. Even the largest difference between covariates is small, at 3.72%.

Table 6 Summary of the distribution of the standardised absolute bias before and after matching

Percentile	Not at school students		At school students	
	Before	After	Before	After
1	4.64	.394	2.76	0
5	5.14	.396	2.76	0
10	10.44	.449	7.85	0.199
25	17.01	.916	9.01	0.673
50	37.59	2.36	13.68	1.41
75	47.67	5.246	30.54	2.12
90	88.91	9.415	39.41	2.55
95	106.14	11.48	54.87	3.72
99	119.28	13.33	54.87	3.72

Note: The standardised absolute bias is the absolute difference in means of the covariates of the treated and control groups standardised by dividing by the overall mean for each of the variables.

The estimation of treatment effects by matching methods is a non-standard process. The four most common methods are one-to-one matching, nearest neighbours, localised linear regression, radius and kernel matching. In turn, each of these methods can be implemented in several different ways. There is, however, little to guide the selection of a particular method for a specific application. In applied work therefore it is common to select one method on a priori grounds and, if deemed necessary, to investigate how robust the estimates are if one or more of the other methods are used. Here we simply report the results from using all of these six basic methods. A basic description of these methods can be found in Caliendo and Kopeinig (2008).

Matching estimates of treatment effects are given in table 7. The top half of the table presents the results for not-at-school students, using six different methods of matching. The different methods all match the treated with controls equally well using the mean standardised bias as the measure of matching quality. All methods also yield similar estimates of the treatment effect, of about 11 percentage points. In other words, by doing a course as a pre-apprenticeship activity, a student would have increased his/her chances of obtaining an apprenticeship by 11 percentage point, to 23%, compared with doing the same course for another reason. This effect is much smaller than the unmatched mean difference between the two groups given in table 3. It implies that about one-half of the difference is due to the differences in the personal characteristics.

For students still at school there are much larger differences between the methods. While there are no compelling reasons for why one estimate should be any better than another, the well-balanced treatment and control groups used in the calliper estimates (with a standardised bias of 0.61%) suggest that the lower estimates, in the six to nine-percentage point range, are more credible than the higher estimates. We note, however, that although the effect is smaller for the at-school students, the proportionate effect is of the same order of magnitude for both groups.

Table 7 Estimates of the average effect of treatment on the treated

		Means		
Not at school (N = 5121)	Treated	Controls	Difference	Median standardised bias before matching
Matching method	*ATT*	*Standard error*	*Off common support*	*Median standardised bias after matching*
One-to one	0.105	0.033	2 treated	2.36
Nearest neighbour	0.096	0.018	2 treated	2.42
Local linear regression	0.129	0.013	2 treated	2.42
Kernel	0.110	0.011	2 treated	2.75
Calliper	0.106	0.012	82 treated	1.51
At school (N = 3519)	Treated	Controls	Difference	Median standardised bias before matching
Matching method	*ATT*	*Standard error*	*Off common support*	*Median standardised bias after matching*
One-to-one	0.155	0.056	-	1.41
Nearest neighbours	0.130	0.034	-	1.42
Local linear regression	0.088	0.034	-	1.41
Kernel	0.061	0.013	-	1.78
Calliper	0.065	0.014	29 treated	0.61

Sensitivity analysis

The estimates of the causal effects in the previous section are based on the conditional independence assumption. If this assumption does not hold, because the treated and controls differ with respect to unobservable factors, the estimates of the treatment effects are biased. In the case at hand, the main concern is the individual preferences and motivation that influence both the outcome — getting an apprenticeship — and the selection into treatment — doing a pre-apprenticeship course. While it is not possible to test for the omission of these factors, it is possible to assess whether inference about the treatment effects may be altered by unobserved factors. More precisely, it is possible to determine how strongly an unmeasured factor must influence the selection process to undermine the inferences based on matching analysis.

The Rosenbaum (1995) method for doing this is based on the assumption that the individual treatment probability (P_i) is determined not only by the observable factors (X) but also by an unobservable component U, i.e. $P_i = F(x_i, u_i)$. The latter effect means that two individuals with the same x have different chances of receiving treatment. Assigning hypothetical values or a distribution to U and varying its relative importance allow the researcher to assess the sensitivity of the inferences. Based on that, bounds for significance levels and confidence intervals can be derived. Becker and Caliendo (2007) provide an implementation of this method in Stata.

Suppose that motivation is the omitted factor and that students are either strongly motivated to obtain an apprenticeship (U = 1) or not motivated (U = 0). If the probability of being treated as a function of X and U takes a particular form, it can be shown that the odds ratio of being treated is bounded by

$$1/e^{\gamma} \leq P_i(1-P_j)/P_j(1-P_i) \geq e^{\gamma}$$

where γ is a parameter that determines the relative influence of U on the probability of being treated.

If $e^{\gamma} = 1$ the odds ratio is one, and two individuals with the same observable characteristics have the same probability of being treated. There is, in other words, no selection bias due to omitted factors. As e^{γ} increases, the weight of the unobservable factor increases and the relative odds of motivated and non-motivated students can move further and further apart. At $e^{\gamma} = 2$ the relative odds can differ by as much as a factor of two, even if they are identical in respect of the observable variables.

To assess the sensitivity we simply increase the value of e^{γ}, starting at one and repeatedly test whether the null of no treatment effect can be rejected as e^{γ} is increased. If the test cannot reject the null as the influence of an omitted factor is increased, the original inference of a positive treatment effect remains intact. Alternatively, and depending on the significance level of the test, we conclude that the original inference (here of a positive effect) is sensitive to an omitted factor. This assessment is based on the Mantel and Haenszel (MH) test statistics originally developed to test for repeated independence. This MH statistic is based on a comparison of the number of positive outcomes among the treated with the expected number under the null that the treatment effect is zero.

The results of the sensitivity analysis are given in table 8. In the case of students not at school we note that if there are no omitted factors, $e^{\gamma} = 1$, the MH test statistic is 21.37 with a p-value close to zero. This simply restates the findings in table 7, that there is strong evidence of a positive treatment effect. As e^{γ} is increased, we increase the relative odds of the treated being motivated students and

having a positive outcome (getting an apprenticeship). This reduces the value of the test statistic reflecting the increasing uncertainty. But the rate of this reduction is small. Even the case where $e^{\gamma} = 2$, the MH statistic is highly significant, with a p value close to zero. For at-school students the evidence for a positive treatment effect is much more sensitive to omitted factors. For any value of e^{γ} greater than about 1.3, the hypothesis of no treatment effect cannot be rejected at the 5% level.

Table 8 Manter-Haenszel test statistics for a positive treatment effect

e^{γ}	Not at school		At school	
	MH	p-value	MH	p-value
1	21.37	0.000	5.07	0.000
1.5	16.55	0.000	1.31	0.094
2	13.41	0.000	1.22	0.110
2.5	11.21	0.000	0.53	0.299
3	9.35	0.000	0.09	0.426

It is not possible to estimate the magnitude of any unobservable selection effect or correct the estimate for any such effect. A sensitivity analysis leaves the point estimates unaffected. By the same token, sensitivity analysis is not a test of the conditional independence assumption: whether there are any omitted factors that affect selection into treatment or the outcome. In respect of not-at-school students, all we can say is that the evidence in favour of a positive treatment effect is robust: insensitive to an omitted factor even if it has a large influence on the selection into treatment. In the case of at-school students, the opposite result was obtained. The inference that treatment has a positive effect is sensitive to omitted factors. Since we cannot exclude this possibility, we cannot be confident that there is indeed a positive effect.

Conclusions

As noted in the introduction, there has been a renewed interest in pre-apprenticeships as a pathway to apprenticeships, and the number of pre-apprenticeship students has increased markedly in recent times. In Western Australia there are no fewer than 70 VET courses designated as pre-apprenticeship courses available both to students in general and to those still at school. However, in the data used for this study pre-apprenticeship courses account for less than 2% of the total VET enrolment, and only 7% of commencing apprentices have undertaken a pre-apprenticeship course.

Using matching methods, this paper estimates the effect of pre-apprenticeships on the principal indicator of outcomes — whether a pre-apprenticeship increases a student's chances of getting an apprenticeship. In the case of not-at-school students, the point estimate suggests that the effect of a pre-apprenticeship is to increase their chance of getting an apprenticeship by about 11 percentage points, from 12 to 23%, that is, roughly doubling their chances. For at-school students, the percentage estimates are smaller but the proportionate effect, an approximate doubling, the same. These point estimates, however, are conditional on an independence assumption that is unlikely to hold. Examining the sensitivity of the results to this assumption, it was found that the result for the not-at-school students was not sensitive to the independence assumption. That is, even though the point estimate may be subject to a positive bias, we could reject the hypothesis that a pre-apprenticeship has a zero effect. The results for at-school students were much more sensitive to the independence assumption and we were unable to infer that the effect is positive with any degree of confidence.

The weak findings raise the question of whether stronger results could have been obtained if more and better data had been available. Since the aim of pre-apprenticeship students is to get an apprenticeship, whether they do is an obvious indicator of success. But it is the use of this indicator that creates an identification problem. Only those who wish to get an apprenticeship, but have not been able to get one, are going to do a pre-apprenticeship. Pre-apprentices are a selected group, and their selection is based on their common aim to get an apprenticeship, which in turn is shaped by unobservable factors. Because of that, they will always be more successful in getting an apprenticeship than individuals who have no desire to do so.

It follows that apprentices constitute the only control group that might satisfy the assumption required to identify a treatment effect; only apprentices have the same (unobservable) preferences as pre-apprentices — a wish to do an apprenticeship. Thus, one approach is to view pre-apprenticeships as being one of two ways of joining a trade, the other being starting an apprenticeship without first undertaking a pre-apprenticeship course. One could then use the matched survival (in the combined pre- and apprenticeships) analysis to follow the two groups over time to estimate the pre-apprenticeship treatment effect — whether students who have done a pre-apprenticeship survive longer than those who go directly into an apprenticeship. In the limit, this reduces to a comparison of the apprentice completion rates. Given the low transition rate from pre-apprenticeship to apprenticeship noted in this paper, such an analysis is likely to show that a pre-apprenticeship has a negative effect on the survival rate.

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