Longitudinal Surveys of Australian Youth

Research Report 38

Assessing the Value of Additional Years of Schooling for the Non-academically Inclined

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Assessing the Value of Additional Years of Schooling for the Non-academically Inclined

1. INTRODUCTION

Schooling plays several important roles in society, and the relative emphasis between these roles shifts as students progress through their school life. Toward the final years of secondary education the main role of schools is to help prepare and direct students into the first stage of their careers, be that university, further vocational education and training or directly into the workforce. This preparation includes the provision of general education required in those anticipated careers, as well as the mechanisms to signal to students, employers and institutions which students are best suited to alternative jobs and pathways. After some point, the net social gain from each additional year of schooling will begin to diminish, and we can think of a "break-even point" which defines the socially optimal number of years of schooling. This concept is perhaps most commonly understood within the framework of human capital theory.

The optimal number of years of schooling will slowly change as a result of structural and technological change in the pattern of production, and will fluctuate with macro-economic conditions – with the youth unemployment rate in particular. It will also differ significantly between individuals according to their skills, career aspirations and other attributes. Skill-bias technological change, high youth unemployment rates and the superior labour market outcomes of those who complete Year 12 have all been used at various times to argue that school retention rates and/or the compulsory level of schooling need to be increased. However these arguments rarely address, in any quantitative sense, what the optimal level of schooling may be and they ignore the critical issue of individual heterogeneity — how that optimum may vary from one individual to another.

Numerous studies have identified labour market benefits that arise from further years of schooling. The more sophisticated of these attempt to control for omitted variable bias, particularly in the context of distinguishing true benefits of additional years of schooling from the "screening hypothesis". The screening hypothesis states that the superior outcomes observed for those with higher levels of education are due to the inherent characteristics already possessed by those individuals who continue on in education. That is, further education screens out or "signals" the most capable, but does not in itself add to their productivity and earnings capacity other than by improving the information set for matching individuals to jobs (see Harmon, Oosterbeek and Walker 2000: 19-22). More generally, a correlation between labour market outcomes and selection into further schooling and education, or synergies between ability and education, may lead to over-estimates of the benefits of extra years of schooling if these are not adequately controlled for in the estimation. While considerable care has been taken to identify the "true" effect of further years of schooling or higher levels of education, the focus continues to be on estimating the "effect" of education for those more likely to participate in it.

In contrast, this paper seeks to assess the benefits of additional years of schooling for those Australian youth who are not well suited to further education. This has a very important policy context as school retention rates have increased markedly over recent decades and many traditional VET pathways have disappeared. Raising the compulsory schooling age is regularly put forward as a policy response to high youth unemployment rates. However, there is a lack of empirical evidence available to show how this may impact on those affected. If, for some segments of a cohort, schooling does largely serve as a signalling mechanism, then increasing the compulsory schooling age may only serve to devalue the information content and reduce the efficiency of the youth labour market.

In this report data from the 1995 Year 9 Cohort of the Longitudinal Surveys of Australian Youth (LSAY) is used along with a variety of empirical approaches to assess the benefits of additional years of schooling for various groups of youth conditional upon their estimated propensity to engage in further schooling. Background material is provided on the school to work transition in Australia (Section 2), previous literature on estimating returns to education (Section 3) and the LSAY survey and cohort sample (Section 4). The impact of the number of years of schooling completed upon wages and the incidence of unemployment over the years of 1997 to 2002 is tested, equating to the (modal) ages of 16 to 21 for the cohort. The empirical estimates are contained in Sections 5 to 7 of the paper. A model of the propensity of young people to complete high school is developed in Section 5. Sections 6 and 7 contain estimates of the impact of years of schooling on earnings and employment status, respectively, utilising the results from the model of propensity to complete school. The evaluation techniques tested include segmenting the sample according to measures of "academic inclination", instrumental variables and propensity score matching. The concluding section provides a summary of the findings and discusses their implications.

2. THE SCHOOL TO WORK TRANSITION - A BACKGROUND

The role of schooling

One can distinguish between three main roles of schools. First, specialisation and economies of scale in the supervision of children frees up other adults (parents) such that they can pursue other economic activities or leisure. Second, schools provide a "pedagogical" system which instils within youth socially acceptable behaviours and attitudes and which helps them to develop the social interaction skills required to realise their own ambitions and goals within society. Third, schools impart more specific vocational knowledge and abilities to improve individuals' potential economic contribution to society or, from an individualistic perspective, to improve their own earnings capacity during their working life. This includes the role of matching individuals to external (non-school) pathways, such as to maximise their future contribution.

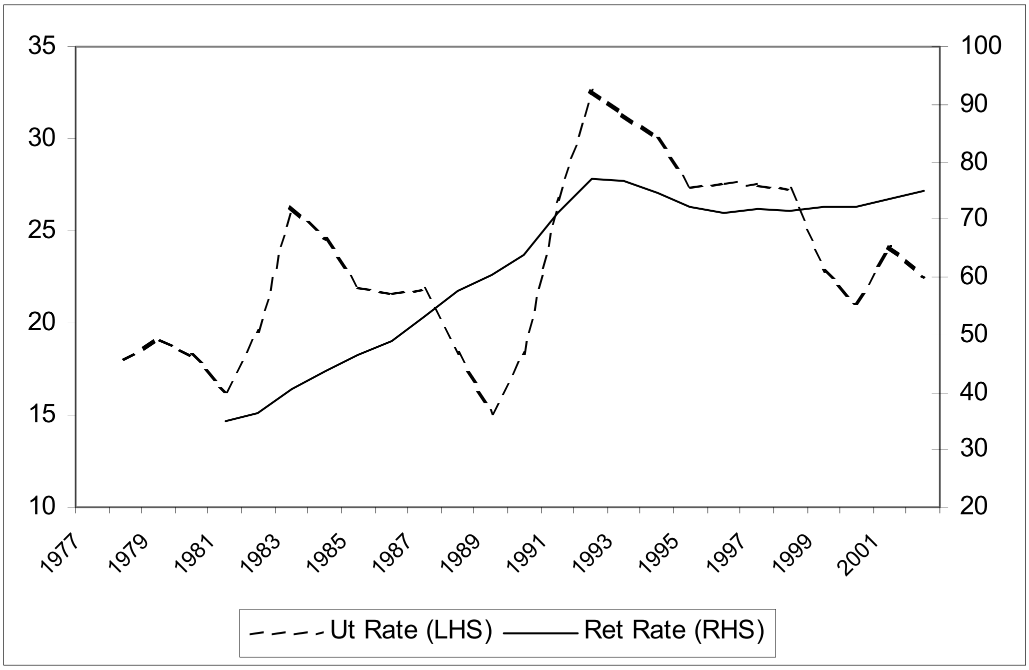
The relative importance of these roles shifts over the schooling period. The relative importance of the "child-minding" role will be greatest in the early years, the pedagogical role will be greatest within the middle years, while the vocational aspect of schooling will be of most importance during the latter years. What then is the optimal amount (duration) of schooling that should be provided? Clearly this will depend upon a number of factors. One is how quickly youth "mature" such that the child-minding role is no longer required and the marginal utility of further pedagogical treatment is diminished. In terms of the economic contribution, we can reasonably assume that the potential economic contribution of youth if they were to leave school increases with age and years of schooling. Put another way, the opportunity cost of remaining in school will start to increase after a given point. This includes the options of directly entering the workforce or of raising earnings potential by entering other channels of vocational education and training. The net present value of the gains from additional years of schooling will eventually begin to decline.

This implies that there is an optimal number of years of schooling where the benefits of further years of schooling equate to the opportunity cost. Presumably the legislated compulsory school age is set such that, for most individuals, the returns from the pedagogical (societal) contribution of school have become marginal relative to the economic cost-benefit decision, and thus the calculation of the net social benefit of additional years of schooling reduces to the individual's private calculations of the net benefit. That is, the private incentives to individuals to participate in further years of schooling beyond the compulsory level will be sufficient to ensure that outcomes approximate a socially optimal level of schooling.

The optimal number of years of schooling for an individual will not be fixed but will fluctuate with economic conditions, the prevailing technology and the supply and efficiency of alternative pathways. More importantly, it will differ between individuals according to their abilities, pedagogical requirements, career aspirations and other preferences. Let us assume that, by the end of the final year of compulsory schooling (in Australia the year in which students turn 15), the childcare role of the education system is insignificant and society has finished "shaping" the individual's behaviour. The decision to stay on at school becomes one based on the individual's preferences and expected stream of earnings and non-pecuniary utility of the various pathways open to them, including entering the labour market directly, leaving school to enter further VET and remaining in school. If the social role of schooling is inconsequential by this stage, there seems no need for intervention in the individual's decision through public policy, other than perhaps to improve the information set available to the student in making this decision. In fact, since schooling is mostly financed publicly, one might expect that individuals would tend to over-invest in schooling from a social perspective. This of course ignores the role of taxation, welfare payments and other externalities in that reckoning.

The school to work transition in Australia

Schooling is compulsory until the age of 15 in all states in Australia. The apparent school retention rate from Year 7/8 to Year 12 stood at 75.1 percent for 2002. School retention rates increased markedly from around 35 percent in the mid-1970s to a high of 77 percent in 1992, from which they appear to have reached a plateau between 70 and 75 percent. At least in part, the rapid increase observed from the 1970s to early 1990s can be seen as a response to rising rates of youth unemployment. The rise in youth unemployment from the mid-1970s was common to most advanced economies and has been attributed to "skill bias-technological change" which has reduced the number of lower-skilled entry level jobs in the economy; increased competition as the female participation rate has risen and lower relative quality of young people entering the labour force as retention rates in education have increased (Ryan and Büchtemann 1996: 308).



Sources: ABS Labour Force Survey – AusStats online statistics and Historical Estimates; ABS Schools Australia, Cat. Nos. 4224.0, 4221.0, various editions; Everingham (1999).

Figure 1 Apparent retention rates from Year 7/8 to Year 12 and youth unemployment rates: Australia

Upon leaving school, the main pathways into the employed workforce open to youth are via tertiary (university) education; via some other form of vocational education and training such as TAFE; directly entering a job; or through some model which combines employment with further vocational education and training, such as apprenticeships or traineeships. Generally entrance into university requires completion of Year 12 and this is the favoured pathway of those youth with better academic results. It is well documented that those who do gain university degrees experience better than average labour market outcomes – lower incidences of unemployment and higher earnings. How much of this is due to the education *per se* and how much is due to their pre-existing individual attributes is not clear.

Apprenticeships were a major pathway from school to work in Australia, particularly for males and early school leavers. However, overall youth participation rates in apprenticeships had been in decline to the 1990s and average starting ages in apprenticeships have increased, such that young people now enter apprenticeships with more years of completed schooling (see Lamb 1997: 16). Much of that decline in apprenticeships was simply due to the decline in the share of output in sectors that employ tradespersons, but the side effect was a decline in the "alternance"[1] model in facilitating the transition from school to work. By and large, the apprenticeship model had failed to extend beyond the traditional trades and into the emerging occupations and sectors (see Dockery 1996). The New Apprenticeships System reforms of 1998 and the incorporation of vocational subjects into school curricula are two examples of policy initiatives to maintain such options for school leavers which have seen a significant rebound in rates of participation in apprenticeships.

The transition to work is relatively unproblematic for the group who successfully complete tertiary studies and our concern here is with young people who are not academically inclined or otherwise not well suited to higher education. Access Economics presents data on outcomes in May of 2001 for school leavers of the previous year (see Table 1). Around one-third of the school leavers went into university and one-quarter into TAFE, while 40 percent were not in education or training. The short-term outcomes differ markedly between early school leavers and those who completed Year 12. Very few early school leavers gain entrance to university but their representation in TAFE is greater. However, 63 percent of this group were not in education or training and they experienced markedly higher incidences of unemployment and non-participation in the labour market than the 29 percent of Year 12 completers who were not in education and training.

[1] The incorporation of learning in the work environment as part of formal education.

Table 1 Short –term outcomes for year 2000 school leavers, May 2001

	Early school leavers		Year 12 completers		Total	
	No.	%	No.	%	No.	%
In education & training						
TAFE	26,024	30.4	41,252[a]	22.4	67,276	24.9
University	1,096	1.3	83,291	45.2	84,387	31.3
Other education or training	4,460	5.2	5,932	3.2	10,392	3.9
Total	*31,580*	*36.9*	*130,475*	*70.9*	*162,055*	*60.1*
Not in education & training						
Employed	27,878	32.6	40,239	21.9	68,117	25.3
Unemployed	15,204	17.8	8,744	4.8	23,948	8.9
Not in labour force	10,908	12.7	4,617	2.5	15,525	5.8
Total	*53,990*	*63.1*	*53,600*[a]	*29.1*	*107,590*	*39.9*
Total	**85,570**	**100.0**	**184,075**	**100.0**	**269,645**	**100.0**

Source: Access Economics 2002, Table 9, p. 12 (compiled from unpublished data from the ABS Education to Work, Cat no. 6227.0).

Note: a. Figures have been adjusted due to apparent errors in the original table (Access Economics 2002, Table 9)

The Longitudinal Surveys of Australian Youth have been used in a number of previous studies to investigate the transition from school to work, including Marks, Hillman and Beavis (2003), Lamb and McKenzie (2001) and Lamb (1997). Ryan (2003: 6-8) provides a review of the Australian literature. Lamb (1997) looks at the early experiences of school leavers and finds literacy and numeracy levels to be major determinants in both school retention and early post-school outcomes. From linear regression models, having completed school is identified as being associated with significantly reduced time in unemployment by age 19. Like this present study, Lamb and McKenzie (2001) focus on school leavers who do not go on to complete either a university degree or Associate Diploma. Using data on the first seven post-school years[2], the major "pathways" from school are identified for a cohort of school leavers from the late 1980s. Inferior outcomes are observed for early school leavers. Those more likely to have a "successful" transition into stable employment were higher achievers at school, more likely to complete Year 12, from higher socio-economic backgrounds and without disabilities. Marks *et al* (2003) similarly find completion of Year 12 to have a beneficial association with the proportion of time spent in full-time work over the ages from 20 to 24 and a small, negative "scarring" effect from time spent looking for work.

In light of such evidence, both here and overseas, increased completion rates of upper-secondary school have become a common policy goal for improving school-to-work transition (Ryan and Büchtemann 1996: 310). Access Economics identifies low school achievement (including poor literacy or numeracy), dislike of school, lower socio-economic background, parental education and occupational background, being of Aboriginal or Torres Strait Islander descent or from a non-English speaking background as factors associated with early school leaving (2002: 10). Modelling commissioned by the Business Council of Australia estimates that increasing participation rates in education and training to Year 12 equivalent from 80% to 90% would generate a 0.28

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2 That is, the first seven post-Year 12 years for the cohort, or until age 24 or 25.

percent increase in GDP per annum by 2020 (BCA 2003: 13). Lamb and McKenzie argue that early school leaving should be prevented since chances of successful intervention are higher while young people are still at school and "Intensive measures to help early leavers in the labour market can be all the more effective if resources are freed up by keeping their numbers low in the first place." (2001: ix).

Policy to increase retention is usually based on such observations of the superior outcomes of completers relative to early school leavers. Thus, the implicit assumption is that if those who currently do not finish school were instead to go on and complete Year 12, they would then achieve outcomes similar to those observed for youth who currently do complete Year 12. As discussed in the following section that assumption may not be so well founded. Indeed, in their study of LSAY data, Marks *et al* identify a large beneficial effect of prior time in full-time employment upon labour market outcomes for persons aged 21 to 25 years — one which is "substantially stronger" than the benefits associated with completion of Year 12 or post-secondary qualifications (2003: vi).

3. ESTIMATING THE RETURNS TO SCHOOLING

Few relationships in economics have attracted a greater degree of empirical scrutiny than that between years of education and labour market outcomes, and more specifically between years of education and earnings. It seems almost that the methodological challenges in estimating the effect have become of more interest to the profession than what that effect may actually be. The dominant theoretical framework for assessing the returns to schooling and education is Human Capital Theory, which has been extensively developed from the initial contributions of Becker (1962) and Schultz (1962). Human Capital Theory treats participation in education and training as an "investment" in human capital. Analogous to investments in physical capital, the initial costs of participation (direct costs and foregone earnings) are recouped through higher post-education productivity and hence higher earnings. The theory's necessary condition that education actually raises productivity is challenged by the screening hypothesis. The screening hypothesis argues that productivity is largely a characteristic inherent to the job rather than to the worker. The observed relationship between education and earnings thus appears simply because educational qualifications are used as a signalling device to allocate "better" workers to higher earning jobs.

Identifying the effect of additional years of schooling and post-school education is an example of the standard evaluation problem. Take an individual who has S years of schooling or education, and whose life-time earnings profile over time is observed to be $Y_t \mid S, t = 1...n$ where t denotes time periods. To assess the effect of additional years of schooling we need to observe the individual's earnings conditional on a different level of schooling, perhaps $Y_t \mid S-1$ or $Y_t \mid S+1$ for each period $t = 1...n$. In practice we can only ever observe earnings conditional on the actual level of schooling attained, and the evaluation problem is one of establishing the counterfactual.

A basic approach is to compare average earnings for individuals conditional upon the number of years of education they have received. To control for other characteristics that may also affect earnings a multivariate model of the following form may be estimated:

(1) $$\ln Y_i = \alpha + \delta S_i + \beta X_i + \mu_i$$

in which X_i is a vector of individual characteristics and β the associated vector of coefficients to be estimated. The coefficient on S, delta, represents an estimate of the effect of additional years of education — using this functional form it represents the percentage wage premium associated with each additional year. However, δ can be expected to be a biased estimate of the "true" effect on several grounds and the extensive literature reflects the attempts to refine this estimate through econometric methods that require less strict assumptions or utilisation of "quasi-experimental" situations in which some of the conditions for obtaining an unbiased estimate are present in the data. See Griliches (1977), Card (2001) and Harmon *et al* (2000) for reviews of this literature.

The major concern with estimating a specification such as (1) above is that of "ability bias". If those persons who have greater "ability" are likely to acquire more years of schooling and would, in any case, have achieved higher earnings, then the estimate of the impact of schooling will be biased upwards if the other variables in X do not adequately capture ability. That is, observed educational attainment is acting as a proxy for unobservable differences in pre-existing ability. This is simply a specific example of selection bias on unobservables, where the unobserved factors in this case are various dimensions of innate ability. There is a more general concern of omitted variables or imperfectly measured variables which may be correlated with both the level of schooling achieved and wages, such as family resources.

One source of endogeneity between ability and the level of schooling accumulated, and thus a source of bias in the estimate of δ, relates to parents' capacity to finance their children's education. As an "investment" decision, the chosen number of years of education depends upon both the initial costs, including foregone earnings, and the post-education earnings premium. Where individuals and their families face high opportunity costs of funds - or a high discount rate - the accumulation of years of education becomes more difficult. It is likely that parents with higher productivity endowments are both less financially constrained and have higher preferences for education, and that some of their higher innate ability is passed on genetically to their children. Thus we would see children of higher natural ability also receive, on average, more years of education (see Harmon *et al* 2000: 6). A range of methodologies have been used to attempt to control for endogeneity by utilising some form of "quasi-experimental" variation in the years of schooling acquired that is imposed upon individuals. These include studies of twins, for whom "natural ability" is assumed to be constant for each pair, and instrumental variable techniques (see below and Appendix B).

A further concern is that in the formulation given in (1), δ represents the average effect of years of schooling and the effect is generally assumed to be linear. In reality the return to each additional year of education may decline as the total number of years of education increase, or the marginal returns may follow a quadratic function (initially increasing and then declining). This may be handled by the inclusion of quadratic or higher order terms among the explanatory variables. However, the returns to years of education may also vary systematically across individuals, perhaps with ability. This possibility is part of the impetus for this study – the hypothesis is that additional years of schooling may deliver benefits to those with higher ability, but much smaller benefits for those of lower ability. In this case education and ability would be said to be complementary, although the opposite could conceivably be true.

The use of earnings as the outcome variable presents an added complication in that observing earnings is conditional upon the individual being in work. Similarly, using the incidence of unemployment, as conventionally measured, observation of the outcome variable requires participation in the labour force. These conditions for inclusion in estimations introduce a further potential source of selection bias – those with lower potential earnings and a higher likelihood of unemployment can be expected to be less likely to be included. This is particularly problematic in the case of women, for whom labour force participation rates are significantly lower than men, and there is evidence of a downward bias in the estimated returns to additional years of education for women (Harmon *et al* 2000: 11-13). For the purposes of estimation, this means that the analyst needs ideally to control for selection into employment or the labour force as well as endogeneity of the years of schooling. Das, Whitney and Vella (2003) have recently proposed non-parametric estimators that can allow for multiple selection rules as well as endogeneity of regressors.

Empirical estimates

Empirical studies across a very large range of countries, samples and utilising many different methodologies have confirmed a positive wage premium from additional years of education. The vast bulk of estimates fall within the interval of between 5 to 10 percent higher wages for each additional year of education completed. From the preceding discussion, an important aspect of the methodology of any study is how the analysis controls for endogeneity between years of education and ability in particular, but also between years of education and other omitted variables more generally.

Chiswick, Lee and Miller (2003) use data from the 1996 Australian Aspects of Literacy Survey to assess the relative contributions to years of education and “ability”, as measured by a number of different tests of literacy and numeracy, upon unemployment and participation rates. As the data is cross-sectional, measured ability will be affected both by innate ability and by participation in education. They find that both education and ability contribute to improved labour market outcomes, and that the literacy and numeracy skills account for about half of the total effect. Thus years of education have a significant “indirect” effect through contributing to higher literacy and numeracy skills, but also a roughly equal “direct” effect in addition to this.

Importantly for this study, Chiswick *et al* (2003) find that only a limited number of variables on literacy and numeracy are needed to capture the effects, and both measured test scores and self-assessment perform well. However, the data used by Chiswick *et al* (2003) did not allow the authors to address potential endogeneity between ability and years of education. In contrast, Harmon *et al* (2003) use data from the Great Britain Child Development Survey which contains scores from various ability tests taken at ages 7, 11 and 16 as well as data on later earnings. Estimating regressions both with and without controls for ability shows that the estimated return to education is lower when ability measures are included. The apparent ability bias is small when controls for ability at age 7 are included, but as much as 4 to 5 percentage points using controls from later ages. The authors argue this is due to the effects of measured ability and education being jointly determined later in life, and prefer the ability measures from age 7 as the better control for ability. Thus they conclude the ability bias is small and that years of education provide only a limited degree of “signalling” (Harmon *et al* 20003: 20-21).

Data on sets of identical twins allow controls for both ability and family background. Table 2 shows results from studies utilising data on identical twins. Using data from the Australian Twins Register, Miller, Mulvey and Martin (1995) estimate an average earnings premium to additional years of education of around 5 to 7 percent. Using data from the same Australian survey, Lee (2000) estimates the return to be 8.9 percent, and finds that measurement errors in schooling data are actually a more serious source of bias than omitted variables on family background.

Table 2 Estimates of the returns to schooling – results from twins studies (percent effect on wages)

Author	Data	OLS %	IV %
Ashenfelter and Rouse (1998)	Princeton Twins Survey	7.8	10
Rouse (1999)	Princeton Twins Survey	7.5	11
Miller *et al* (1995)	Australian Twins Register	4.5	7.4
Isaccson (1999)	Swedish same sex twins	4.0	5.4
Ashenfelter and Zimmerman (1997)	NLS Young Men	4.9	10
Bonjour, Haskel and Hawkes (2000)	St Thomas' Twins Research Unit girls	6.2	7.2

Source: Harmon *et al* (2000), Table 4.5.

Ryan (2003: 9-10), Card (2001) and Harmon *et al* (2000: 25-31) provide reviews of studies which use instrumental variables (IV) or "quasi-experiments" to control for ability and other omitted variables. The method of instrumental variables relies upon identifying some observable variable which is correlated with years of schooling, but uncorrelated with ability. This means that there will be some variation in years of schooling which is not dependent upon ability and which can be used to identify the effect of additional years of schooling independent of ability. IV is in fact a special case of two-stage least squares. An example is if one had data on the distance individuals lived from school or college. Since geographical location can be assumed to be independent of ability, the variation in schooling which can be identified as being due to geographical location can be used as an exogenous regressor to identify the impact of schooling upon earnings.

In the vast bulk of studies, the application of IV leads to *increased* estimates of the return to schooling, typically by around 20 percent (see also Table 2 above). Reasons given for this are that instrumental variable methods also pick up measurement error, or the instrument disproportionately affects individuals with a high marginal rate of return to schooling. Take, for example, a policy difference between states where in some states children complete 12 years of schooling and in other states 13 years. Using the policy differences as an instrument would give the analyst exogenous variation in schooling, but only for those who remained in school to the final years. The effect cannot be determined for those in both states who drop out of school earlier and thus who probably face lower returns to education.

This study concentrates on the effect of participating in additional years of secondary schooling on earnings and employment outcomes. While it is the relationship between schooling and earnings that has been most extensively analysed, studies of employment status also tend to find a beneficial effect of further years of schooling (see Ryan 2003: 6-7). Studies based upon LSAY data, including Marks *et al* (2003) and McMillan and Marks (2003), paint a less clear picture. In the early post-school years, whether or not employment outcomes for school completers are superior to those for non-completers seems to be highly dependent upon the activities the non-completers engage in upon leaving school. There is evidence that time in full-time work offers greater benefits in these early years than further time in school. However, many of the econometric issues discussed above in the case of wages also apply in estimating the effect of schooling upon employment opportunity and have not been addressed in these studies. Ryan (2003) exploits a policy change implemented in South Australia in 1985 as a source of "experimental" variation in the years of schooling accrued for two cohorts. The results suggest that, among those who did not go into full-time post-secondary studies, the additional year of junior schooling increased the probability of being in full-time employment by around 11 percent in the first year out of school. However, no significant effect was identified by the second year out of school.

It must be noted that a range of other factors associated with schooling and chosen pathways from school to work have received a considerable amount of attention with respect to how they influence earnings and other outcomes later in life, and are not considered in this study. These include aspects of the quality of schooling, such as class sizes or pupil-to-teacher ratios, the economic resources available to the youth's family, participation in work experience, peer effects (people from high achieving groups increasing their own performance), neighbourhood effects and the method used to find work.

4. THE LONGITUDINAL SURVEYS OF AUSTRALIAN YOUTH

This report uses data from the 1995 Year 9 cohort of the LSAY. The LSAY comprises a series of panel surveys of young Australians aimed at collecting information on the transition from school to work. Detailed background and technical information on these and associated surveys, the Australian Youth Survey and the Youth in Transition Surveys, can be found in a series of information papers from the Australian Council for Educational Research (ACER). The data used in this study come from a panel survey of youth who were first surveyed as Year 9 students in 1995. The vast bulk of the sample turned either 14 or 15 in that year, depending mainly upon the organisation of school years by age in their state of residence. The sample was selected by a two-stage process in which a random sample of schools was selected, and then a random sample of Year 9 classes from within those schools. The data come from self-completed questionnaires administered in 1995 and 1996 and telephone interviews conducted in each year from 1997 to 2002. A total of 13,613 valid returns were gained from those completing the initial survey in the first year. The attrition rate over the eight waves to date stands at 45 percent and 5,368 individuals participated in all six surveys.

A picture of the cohort's transition from school to work, at a very general level, is presented in Table 3. By definition, all of those in the sample were at school in 1995. By 1999 almost all (96.7%) had left school, with roughly equal proportions in work and post-secondary education. By 2002 work was the main activity for two-thirds of the cohort.

In assessing the benefits derived from completing further years of education, outcomes in terms of the incidence of unemployment as well as the usual measure of wages are investigated. Table 4 presents a descriptive overview of these outcomes conditional upon the level of secondary school undertaken. We distinguish between those who did not go on beyond Year 10 at secondary school, those who did so but did not complete Year 12, and those who did complete Year 12 (or 13). The first row of Table 4 presents the participation rate. The unemployment rate is calculated under the standard definition of the number of unemployed persons (those looking for work and ready to commence work) as a percentage of those participating in the labour force (either working or unemployed).

Two measures of earnings are presented. In the LSAY, individuals are asked how many hours they normally work in a week, how they are paid (eg. hourly, weekly, fortnightly, monthly) and the usual gross pay. From this series of questions hourly wages (before tax) can be calculated. However, this results in quite low wage rates being recorded for many full-time workers who report working a large number of hours per week. For example, around 13 percent of workers report normally working 45 hours or more per week and 6 percent report working 50 hours or more. Thus, in addition to the hourly rate, weekly earnings for all full-time workers (defined as those working 35 hours or more per week) are also calculated, making no adjustment for the number of hours worked.

Table 3 The transition from school to work: current activity by survey year, LSAY Year 9 1995 cohort

	1996[a] (Year 10)	**1997 (Year 11)**	**1998 (Year 12)**	**1999 (Aged 18)**	**2000 (Aged 19)**	**2001 (Aged 20)**	**2002 (Aged 21)**
Still at school (%)	**94.9**	**86.1**	**79.4**	**3.8**	**0.4**	**0.0**	**0.0**
Has left school and main current activity is (%):							
Working	1.8	9.3	14.7	43.6	46.6	56.9	63.5
Doing an apprenticeship	*0.5*	*2.8*	*2.7*	*6.9*	*8.2*	*6.8*	*2.2*
Doing a traineeship	*0.1*	*1.1*	*1.7*	*4.9*	*4.0*	*2.3*	*1.7*
Study	0.3	1.8	1.1	44.2	40.5	33.0	25.1
Work and study[b]	0.0	0.2	0.1	1.4	5.8	2.5	2.3
Looking for work	0.7	2.0	3.6	4.9	4.4	4.3	4.6
Other	0.2	0.6	0.9	1.4	2.3	2.9	3.9
Missing	2.0	0.1	0.3	0.7	0.1	0.4	0.5
Total Left School (%)	**5.1**	**13.9**	**20.6**	**96.2**	**99.6**	**100.0**	**100.0**
Total (%)	**100.0**	**100.0**	**100.0**	**100.0**	**100.0**	**100.0**	**100.0**
Sample number	9837	10307	9738	8783	7889	6876	6095
Sample survival rate	72.3%	75.7%	71.5%	64.5%	58.0%	50.5%	44.8%

Notes: a. data for 1996 relate to main activity since leaving school and is not strictly comparable to other years.
b. were working and studying, but doing neither full-time.

Table 4 2002 Labour market outcomes by year of secondary school completed

	Completed Yr 10 or less	Beyond Yr 10 but did not complete Yr 12	Completed Yr 12[a]	All
Participation Rate	92.0%	81.7%	86.1%	85.7%
Unemployment Rate	7.3%	10.1%	6.9%	7.3%
Wages (means)				
- Hourly rate (all workers)	$15.33	$14.93	$15.10	$15.09
- Weekly wage (FT workers)	$667.41	$604.54	$588.60	$595.37
Observations	237	737	4975	5949

Note: a. Includes those who went on to Year 13.

On these simple comparisons, those who completed Year 12 do not fare uniformly better than those who left school earlier. It is those who went beyond Year 10 secondary school but failed to complete Year 12 who have the lowest participation rates and the highest incidence of unemployment of those in the labour force. Those who completed Year 12 have lower labour force participation than those who did not go beyond Year 10, but this is due to their higher participation in full-time study. They also have the lowest incidence of unemployment. The young people who left at the end of Year 10 or earlier are also observed to have the highest earnings, but of course these figures do not take into account the additional years of experience in the workforce that these workers will have had, particularly relative to those who completed Year 12 and went on to further education.

5. DETERMINANTS OF SCHOOL RETENTION

As discussed, the hypothesis to be tested requires differentiating those who are "less" academically inclined from those who are "more suited" to further schooling and education. To give these notions a more solid empirical foundation, a model of the probability of an individual completing the final year of secondary school is estimated. Previous research utilising the LSAY data has identified better numeracy and literacy levels in early school, having parents from higher socioeconomic backgrounds (eg. professional as opposed to unskilled manual), attending private schools as opposed to government schools, living in a metropolitan area and being from a non-English speaking language background as factors associated with higher rates of school completion, while Indigenous youth have markedly lower rates of retention to Year 12 (see Fullarton, Walker, Ainley and Hillman 2003; Marks, Fleming, Long and McMillan 2000; and Lamb 1997).

Two criteria are utilised to distinguish between school leavers who completed the final year of secondary school and those who did not. Firstly, those who left school in either Year 12 or 13 during the months of October through to December were classified as having completed school. Those who left in a prior year or before October were classed as not having completed school. The second criteria is based on a question from the 1999 survey which directly asked persons who left school since the last interview whether or not they gained their leaving certificate (1998 is the year the cohort was due to graduate from high school). To the extent of any inconsistency, the definition based on the direct questions takes precedence.[3]

The model is fitted using a logit model, which estimates the effect of different individual characteristics (independent variables) on the probability of that person completing Year 12. The model is estimated across the full sample for whom there is data available, including those who go on to enter university. The estimated coefficient for each independent variable therefore represents the average effect for the cohort of that characteristic on the likelihood of an individual completing school. The results are reported in Table 5. A positive coefficient ($\hat{\beta}$) indicates that having larger values of that variable increases the probability of an individual completing school.[4] For dummy variables (which take on a value of one or zero), a positive coefficient indicates that having that characteristic is associated with a greater likelihood of completing school. The coefficients can then be used to calculate a predicted likelihood of each person finishing high school based upon their particular set of characteristics.

Among the explanatory variables are included several measures of ability. These include scores from standardised reading and mathematics tests administered with the first survey as Year 9s (mostly aged 14 or 15). Also in that survey respondents were asked to provide self-assessments of how they were doing in English, maths and in their school subjects overall using a five-point scale ranging from very poorly, not very well, about average,

[3] The data on completion of the leaving certificate is not provided in the public use file, but was made available separately by ACER. Discussions with researchers at ACER also confirmed that having left in October or later in the final year of secondary school is their preferred definition for completion of secondary school.

[4] Technical details of the logistic regression model are provided in Appendix A.

better than average and very well. Each of these measures is positively and significantly related to the likelihood of going on to complete high school. Dummy variables derived from the 1995 survey are also included to indicate whether the person planned to leave school at the end of Year 12; whether they planned to go on to university and whether their parents wanted them to study full-time after leaving school. As would be anticipated, a positive response to each of these indicators is associated with a greater likelihood of completing Year 12.

Other results are in close concordance with those from previous LSAY studies. Persons attending school in metropolitan areas are more likely to complete high school than those from smaller regional centres (defined as having populations between 1,000 and 100,000 people) or from more remote areas. Family background also appears to be important. Note in particular the large negative effect of being of Aboriginal or Torres Strait Islander descent. Youth whose father is of higher occupational status are significantly more likely to finish high school, as are those from a non-English speaking background. One explanation for this latter effect is that increases in the proportion of migrants entering Australia under the skilled migration category has resulted in a pool of recent settlers who have relatively strong preferences for higher educational attainment for their children (see Marks *et al* 2000: 25; Miller and Neo 2003: 339).

No direct measure of wealth or family income is available; however, in 1996 students were asked whether there was a range of consumer goods in their home, such as a washing machine, microwave, computer and so on, as well as whether or not they had a swimming pool. Based on the number of these assets present in the home a “wealth” index is constructed where items that were less commonly present were given a higher weighting. The index can range from zero if none of the items were present to 100 if all were present in the home. This proxy for wealth is also associated with higher school retention rates. A more direct “family resource” effect is captured by the response to a question on why individuals were working in Year 9. Those who indicated that they did so because the family needed the money were considerably less likely to complete school. Although this variable is not significant at the normal levels, it is retained in the model due to the magnitude of the estimated effect.

In the 1995 and 1997 surveys sets of questions were asked from which may be gained some insight into individuals’ attitudes toward school and their personality traits. In the initial survey, the Year 9s were asked whether they agreed or disagreed with each of 30 statements relating to school. In 1997 they were asked to rate themselves on a set of 8 different personality characteristics, such as how popular or outgoing they were, how open to new experiences and so on.

A principal components analysis was carried out to identify linear combinations of the responses (factors) that could be used to summarise the patterns of responses between individuals. This allows the inclusion of a small number of standardised factor scores to be included as explanatory variables in the model in place of an otherwise unwieldy array of highly correlated, potential independent variables. As always with factor analysis the choice of how many factors to retain is somewhat arbitrary (see Kline 1994). Based on the proportion of variance explained by each factor and how meaningful the combination of highly weighted questions appeared, scores are calculated for five factors relating to attitudes towards school and two relating to self-perceptions. The factor scores calculated are standardised to have a mean of zero for the sample population and a standard deviation of one. For the set of questions relating to the students' feelings about school, the five factors can be summarised as follows:

- Enjoys schoolwork – this factor is most strongly correlated with (or "loads on") agreement with statements such as "learning is a lot of fun", "I get excited about the work that we do", "I like to do extra work", "I like learning" and so on.
- Getting on well with teachers – this factor loads most heavily on the statements "teachers are fair and just", "teachers treat me fairly in class", "teachers listen to what I say" and "teachers help me to do my best".
- Being a successful student - correlates with agreement on "I always achieve a satisfactory standard in my work", "I am a success as a student", "I know how to cope with the work" and "I know I can do well enough to be successful".
- Valuing what is learnt in school – loads most heavily on statements such as "the things I learn will help me in my adult life", "the work I do is good preparation for my future" and "I have acquired skills that will be of use to me when I leave school".
- Feeling happy and secure at school – correlates with the statements "I feel happy", "I feel safe and secure" and "I get enjoyment from being here".

With respect to the individuals' perceptions of their "self" as given in 1997, two dominant factors can be identified:

- extrovert - strongly correlated with seeing themselves as being outgoing, confident, popular and open to new experiences.
- easygoing - seeing themselves as being calm, agreeable and hardworking.

By construction, the mean for the population as a whole for each of the factor scores is zero, with a standard deviation of 1. Two of the factor scores are significant in the regression analysis of the probability of completing high school. These are the factor scores for finding school a happy and safe place and the factor score for the "extrovert" personality trait. As would be anticipated, a positive sentiment towards school is associated with higher school retention, while being more extroverted is associated with a likelihood of leaving school before completing Year 12.

From the perspective of modelling the effects of further years of education on labour market outcomes, it is encouraging that the data contain robust controls for ability, plus a number of potential "instruments" which are robust in determining school retention but which may not otherwise be expected to be associated with labour market outcomes after controlling for ability. These include parental and individual expectations of future schooling, attitudes towards school, financial constraints on schooling and region.

Table 5 Logistic regression model of probability of completing secondary school

Independent Variables [range]	$\widehat{\beta}$		**Prob >t-stat**
Intercept	-2.7724	***	0.000
Male	-0.4682	***	0.000
Reading achievement score (1995) [0-20]	0.0399	***	0.004
Maths achievement score (1995) [0-20]	0.0726	***	0.000
Self assessed ability - English (1995) [1-5]	0.1422	**	0.026
Self assessed ability – Maths (1995) [1-5]	0.1942	***	0.000
Self assessed ability - Overall (1995) [1-5]	0.2339	***	0.002
Location of residence (1995)			
— Metropolitan	—		
— Major regional centre	-0.2993	***	0.003
— Rural and remote	-0.2454	**	0.024
Wealth Index (weighted) (1996) [0-100]	0.0055	***	0.001
Worked because family needed money (1995)	-0.3583		0.172
Plans to finish Year 12 (1995)	0.7893	***	0.000
Parents want me to study FT after school (1995)	0.1474		0.224
Plans to go to university (1995)	0.8024	***	0.000
Aboriginal or Torres Strait Islander	-1.0768	***	0.000
Father's occ manager/prof/para-prof	0.4295	***	0.000
Living Status (1997):			
— Lived with two parents	—		
— Lived in sole-parent home	-0.2789	**	0.026
— Had left home	-1.4311	***	0.000
English not 1st language at home (1995)	0.5693	***	0.003
Factors - attitudes towards school (1995)			
— finds school a happy and safe place	0.1041	**	0.020
Factors – view of self (1997)			
— extrovert	-0.2653	***	0.000
Observations	5040		
Degrees of freedom	20		
Likelihood ratio	1019.2	***	0.000
Score	1010.0	***	0.000
Wald	737.2	***	0.000
Concordant (percent)	81.3		
Discordant (percent)	18.4		

Notes: ***, **, and * denote that the estimate is significantly different from zero at the 1 percent, 5 percent and 10 percent level, respectively.

Factor scores are calculated to have a mean of zero and standard deviation of one for the sample overall.

Other variables are dummy (binary) variables unless their range is given in square brackets.

6. THE EFFECT OF YEARS OF SCHOOLING ON EARNINGS

A standard model

Earnings

A standard earnings function would incorporate among the explanatory variables gender, age, marital status and human capital variables. Measures for human capital may include years of education or qualifications, labour market experience and possibly health status. For the purposes here, it would be ideal to include a variable on years of education, however it is quite difficult to extract the precise number of years of post-school education using the LSAY data. There are several ways of doing so. One is to use the questions from each year of the survey to identify the courses undertaken and their commencement and completion dates. However, the accuracy and coverage of data derived from these responses is questionable, leading to the inclusion of an additional module of questions in the 2001 survey to record all episodes of post-school education and training (up to a maximum of 4 episodes). A further problem relates to the difficulty in knowing whether study has been continuous and in what proportion it may have been full-time or part-time.

Second, the additional questions from the 2001 survey can be used. However, the problem of discontinuity and "intensity" of study remains, with additional scope for recall error. The far simpler approach is to identify the highest qualification attained and ascribe to this qualification a typical "full-time equivalent" period of post-school education. Important limitations to this approach are the neglect of any years of education undertaken but which did not lead to a qualification, such as if the student failed to complete the course, and the neglect of years of education undertaken in courses other than those leading to the person's highest qualification. However, in light of the limitations involved under the alternative approaches, this appears the best way to proceed.

As a starting point, parsimonious wage equations for the hourly wage for all workers and the weekly wage for full-time workers are estimated. Only jobs observed for persons who have left school are included.[5] Further, earnings can be observed only for persons who are in employment, but no attempt is made in the analyses to adjust for potential bias arising from this sample selection process. Earnings observed in each wave of the survey from 1997 to 2002 are converted to real 1997 dollars and the observations pooled, such that an individual may contribute up to six observations.[6] The standard approach of estimating an ordinary least squares regression using the log of the wage as the dependent variable is followed, with the exception that the estimation procedure takes into account the fact that we have repeated observations for each individual.[7] In models 6.1 and 6.3 of

5 Some outliers have been removed from the data used in the estimation. Also removed were cases in which weekly earnings were reported as $999. Clustering at this value suggests that some missing values may have been erroneously coded as 999, instead of the actual missing value code of 9999.

6 More specifically, the CPI in the December quarter of each year is used to index earnings to the December 1997 quarter. The value of the CPI in December 1997 was 120.0, and the numerators to give deflators for 1998 through to 2002 are 121.9, 124.1, 131.3, 135.4 and 139.5, respectively (ABS 2004).

7 More precisely the MIXED procedure in SAS, which allows for correlations between the error terms for observations on the same individual, is used with the compound symmetry option for the structure of the covariance matrix of the error terms. See Appendix A for an analogous discussion with respect to the correlation between error terms for repeat observations on individuals for the models of employment outcomes.

Table 6 variables for years of education, work experience and age are included along with gender, marital status and several indicators of socio-economic background. For hourly earnings (model 6.1) a dummy variable indicating whether the job is part-time is included, but this is not relevant for the model for full-time earnings (6.3). A variable indicating whether or not the person has completed an apprenticeship is also included as an additional control for human capital (time in an apprenticeship is modelled here as work experience but not as time in post-school education).

The results for the estimated coefficients generally conform to expectations. Real wages increase by around 10 percent per year as the cohort ages. They also increase with each year of work experience, but at a declining rate as evidenced by the negative coefficient on the term for experience squared. There is however a limitation with the measure of work experience in that it is not possible to fully distinguish between full-time and part-time work experience. There is a wage premium of 2 to 4 percent for married persons. Males also receive a positive wage premium, particularly among persons in full-time work, and persons working in cities receive slightly higher wages. The effects observed for these variables are well established in the literature.

In other results, family background again has a significant influence. Young people who reported in 1995 that their father had a white-collar occupation (technical, para-professional, professional or managerial) or who came from wealthier households appeared to secure higher paying jobs. Initially three variables were included to capture family structure based on survey data in 1997: those living in two-parent families, those living in sole parent families and those who had left home. No significant difference in earnings is observed between those who had lived in two-parent and sole-parent families at age 16, thus only the remaining category of "left home" is retained. Although this variable does not quite attain significance at the 10 percent level, its estimated effect is consistently negative and of some magnitude in the models for hourly earnings, implying a penalty of around 3 percent for young people who leave home at a young age. It is likely that this reflects, in part, broader circumstances associated with youth experiencing difficulties in their home lives.

Perhaps surprisingly, persons from non-English speaking backgrounds also display higher earnings. Having completed an apprenticeship increases earnings by around 4 to 5 percent. Hourly earnings for those in part-time work are estimated to be 13 to 14 percent higher than for those working full-time. This is consistent with casuals in Australia receiving higher wages in lieu of benefits such as sick leave, annual leave and superannuation, but also with what appears to be the case of a significant proportion of full-time workers working additional hours for which they do not get paid.

The results in models 6.1 and 6.3 suggest that each additional year of education provides the young person with a positive but small wage premium of the order of 1 percent. This effect is highly significant for both hourly and weekly full-time earnings. In models 6.2 and 6.4 the available controls for ability are added. The ability variables are obviously highly correlated with one another and the "general to specific" procedure is followed whereby the least significant measures are progressively removed and only significant measures retained. Their inclusion does indeed diminish the estimated effect of years of education — by around one-third in each case — however it has little effect on the other variables. For the model of hourly earnings, the coefficient reduces from an implied 0.9 percent increase in earnings with each prior year of education accumulated to 0.6 percent with each year, now significant only at the 5 percent level. In the model for weekly full-time earnings the estimated effect of years of education reduces from a 1.4 percent to a 1 percent wage premium, also significant at the 5 percent level.

Table 6 Wage equations: hourly earnings and weekly FT earnings, 1997-2002

Variable	Hourly Earnings				Weekly FT Earnings			
	Basic Equation (6.1)		With controls for ability (6.2)		Basic Equation (6.3)		With controls for ability (6.4)	
	$\hat{\beta}$	Pr > |t|	$\hat{\beta}$	Pr > |t|	$\hat{\beta}$	Pr > |t|	$\hat{\beta}$	Pr > |t|
Intercept	0.0412	0.475	-0.0115	0.847	3.2007	0.000	3.1284	0.000
Male	0.0244	0.000	0.0220	0.002	0.0595	0.000	0.0582	0.000
Married	0.0408	0.001	0.0432	0.001	0.0267	0.060	0.0230	0.106
Cohort age (yrs)	0.1024	0.000	0.1007	0.000	0.1219	0.000	0.1217	0.000
Lived (1995)								
— Metropolitan	—		—		—		—	
— Major regional centre	-0.0202	0.015	-0.0178	0.034	-0.0043	0.709	-0.0012	0.916
— Rural and remote	-0.0366	0.000	-0.0348	0.000	-0.0199	0.102	-0.0191	0.118
Non-Eng sp. background	0.0288	0.039	0.0337	0.017	0.0561	0.011	0.0672	0.003
Wealth Index [0-100]	0.0003	0.048	0.0002	0.082	0.0002	0.173	0.0002	0.216
Father professional (95)	0.0361	0.000	0.0316	0.000	0.0362	0.001	0.0326	0.002
Had left home (97)	-0.0331	0.171	-0.0324	0.186	-0.0169	0.590	-0.0179	0.570
Years of education	0.0086	0.005	0.0062	0.048	0.0144	0.001	0.0098	0.025
Completed apprenticeship	0.0361	0.079	0.0405	0.050	0.0477	0.021	0.0497	0.016
Yrs of work experience	0.0579	0.000	0.0597	0.000	0.0997	0.000	0.1019	0.000
Experience squared	-0.0060	0.000	-0.0061	0.000	-0.0103	0.000	-0.0106	0.000
Job is part-time	0.1394	0.000	0.1330	0.000				
1995 test scores [0-20]								
— reading			0.0021	0.079			0.0050	0.001
— maths			0.0025	0.047				
Self-assessment (95) [1-5]								
— English ability			0.0069	0.115				
— maths ability			0.0084	0.038			0.0188	0.000
Subjects	5685		5542		5686		5595	
Max obs/subject	6		6		6		6	
Observations used	15893		15516		7320		7219	
Fit statistics:								
Res Log Likelihood	-4818.4		-4730.4		-1376.7		-1336.1	
AIC	-4820.4		-4732.4		-1378.7		-1338.1	
SBC	-4827.1		-4739.0		-1385.3		-1344.7	
-2 Res Log Likelihood	9636.9		9460.8		2753.4		2672.2	
Likelihood Ratio Test	932.98	0.000	898.52	0.000	1090.43	0.000	1081.78	0.000

The more direct concern here is with the effect of additional years of schooling as opposed to years of education, which encompasses years of both schooling and post-school education. The two are of course inextricably linked — completion of high school is generally a prerequisite to go on to university, which around 30 to 40 percent of all young people now do (see Marks *et al* 2000: Table 5). As the LSAY data is available to 2002, the year most of the cohort turns 21, many of those who went on to higher education will only just have entered the labour market by the end of the panel. Estimating the effect of years of education will be confounded by the interactions between time spent in education and foregone work experience. To best capture the effect of the decision to stay on at school, which includes its role as a pathway to post-secondary education, the models in Table 6 are re-estimated in reduced form by replacing years of education with years of schooling.

The other results remain relatively unchanged and for brevity we report only the coefficients on years of schooling (Table 7). Using this specification the estimated effect of each additional year of schooling on hourly wages is to increase wages by 1.1 percent. However, this estimate is only weakly significantly different to zero and when controls for ability are included the estimate becomes very close to zero and insignificant. In the case of full-time weekly earnings, each additional year spent in school is estimated to actually reduce earnings by 1.4 percent when controls for ability are omitted (weakly significant), and 2.6 percent otherwise (highly significant). Both sets of results are consistent with joint positive associations between ability and earnings and between ability and accumulated schooling.

Table 7 Coefficient on years of schooling (reduced form wage equations)

	Hourly wage	Full-time wage
No controls for ability	0.0115	-0.0144
(Pr > |t|)	(0.092)	(0.080)
Controls for ability	-0.0006	-0.0258
(Pr > |t|)	(0.934)	(0.002)

Segmenting the sample

The hypothesis of major interest is that extra years of schooling may not offer the same "pay-off" for youth who are non-academically inclined as it does for those more suited to continuing in school and in further post-school education. To assess the pay-off for non-academically inclined youth wage equations are estimated for two sub-samples selected to represent a group who are "non-academically inclined". The first subgroup is defined using the model of the likelihood of completing school developed in Section 5. The sample for estimation is those whose predicted likelihood of completing school is 89.6 percent or less, representing the bottom 50 percent of people for whom there are observations on wages at some time between 1997 to 2002. The second sub-sample is the group of young people who did not go directly on to university or other post-secondary studies after leaving high school. More specifically, this group is defined as those whose main activity was not study in the year after they left school. They may or may not have attained their secondary school leaving certificate and may or may not go on to participate in post-school education in latter years. Thus the first of these samples comprises individuals who are predicted *ex-ante* to be less likely to go on to finish school, while the second comprises individuals who are observed *ex-post* not to have gone on to further education immediately following school.

Another way of stating the hypothesis is that the expected return to additional years of schooling is not constant across the sample, but increases with the ability or academic inclination of the individual. Thus a more direct way of testing this hypothesis is to include in the estimating equation variables for years of schooling, the propensity to complete school and an interaction term between the two.

The results from wage equations estimated in accordance with each of these tests of the hypothesis are presented in Table 8. Table 8(a) reports models of hourly earnings and Table 8(b) the models for usual weekly earnings reported by those in full-time work. As before the sample includes only observations on wages for those who have left school and some outliers have been removed. The set of explanatory variables tested is also now expanded to include the factor scores relating to the young peoples' attitudes towards school and self-perceptions of their personality traits not normally available in wage-equations (see Section 5). In the models in which the interaction terms are included (8a.4 and 8b.4), the ability variables are excluded as these effects will be largely captured in the estimated probability of completing school. Indeed, when the ability controls are included along with the predicted probability of completing school the signs on the estimated coefficients for some of the measures of ability become perverse.

To provide a benchmark, initial estimates for the full sample are presented showing no significant effect of additional years of schooling on hourly earnings (model 8a.1) and a wage penalty of 2 percent for each additional year of attendance at high school for weekly full-time earnings, significant at the 1 percent level (model 8b.1), consistent with the results reported in Table 7. There is little evidence that the effect of years of schooling on real earnings is significantly lower when the models are estimated among those less suited to further schooling. For hourly earnings, the estimate of the coefficient on earnings remains close to zero and insignificant for the sample of school leavers who did not go directly from school to further education (model 8a.3). Among the 50 percent of youth predicted as least likely to complete Year 12 the estimated coefficient on years of schooling is now negative but still fails to attain significance (model 8a.2). For real weekly earnings of full-time workers the estimated effect of additional years of schooling remains significant and relatively unchanged in model 8b.2, while the estimate becomes insignificant for those who did not go straight on to post-secondary education (model 8b.3).

The models in which an interaction term between the estimated probability of completing school and the actual number of years of schooling completed is included, models 8a.4 and 8b.4, show the strongest evidence of the return to additional years of schooling increasing with academic ability. In both models the interaction term dominates with regard to any positive effect of schooling and schooling propensity. Exactly what the coefficients imply in practical terms becomes difficult to gauge. To provide a simple example, note that the probability of finishing school has a theoretical range of 0 to 1, although the average student has roughly a 0.75 likelihood of completing school. The bulk of the values for years of schooling lie in the range from 10 to 12. Consider an average student with predicted probability of completing Year 12 of 75 percent. Using the coefficients from model 8b.4, the estimated effect of that individual remaining in school to complete Year 12 rather than leaving after Year 10 is to *decrease* their weekly earnings in full-time employment by 3.2 percent. Take, on the other hand, a star pupil who has a 100 percent predicted likelihood of completing Year 12. The modelled effect of that young person staying on to complete Year 12 rather than leaving after year 10 is to *increase* full-time earnings by 2.2 percent in the early years of their career.

While the focus of the analysis is on the effect of years of schooling, results for some other variables are of particular interest. The first is a counterintuitive result in which young people of Aboriginal or Torres Strait Islander descent appear to have higher hourly earnings among the "non-academically inclined" group. At around 7 percent, the premium is quite large and seems at odds with the well-established disadvantage faced by Indigenous Australians in the labour market. A possible explanation for this is that Aboriginal and Torres Strait Islander persons, when employed, are disproportionately employed in public sector jobs (see Altman and Taylor 1995).[8] In turn, young workers in the public sector may be less likely to work additional unpaid hours than those in the private sector. Second there is evidence of a persistent "scarring" effect for youth brought up in households that are financially constrained. Though limited effects of the "wealth" index are found, the individual working while at school in Year 9 and indicating that they do so because their family needs the money continues to be associated with lower earnings upon leaving school. Having completed an apprenticeship is associated with higher hourly earnings of between 3 to 6 percent. A similar positive premium is also identified for full-time weekly earnings, although the estimates are not significant in all models. Finally, persons who are more extroverted, as defined from the factor scores generated from responses in the 1997 survey, appear also to attract a small positive wage premium.

8 An alternative explanation, offered by an anonymous referee, is that the result arises due to differential sample attrition patterns among Indigenous respondents.

Table 8(a) Wage equations: Segmented samples and inter-action terms

(a) Real hourly earnings (1997 dollars)

Variable	Full Sample (8a.1) $\hat{\beta}$	Pr > |t|	Lowest 50% of predicted likelihood to complete school (8a.2) $\hat{\beta}$	Pr > |t|	School leavers not entering post-school education (8a.3) $\hat{\beta}$	Pr > |t|	Full sample, schooling/ability interaction terms (8a.4) $\hat{\beta}$	Pr > |t|
Intercept	0.0167	0.843	-0.1020	0.379	-0.0422	0.748	0.6375	0.017
Male	0.0208	0.004	0.0246	0.032	0.0196	0.137	0.0304	0.000
Married	0.0426	0.001	0.0343	0.069	0.0259	0.176	0.0456	0.002
Cohort age (yrs)	0.1026	0.000	0.1163	0.000	0.1011	0.000	0.1040	0.000
Lived (1995)								
— Metropolitan	—		—		—		—	
— Major regional centre	-0.0177	0.035			-0.0210	0.160	-0.0151	0.110
— Rural and remote	-0.0348	0.000	-0.0260	0.045	-0.0196	0.231	-0.0371	0.000
Non-Eng sp. background	0.0342	0.016					0.0267	0.088
Aboriginal/Torres SI.			0.0598	0.119	0.0785	0.114	0.0800	0.028
Worked (need money) (95)	-0.0341	0.159			-0.0470	0.255	-0.0329	0.218
Wealth Index [0-100]	0.0002	0.072						
Father professional (95)	0.0317	0.000	0.0336	0.011	0.0428	0.003	0.0273	0.001
Had left home (97)	-0.0345	0.162						
1995 test scores [0-20]								
— reading	0.0022	0.075	0.0050	0.002	0.0036	0.056		
— maths	0.0026	0.041						
Self assessment (95) [1-5]								
— English ability	0.0074	0.094			0.0149	0.072		
— maths ability	0.0088	0.031						
Factors (1997) - extrovert					0.0086	0.198	0.0103	0.009
Years of schooling	0.0005	0.942	-0.0110	0.232	0.0022	0.814	-0.0526	0.024
Completed apprenticeship	0.0355	0.088	0.0423	0.096	0.0545	0.046	0.0611	0.009
Yrs of work experience	0.0589	0.000	0.0796	0.000	0.0990	0.000	0.0568	0.000
Experience squared	-0.0060	0.000	-0.0095	0.000	-0.0093	0.000	-0.0061	0.000
Job is part-time	0.1329	0.000	0.1588	0.000	0.1738	0.000	0.1292	0.000
Prob(Finish school)							-0.8382	0.030
P(Finish Sch)*Yrs School							0.0830	0.014
Subjects	5542		2255		1786		4510	
Max obs/subject	6		6		6		6	
Observations used	15516		6393		5082		12631	
Fit statistics:								
Res Log Likelihood	-4733.3		-2031.8		-1461.4		-3803.2	
AIC	-4735.3		-2033.8		-1463.4		-3805.2	
SBC	-4741.9		-2039.5		-1468.9		-3811.7	
-2 Res Log Likelihood	9466.6		4063.6		2922.8		7606.5	
Likelihood Ratio Test	898.6	0.000	510.5	0.000	526.1	0.000	793.3	0.000

Note: Factor scores have a sample mean of 0 and standard deviation of 1.

Table 8(b) Wage equations: Segmented samples and inter-action terms

(b) Real weekly full-time earnings (1997 dollars)

Variable	Full Sample (8b.1) $\widehat{\beta}$	Full Sample (8b.1) Pr > |t|	Lowest 50% of predicted likelihood to complete school (8b.2) $\widehat{\beta}$	(8b.2) Pr > |t|	School leavers not entering post-school education (8b.3) $\widehat{\beta}$	(8b.3) Pr > |t|	Full sample, schooling/ability interaction terms (8b.4) $\widehat{\beta}$	(8b.4) Pr > |t|
Intercept	3.2907	0.000	3.3952	0.000	3.5148	0.000	4.1595	0.000
Male	0.0508	0.000	0.0620	0.000	0.0567	0.000	0.0714	0.000
Married	0.0213	0.139	0.0216	0.265	0.0290	0.142	0.0267	0.092
Cohort age (yrs)	0.1334	0.000	0.1296	0.000	0.1146	0.000	0.1326	0.000
Lived (1995)								
— Metropolitan								
— Rural and remote	-0.0227	0.047	-0.0287	0.065			-0.0208	0.103
Non-Eng sp. background	0.0717	0.002	0.0396	0.294			0.0598	0.016
Aboriginal/Torres SI.							0.0653	0.142
Worked (need money) (95)			-0.0223	0.581	-0.0912	0.049		
Father professional (95)	0.0340	0.001	0.0151	0.366	0.0408	0.015	0.0159	0.191
Living Status (1997):								
— home, two parents								
— home, sole parent					0.0362	0.125		
— had left home	-0.0440	0.180			-0.0152	0.726		
1995 test scores [0-20]								
— reading	0.0063	0.000	0.0073	0.000	0.0060	0.005		
Self assessment (95) [1-5]								
— maths ability	0.0214	0.000			0.0168	0.045		
Factors (1997):								
— extrovert	0.0130	0.010	0.0103	0.171	0.0122	0.112	0.0177	0.002
Years of schooling	-0.0225	0.008	-0.0240	0.024	-0.0138	0.209	-0.0973	0.001
Completed apprenticeship	0.0269	0.202	0.0294	0.231	0.0731	0.005	0.0418	0.070
Yrs of work experience	0.0935	0.000	0.1094	0.000	0.1075	0.000	0.0986	0.000
Experience squared	-0.0105	0.000	-0.0118	0.000	-0.0102	0.000	-0.0114	0.000
Prob(Finish school)							-1.0570	0.021
P(Finish Sch)*Yrs School							0.1081	0.007
Subjects	5444		2256		1762		4511	
Max obs/subject	6		6		6		6	
Observations used	7002		3927		3772		5753	
Fit statistics:								
Res Log Likelihood	-1246.6		-688.1		-673.7		-983.8	
AIC	-1248.6		-690.1		-675.7		-985.8	
SBC	-1255.2		-695.8		-681.2		-992.2	
-2 Res Log Likelihood	2493.3		1376.3		1347.5		1967.6	
Likelihood Ratio Test	1047.6	0.000	770.0	0.000	805.4	0.000	929.5	0.000

Note: Factor scores have a sample mean of 0 and standard deviation of 1.

Instrumental variables

As outlined in Section 3, the possible presence of unobservable characteristics that influence both wages and the likelihood of completing school means that estimates of the effect of years of schooling on wages may be biased. In other words, the number of years of schooling accumulated is endogenous rather than an exogenous regressor that can be used to explain earnings. An econometric method to deal with this problem of endogeneity is instrumental variables. This involves jointly estimating the factors that influence the level of schooling attained and the wage outcome. Provided that a source of variation in the level of schooling can be identified that is unrelated to wages, then in theory this can be used to gain unbiased estimates of the effect of schooling on wages. To use the example noted in Section 3, the distance individuals live from the nearest school may influence the level of schooling attained, but have no other effect on wages. Thus any variation in individuals' levels of schooling attributable to their location can be treated as exogenous. That is, distance from school can be used to "instrument" variation in years of schooling.

Instrumental variables models of wages are estimated across the full sample and the two samples restricted to represent non-academically inclined youth. Allowance is again made for the fact that the sample consists of repeated observations on individuals (see Appendix B for a more technical discussion of the model specification). The main factors that influence the level of schooling attained have already been identified in the estimation of the probability of completing high school (Section 5). As noted, these contain a number of variables which are likely to provide exogenous variation in the level of schooling acquired, including attitudinal variables, individual and parental plans, financial constraints and family structure.

The results from the instrumental variables models for hourly wages are presented in Table 9(a) and for full-time weekly earnings in Table 9(b). For both measures of wages the estimated effects of years of schooling are higher using instrumental variables than ordinary least squares, consistent with the previous literature.[9] Across the full sample, each additional year of schooling completed is estimated to increase earnings by around 5 percent and the coefficient is significantly different from zero at the 10 percent level (models 9a.1 and 9b.1). When the sample is restricted to those who are non-academically inclined, it can be seen that the estimated effect of years of schooling becomes much smaller and insignificant in the case of real hourly earnings. This also holds for full-time weekly earnings when the model is estimated across the half of the sample with the lowest likelihood of completing school. However for those who did not go directly into further full-time education after leaving school the estimated effect of additional years of schooling is actually enhanced to 6.2% (model 9b.3).

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[9] See the discussion in Section 2 as to why instrumental variables estimates tend to result in higher estimates.

Table 9(a) Instrumental variable estimations of wage equations

(a) Real hourly earnings (1997 dollars)

Variable	Full Sample (9a.1) $\hat{\beta}$	Full Sample (9a.1) Pr > |t|	Lowest 50% of predicted likelihood to complete school (9a.2) $\hat{\beta}$	Lowest 50% of predicted likelihood to complete school (9a.2) Pr > |t|	School leavers not entering post-school education (9a.3) $\hat{\beta}$	School leavers not entering post-school education (9a.3) Pr > |t|
Intercept	-0.3812	0.114	-0.2649	0.257	-0.2578	0.263
Male	0.0243	0.003	0.0278	0.014	0.0282	0.043
Married	0.0605	0.000	0.0636	0.003	0.0457	0.055
Cohort age (yrs)	0.0967	0.000	0.1086	0.000	0.1020	0.000
Lived (1995)						
— Metropolitan	—		—		—	
— Major regional centre	-0.0174	0.065	-0.0036	0.782	-0.0231	0.167
— Rural and remote	-0.0349	0.001	-0.0169	0.231	-0.0179	0.311
Non-Eng sp. background	0.0416	0.015	0.0339	0.280	0.0238	0.633
Aboriginal/Torres SI.	0.0875	0.030	0.0734	0.075	0.1384	0.022
Father professional	0.0300	0.000	0.0313	0.020	0.0504	0.001
1995 test scores [0-20]						
— maths	0.0042	0.001	0.0020	0.258	0.0005	0.842
Self assessment (95) [1-5]						
— English ability	0.0082	0.122	0.0032	0.717	0.0187	0.087
Factors (1997) - extrovert	0.0078	0.057	0.0128	0.046	0.0163	0.032
Years of schooling	0.0495	0.083	0.0190	0.555	0.0231	0.526
Completed apprenticeship	0.0534	0.134	0.0228	0.518	0.0474	0.306
Yrs of work experience	0.0536	0.000	0.0745	0.000	0.0967	0.000
Experience squared	-0.0053	0.001	-0.0084	0.000	-0.0103	0.000
Job is part-time	0.1193	0.000	0.1445	0.000	0.1691	0.000
Subjects	4540		2255		1424	
Observations	12723		6393		4093	
Degrees of Freedom	16		16		16	
R-Square	0.2078		0.2422		0.2851	
F Value	183.29	0.000	120.32	0.000	92.69	0.000

Notes: Factor scores have a sample mean of 0 and standard deviation of 1.

List of instruments in addition to regressors: Plans to complete school (95); parents want me to study FT after school (95); plan to go to uni (95); attitude towards school (95) - factor scores for valuing what is learnt and finding school a happy and safe place; family status (97) - lived in sole parent home, had left home, worked because family needed the money (95); reading achievement score (95).

Table 9(b) Instrumental variable estimations of wage equations

(b) Real weekly full-time earnings (1997 dollars)

Variable	Full Sample (9b.1) $\hat{\beta}$	Pr > |t|	Lowest 50% of predicted likelihood to complete school (9b.2) $\hat{\beta}$	Pr > |t|	School leavers not entering post-school education (9b.3) $\hat{\beta}$	Pr > |t|
Intercept	2.9431	0.000	3.3074	0.000	3.2085	0.000
Male	0.0723	0.000	0.0729	0.000	0.0800	0.000
Married	0.0610	0.001	0.0678	0.004	0.0682	0.009
Cohort age (yrs)	0.1084	0.000	0.1105	0.000	0.0864	0.000
Lived (1995)						
— Metropolitan	—		—		—	
— Major regional centre	-0.0076	0.589	-0.0082	0.633	-0.0153	0.457
— Rural and remote	-0.0239	0.095	-0.0260	0.129	-0.0048	0.820
Non-Eng sp. background	0.0835	0.002	0.0478	0.295	0.0099	0.846
Had disability (1995)	0.0644	0.159	0.0684	0.228	-0.0040	0.946
Aboriginal/Torres SI.	0.0730	0.064	0.0498	0.171	0.0488	0.398
Worked (need money) (95)	-0.0373	0.352	-0.0421	0.353	-0.1013	0.090
Father professional	0.0314	0.011	0.0243	0.158	0.0385	0.043
1995 test scores [0-20]						
— reading	0.0060	0.001	0.0067	0.001	0.0058	0.021
Self assessment (95) [1-5]						
— maths ability	0.0082	0.212	0.0049	0.571	0.0012	0.911
Factors (1997) - extrovert	0.0159	0.009	0.0135	0.093	0.0223	0.015
Years of schooling	0.0514	0.092	0.0133	0.673	0.0622	0.074
Completed apprenticeship	0.0436	0.243	0.0037	0.919	0.0840	0.092
Yrs of work experience	0.0937	0.000	0.1124	0.000	0.1184	0.000
Experience squared	-0.0094	0.000	-0.0115	0.000	-0.0107	0.000
Subjects	2666		1664		1198	
Observations	5753		3927		3073	
Degrees of Freedom	17		17		17	
R-Square	0.3082		0.3263		0.3083	
F Value	151.28	0.000	112.54	0.000	84.69	0.000

Notes: Factor scores have a sample mean of 0 and standard deviation of 1.

List of instruments in addition to regressors: Plans to complete school (95); parents want me to study FT after school (95); plan to go to uni (95); attitude towards school (95) - factor scores for valuing what is learnt and finding school a happy and safe place; family status (97) - had left home; maths achievement score (95); self-assessed ability (95) – English and overall; view of self (97) – factor score for easygoing.

The surprising result of higher wages received by Aboriginal or Torres Strait Islander youth persists in several of these models, including now with respect to full-time weekly wages for the full sample (model 9b.1). The positive association identified previously between earnings and the personality trait of extroversion is also robust to estimation by instrumental variables.

Propensity score matching

The motivation behind experimental research designs is that the problem of joint determination of outcomes and participation in the "treatment" is overcome through random assignment of individuals to treatment and non-treatment (control) groups. Ethical considerations clearly preclude the use of experimental evaluation methods in the assessment of the effect of years of schooling, as this would require excluding certain individuals who desired to do so from completing secondary school. Methods such as instrumental variables seek to utilise natural experiments or quasi-experimental situations where schooling levels differ due to factors known to be exogenous, such as the distance to the nearest school or legislative changes. Matching methods also attempt to mimic an experimental research design. For each individual within the sample who received the "treatment" the matching algorithm identifies the most similar individual who did not receive the treatment, and compares the mean difference in outcomes for the treated and the non-treated individuals. It seeks to compare outcomes for two samples of individuals who are as identical as possible in all other respects except for their treatment status.

Matching methods differ primarily in the method used to identify the "most similar" match within the sample. A range of approaches are available, and the most straightforward to employ here is propensity score matching which matches the individuals according to their score derived from a model of the individuals' propensity to receive the treatment. Such a propensity score is already available through the model estimated in Table 5 (see Appendix C for details). Applying propensity score matching to the evaluation question at hand has other advantages. First there is no need to impose any functional form in the estimation of the effect of the treatment, as must be done, for example, in estimating the standard wage equation. Second the method produces several different parameters that are of interest in the evaluation and which are straightforward to interpret.

The parameter that is usually of greatest interest in the evaluation is the average effect of the treatment on the treated. That is, the mean of the difference in outcomes for the treated relative to their untreated "match". However, of as much interest in our case is the average effect of the treatment on the untreated. That is, the average difference in the wages for individuals who did not receive the treatment and the wages of their nearest match who did so can be calculated. We are interested here in both the effect of additional years of schooling on those who have undertaken it, as well as what the effect would have been for those who have accumulated fewer years of schooling.

It no longer makes much sense to pool the data from different years as this would risk making comparisons between individuals' earnings in different years. Rather the earnings data is restricted to that from the most recent year's survey (2002). This is the preferred year as individuals' careers have had the most time to develop and the bulk of the cohort will have completed their initial participation in the post-school vocational education and training.

Table 10 Comparisons of wage outcomes for high school completers and non-completers by propensity score matching

(a) 2002 hourly wages

Outcome	Treated	Controls	Effect (Percent)[a]	Std Error of effect
Full Sample	(n=2320)	(n=336)		
Unmatched	$12.27	$12.05	1.83	
Matched:				
Ave. treatment effect on treated	$12.27	$12.30	-0.27	3.14
Ave. treatment effect on untreated[b]	$12.12	$12.05	0.58	3.08
Ave. treatment effect	-0.16	3.03	-0.16	3.03
Lowest 50% of predicted likelihood to complete school				
	(n=1037)	(n=291)		
Ave. treatment effect on treated	$12.02	$11.76	2.16	3.78
Ave. treatment effect on untreated[b]	$12.15	$11.97	1.51	3.90
School leavers not entering post-school education				
	(n=463)	(n=231)		
Ave. treatment effect on treated	$11.88	$12.64	-6.14**	2.90
Ave. treatment effect on untreated[b]	$11.85	$12.15	-2.54	3.12

(b) 2002 Full-time weekly earnings

Outcome	Treated	Controls	Effect (Percent)[a]	Std Error of effect
Full Sample	(n=1124)	(n=262)		
Unmatched	$490.02	$501.67	-2.35	
Matched:				
Ave. treatment effect on treated	$490.02	$507.36	-3.48*	2.12
Ave. treatment effect on untreated[b]	$490.46	$501.67	-2.26	3.35
Ave. treatment effect			-3.25	2.27
Lowest 50% of predicted likelihood to complete school				
	(n=479)	(n=215)		
Ave. treatment effect on treated	$485.65	$503.82	-3.67	3.77
Ave. treatment effect on untreated[b]	$497.85	$499.84	-0.40	3.94
School leavers not entering post-school education				
	(n=361)	(n=189)		
Ave. treatment effect on treated	$482.44	$549.93	-13.09***	3.15
Ave. treatment effect on untreated[b]	$476.11	$510.12	-6.90**	3.35

Notes: a. Calculated as percentage difference between the means for the treated and the controls.

***, **, and * denote that the mean difference is significantly different from zero at the 1 percent, 5 percent and 10 percent level, respectively.

b. In the output for the “average treatment effect on the untreated” statistic, the psmatch27 routine refers to those who did not receive the treatment as “treated” and those who did receive the treatment as “controls”. This makes sense in that the statistic is an estimate of the effect of “not receiving the treatment”. However, here the labels are reversed for simplicity of interpretation.

Taking the sample as a whole, there is no significant difference between the hourly earnings of those who had completed school and their matched counterparts who had not completed school. The matched comparison does show lower weekly earnings of 3.5 percent for full-time workers, a result that is weakly significant. The literal interpretation of this result is that those who did complete school and were working full-time in 2002 would have had higher earnings had they instead left school earlier. When the sample is restricted to those with the lowest predicted likelihood of completing school, the estimated differences in earnings between school-completers and their most similar counterparts who did not complete school are insignificant. Support for the hypothesis that those with lower academic inclination will receive lower benefits from staying on at school is found in the results for school leavers who did not go into further education after leaving school. For those within this group who did complete school, the average effect of doing so is estimated to be a decrease in hourly earnings of 6.1 percent (significant at the 5 percent level) and a very large decrease in full-time weekly earnings of 13.1 percent (highly significant).

The estimate of the average treatment effect on the untreated is negative in most cases but attains significance only in the case of full-time weekly earnings for the school leavers who did not go straight on to further education. This estimate implies that, by matching those who did not complete school to their most similar counterparts who did complete school, the group of people who did not complete school would in fact have suffered lower weekly full-time earnings of 6.9 percent had they instead stayed on to the end of Year 12. Again this is consistent with the hypothesis that those of lesser academic inclination will not benefit from remaining in school. However, the hypothesis might also be taken to suggest that the estimates of the treatment effect on the untreated would be lower (either a smaller positive or a larger negative estimate) and more robust than the corresponding estimates of the treatment effect on the treated. The results here do not conform to this expectation though, to repeat, these estimates are relatively imprecise in terms of their statistical significance.

7. THE EFFECT OF YEARS OF SCHOOLING UPON EMPLOYMENT STATUS

After wages, probably the most widely used gauge of labour market success between different groups is the unemployment rate. This is defined as the number of persons who are unemployed (actively looking for work and ready to commence work) as a percentage of the number participating in the labour force (either employed or unemployed). In this section we model the effect of years of schooling upon the likelihood of being employed, conditional upon participation in the labour force. This is quantitatively identical to modelling the probability of being unemployed, however, modelling the probability of employment retains consistency with previous results in that a positive coefficient on the years of schooling variable means that additional years of schooling have a beneficial effect.

As the outcome is measured by a binary variable, logit models of the probability of being employed are estimated conditional upon the number of years of schooling completed. Observations are pooled over the surveys for the years from 1997 to 2002. The estimation procedure utilises the longitudinal nature of the data by taking account of the potential additional correlations between repeat observations for each individual (see Appendix A). A similar approach to that used in estimating the impact of additional years of schooling on wages is followed. An initial model is estimated for the full sample of observations but without "ability" measures among the explanatory variables. Additional models are then estimated with the inclusion of variables relating to ability in Year 9; with the inclusion of an ability/schooling interaction term; and with the sample restricted to those defined as less academically inclined. Finally, propensity score matching is applied to further test the effect of completing school on the probability of being in work as opposed to being unemployed in 2002.

In the wage equations reported in Section 6 a variable for years of work experience was included (and its square). This is problematic in models of the probability of being in employment as there will be a high degree of endogeneity, or "reverse causality", between the measure of years of work experience and the dependent variable. That is, if an individual is observed to be in a job at a point in time then, all other things equal, they will have accumulated more work experience than had they not been observed to be in a job.[10] Thus years of work experience is not included among the explanatory variables. It could be argued, in any case, that this is the preferred specification since the potential to accumulate work experience is partly dependent upon how long one remains in school. Omission of the variable allows the effect of reduced potential for work experience to be reflected in the estimate of the effect of additional years of schooling.

The results for the models estimated over the full sample are reported in Table 11. When controls for ability are added (model 11.2), the score for the standardised mathematics test is the only additional variable to prove significant. As the estimates of the coefficients on the explanatory variables are consistent across the different models the full results for models estimated over the two sub-samples of less academically inclined youth are left to Table A1 of the Appendices. Tables 12 and 13 summarise the estimates of the effects of additional years of schooling for each model and present the

[10] To be precise, being observed to be in employment will increase the measure of accumulated work experience by the time spent in the current job since the previous observation on that individual.

corresponding simulated effects of changing the years of schooling for the "average" individual from the sample from 10 to 12 years.

Table 11 Probability of employment, conditional on participating in the labour force; logit estimates for the full sample

	Ability variables omitted (11.1)		Ability variables included (11.2)		With schooling/ability interaction terms (11.3)	
Variable	$\widehat{\beta}$	Pr > |t|	$\widehat{\beta}$	Pr > |t|	$\widehat{\beta}$	Pr > |t|
Intercept	1.1144	0.129	1.1916	0.108	1.2186	0.600
Male	-0.1730	0.008	-0.2000	0.002	-0.1075	0.138
Married	0.1357	0.350	0.1338	0.360	0.0788	0.613
Has Children	-0.9647	0.000	-0.8990	0.001	-0.8885	0.002
Cohort age (yrs)	0.1717	0.000	0.1693	0.000	0.1678	0.000
Lived (1995)						
— Metropolitan	—		—		—	
— Major regional centre	-0.0355	0.661	-0.0361	0.658	-0.0651	0.459
— Rural and remote	-0.1351	0.121	-0.1337	0.128	-0.1334	0.161
Non-Eng sp. background	-0.9069	0.000	-0.8737	0.000	-0.8968	0.000
Had disability (1995)	-0.3848	0.049	-0.3727	0.059	-0.4496	0.028
Aboriginal or Torres St. Islander	-0.4904	0.022	-0.4579	0.033	-0.4022	0.120
Wealth Index [0-100]	0.0066	0.000	0.0063	0.000	0.0063	0.000
Living Status (1997):						
— home, two parents						
- *at least one working*	—		—		—	
- *neither working*	-0.7175	0.000	-0.6796	0.000	-0.7818	0.000
— home, sole parent						
- *parent working*	-0.3200	0.001	-0.3113	0.002	-0.3952	0.000
- *parent not working*	-0.6492	0.000	-0.6228	0.000	-0.8172	0.000
— had left home	-0.6369	0.000	-0.6413	0.000	-0.5858	0.004
Factors (1997) - extrovert	0.1711	0.000	0.1827	0.000	0.1883	0.000
Factors- attitudes towards school (1995)						
— values what is taught	0.0894	0.014	0.0702	0.056	0.0890	0.037
— happy & safe place	0.0882	0.021	0.0899	0.018	0.1455	0.001
Years of schooling	-0.1747	0.007	-0.2126	0.001	-0.1809	0.372
Completed app'ship	0.7019	0.007	0.7178	0.006	0.5929	0.032
1995 test scores [0-20]						
— maths			0.0338	0.000		
Prob(Finish school)					1.2129	0.725
P(Finish Sch)*Yrs School					-0.0963	0.747
Subjects	8638		8638		8638	
Max obs per subject	6		6		6	
Observations used	17422		17246		15480	
Fit statistics:						
Deviance (value/DoF)	0.5759		0.5735		0.5558	
Pearson χ^2 (value/DoF)	1.0034		1.0064		1.0022	
Log Likelihood	-5011		-4940		-4296	

Note: Factor scores have a sample mean of 0 and standard deviation of 1.

Before turning to these effects, some other results deserve mention. Having children at this young age is estimated to have a very strong and negative effect upon employment prospects. Recall that the sample is already restricted, by definition, to those participating in the labour force. Participation models (not reported) confirm that having children already significantly reduces participation rates for females as expected, and the effect on employment may largely reflect difficulties facing young single mothers who do try to combine work with child rearing. There is evidence that living in a sole-parent household at around the age of 16 reduces future employment prospects. When a further distinction is made between households with no working parent and those with at least one parent in employment, it seems that the more important factor is the presence of at least one working parent in the household. Being from a two-parent family but with no parent working has a large negative effect on the probability of being in employment between the ages of 16 and 21 relative to being from a two-parent household with a working parent. The detrimental effect is as large as being from a sole-parent, workless household, and larger than the effect of being from a sole-parent household where that parent is in employment.[11] While this is suggestive of "intergenerational unemployment" or negative role model effects other explanations are possible, such as members of the household facing a common, depressed regional labour market. Those from non-English speaking backgrounds, of Aboriginal or Torres Strait Islander descent and those with disabilities also face significant barriers to employment.

The estimated effect of each additional year of schooling accumulated is actually to reduce the likelihood of being employed as opposed to unemployed. The estimated coefficient is around –0.2 and highly significant in three of the models (see Table 12). When the predicted likelihood of completing school and the interaction term between this likelihood and years of schooling are included, the estimate on years of schooling is similar in magnitude but now insignificant. The coefficient on the interaction term is close to zero and insignificant. All positive effects are concentrated on the variable for the predicted probability of completing school, which is also insignificant. The signs of these coefficients suggest that positive employment effects flow from individuals' pre-existing circumstances and attributes (attitudes and ability) rather than from participation in schooling. Statistically, however, the hypothesis that these variables have zero effect cannot be rejected at the normal levels of significance. The coefficient on years of schooling in the model estimated across those who left school and did not go straight on to further education is close to zero and insignificant (model A1.2, Tables 12 and A1).

To demonstrate the magnitude of these estimated effects of additional years of schooling, Table 12 presents the predicted unemployment rates for a young person who has completed Year 10 and a young person who has completed Year 12, with all other variables evaluated at their means. The results from the benchmark model (11.1) estimated over the full sample returns a predicted unemployment rate for this "average" individual from the sample of 8.5 percent. Additional years of schooling are estimated to have a detrimental and highly significant effect. The predicted unemployment rate for a youth who has completed Year 10 is 6.6 percent, as opposed to 9.0 percent if that youth

[11] This specification was in fact tested in the wage equations modelled in Sections 6 (Segmenting the Sample and Instrumental Variables). This further distinction went unreported, however, since in no cases were the coefficients significantly different for a two-parent household with no parent in work versus a two-parent household with at least one parent in work; or between a sole parent household in which the parent worked and a sole-parent household in which the parent did not work.

had instead completed high school, a differential of 2.5 percentage points (after allowing for rounding). When the ability variables are added to the model, the estimated "penalty" associated with staying on in school is larger at 3.0 percentage points, consistent with part of the estimated effect of schooling in the initial model reflecting unobserved ability.

Table 12 Predicted incidence of unemployment, conditional upon years of schooling

	Full sample, no ability variables (Model 11.1)	**Full sample, with ability variables (Model 11.2)**	**Lowest 50% of predicted likelihood to complete school (Model A1.1)**	**School leavers not entering post-school education (Model A1.2)**
Coeff on yrs of schooling	-0.1747	-0.2126	-0.2462	-0.0361
(Significance level)	(0.007)	(0.001)	(0.003)	(0.678)
Unemployment rates				
All variables at means	8.5%	8.4%	8.1%	7.5%
Completed Year 10	6.6%	6.2%	5.8%	7.2%
Completed Year 12	9.0%	9.1%	9.1%	7.7%
Difference (pct points)	-2.5	-3.0	-3.3	-0.5

The results from the restricted samples are mixed. For those with the lower predicted likelihood of completing school the estimated effect of additional years of schooling on employment opportunity is more robust. The predicted unemployment rate is 5.8 percent for the average youth from within this group who left school after Year 10, compared to 9.1 percent if that youth had stayed on to Year 12. This result is consistent with the hypothesis that the benefits of further years of schooling are lower for less academically inclined youth. However, the small and insignificant result for the sample of school leavers who did not go into further education in the year after leaving school is inconsistent with the hypothesis.

The picture is more complicated when interaction terms are added as it is desirable to present the predicted incidences contingent upon the likelihood of completing school as well as the actual years of schooling accumulated. For completeness, a set of such contingencies are presented in Table 13, though it must be remembered that the estimates of the coefficients on years of schooling, the predicted probability of completing school and the interaction term are very imprecise in statistical terms. Again the models predict those who complete Year 12 to have a higher incidence of unemployment by between 3.2 and 3.6 percentage points, an effect that is relatively constant across the range of pre-existing attributes (as measured by the probability of completing school). The positive effect of pre-existing attributes is estimated to be much larger for early school leavers than for those who complete Year 12.

Table 13 Predicted incidence of unemployment, conditional upon years of schooling and predicted likelihood of completing school[a]

	Predicted probability of completing school			
Years of schooling completed	0.5	0.75	Sample mean (0.82)	1.0
Completed Year 10	6.0%	5.7%	5.6%	5.4%
Sample mean (11.58 yrs)	8.4%	8.3%	8.3%	8.1%
Completed Year 12	9.2%	9.1%	9.1%	9.0%
Yr 12 vs. Yr 10 difference (percentage points)	-3.2	-3.4	-3.5	-3.6

Note: a. Based upon results from Model 11.3.

Employment status and propensity score matching

Finally, the propensity score matching technique discussed in Section 6 is applied to the 2002 data to test the effect of additional years of schooling on employment rates.[12] The results are presented in Table 14. Consistent with the results from the logit models the raw means for those who completed high school (the treated) and those who did not show that, in 2002, employment rates were marginally higher for those who did not complete school (the untreated). Also, the average estimated effect of completing Year 12 for those who did so (the average treatment effect on the treated) is negative. No other results attain statistical significance. Following the hypothesis that the benefits of additional years of schooling are concentrated among the most able, this effect would be expected to have been larger (a larger positive or smaller negative result) for the full sample than within the two restricted samples. One would similarly expect the average effect of the treatment on the treated to be larger than the average effect of the treatment on the untreated (the estimate of what the effect of completing school would have been for those who did not complete school). None of these tests are supported by the results, however, the uncertainty attached to the estimates means that little can be taken from the results of the matching exercise in this instance.

Table 14 Comparisons of employment outcomes for high school completers and non-completers by propensity score matching

Outcome	Treated (1)	Controls (2)	Difference (1)-(2)	Std Error of difference
Full Sample	(n=2689)	(n=419)		
Unmatched	0.931	0.936	-0.004	n.a.
Matched:				
Ave. treatment effect on treated	0.931	0.964	-0.032**	0.013
Ave. treatment effect on untreated[a]	0.943	0.936	0.007	0.020
Ave. treatment effect			-0.027**	0.011
Lowest 50% of predicted likelihood to complete school				
	(n=1192)	(n=361)		
Ave. treatment effect on treated	0.930	0.940	-0.010	0.018
Ave. treatment effect on untreated[a]	0.945	0.934	0.011	0.024
School leavers not entering post-school education				
	(n=505)	(n=290)		
Ave. treatment effect on treated	0.952	0.970	-0.018	0.018
Ave. treatment effect on untreated[a]	0.941	0.941	0.000	0.025

Notes ***, **, and * denote that the mean difference is significantly different from zero at the 1 percent, 5 percent and 10 percent level, respectively;
a. see notes to Table 10.

12 See Appendix C for a detailed discussion of the propensity score matching method used.

8. SUMMARY AND CONCLUSIONS

It is often argued that increasing the level of schooling accumulated by young people should be a policy objective, possibly through increased school retention rates or by raising the compulsory level of schooling. Sometimes this argument is grounded in theory, such as the increasing importance of education due to skill-biased technological change and other changes in the nature of the labour market. Most commonly, however, it is based on observations that those who complete high school fare better in the labour market than early school leavers. Indeed contributions in the recent Australian literature highlight inferior outcomes for those who leave school early compared to those who complete Year 12 and stress the importance of school retention.

This study is motivated by obvious question marks over the strength of those assumptions and the policy implications drawn from them. It seems dangerous to paint all young people with the same brush and surely there are some young people who simply are not well suited to the schooling environment, either in terms of their individual preferences or of the benefits they can expect to gain. To express this in more concrete terms, the concern is with the implicit assumption that because those who complete school achieve superior outcomes, *therefore* those who did not complete school would also have achieved better outcomes if they had instead stayed on at school. It is well established in the evaluation literature that such an assumption cannot be made. The observed effect of the treatment may not be due to the treatment *per se*, but rather to other characteristics associated with selection into the treatment. And even if the outcomes for the treated can be attributed to the treatment, the treatment may not have the same effect on the untreated were they to undergo it.

In the case of years of schooling (or years of education more generally) it is clear that individuals with "positive" labour market characteristics, most notably academic ability, accumulate more years of schooling. Thus part of the apparent return to schooling simply reflects the pre-existing attributes of those individuals who accumulate greater levels of education. Secondly, the returns or benefits from further years of education may not be homogenous across individuals but rather it is likely that they increase with academic ability.

In estimating the effect of years of education on earnings the economic literature has gone to great lengths in attempting to disentangle these various effects and to isolate the "true" marginal impact of years of education. This includes leading contributions in Australia by Chiswick *et al* (2003), Lee (2000) and Miller *et al* (1995). A broad consensus would be that each additional year of education increases earnings by between 5-10 percent. However, these studies mainly seek to identify either the average effect of years of education or the "effect of the treatment on the treated", not the effect for those who have received fewer years of education or are otherwise less inclined to participate in further schooling and education. Less effort appears to have been made in determining the effect of years of education on labour force status.

Due to the particular policy issue being addressed, the departure in this study is that it concentrates on the effects of further years of schooling, rather than of education more generally, and specifically seeks to assess the effects of further years of schooling for those who are less likely to receive them – the "less academically inclined". The effects upon earnings and employment status are estimated. Standard regression models are estimated across the full sample to provide "benchmark" estimates, including instrumental variables models in the case of wages, and these incorporate a range of controls for ability available in the LSAY. A number of techniques are then used to assess the effects for non-academically inclined youth. Interaction terms between years of schooling accumulated and the predicted propensity to complete school are included to test for heterogeneity in the effect of schooling. The regression models are estimated across sub-samples chosen to represent non-academically inclined youth under two different definitions. Finally matching methods are used to identify both the "average treatment effect on the treated" and the "average treatment effect on the untreated" for the full and restricted samples.

Two potential measures of earnings, hourly wages and full-time weekly wages, are available. The derivation of hourly wages introduces an additional source of error via the reported number of hours usually worked. Also, many full-time workers appear to work a large number of unpaid hours, reducing their estimated earnings when calculated on an hourly basis. It could be argued that either is the better measure of earnings. To ensure the results are robust to the different specifications, the analysis is repeated using both earnings measures.

As one means of identifying those who are most suited to staying on at school, a model of the probability of completing high school is estimated. It confirms the importance of family background and of future intentions in determining completion rates. Indigenous youth, those from sole parent homes and who had left home by age 16 are significantly less likely to complete high school. As anticipated, there are strong correlations between various measures of ability and school retention. Estimating typical OLS wage equations across the full sample, a small positive effect of additional years of schooling upon hourly wages between 1997 and 2002 is identified, and a small negative effect for weekly full-time earnings. Both results are weakly significant. The inclusion of controls for ability results in no significant effect on hourly earnings and a reduction in full-time weekly earnings of 2.6 percent for additional year of schooling. Estimation of the models on the 50 percent of the sample with the lowest predicted likelihood of completing school and the sample of those who do not go into further education after leaving school provides does not add much in the way of further evidence. The estimates across the smaller samples are insignificant with one exception, in which the estimate for full-time weekly earnings remains largely unchanged.

The inclusion of an interaction term between the individual's years of schooling and their predicted likelihood of completing school provides the strongest evidence of heterogeneity of the effect of schooling on wages. The positive effects are captured exclusively through the interaction term rather than the years of schooling itself, suggesting complementarity between schooling propensity, which relates largely to ability, and the returns to schooling.

Estimation by instrumental variables returns an estimate of the returns to additional years of schooling of around 5 percent. This is more in line with existing Australian estimates of the returns to additional years of education of between 5 to 9 percent (Miller *et al* 1995; Lee 2000). In three of the four models estimated over the sample of less academically inclined youth this estimate becomes insignificantly different to zero. However, in the case of full-time weekly earnings for school leavers not entering post-school education in the following year, the estimated premium actually increases to 6.5 percent.

Using propensity score matching, significant effects of completing Year 12 on hourly earnings in 2002 are identified only for the sample of school leavers who did not go straight on to further education, and the estimated effect is negative for both hourly and full-time weekly earnings. For full-time weekly earnings in 2002, the "average effect of the treatment on the untreated" is also negative for this group, but the implied wage penalty (6.9 percent) associated with completing school is much smaller than that implied for the "average treatment effect on the treated" (13.1 percent). These results offer conflicting evidence. The more robust and negative effects observed for the sub-sample is consistent with the hypothesis that schooling offers lower benefits for less-academically inclined youth. However, the estimates of the "average effect of the treatment on the untreated" do not support the notion that those who currently do not complete school would not receive as great a benefit from doing so as that received by those who currently do complete school.

Turning to the evidence with respect to the impact of schooling on employment opportunity, initial estimates for the full cohort suggest the effect of completing school as opposed to leaving after Year 10 is actually to increase the probability of being unemployed from 6.6 percent to 9.0 percent for those participating in the labour force between the ages of 16 to 21. The "penalty" associated with further years of education is higher when controls for ability are included and when the sample is restricted to those who are predicted to have a lower likelihood of completing school. These results are consistent with additional years of schooling providing a lesser benefit (or, in this case, a greater penalty) for youth of lower academic inclination. No statistically significant findings relating to employment opportunity could be drawn from the models for school leavers who did not go on to further education, the model incorporating the interaction term, or from propensity score matching.

Thus in the case of both wages and the incidence of unemployment, there is evidence of the benefits to schooling being concentrated among the most able. The results broadly confirm the existence of synergistic effects between schooling and natural ability as proxied by performance in English and mathematics in Year 9. This may appear self-evident. Assuming individuals possess at least some degree of idiosyncratic information as to whether or not remaining in school is the best option for them, we would expect a self-selection process in which those standing to gain the most from school would, on average, be more likely to continue in school. However, empirical validation of this relationship is not trivial given the continuing pressures to increase the level of schooling provided to all.

As to whether further years of schooling is detrimental to some young people, as the literal interpretation of many of the results suggests, is less clear. The variety of econometric methods used also presents a fairly wide range of estimates of the effect of additional years of schooling. The unanticipated result that further years of schooling has a negative impact on full-time weekly wages and the incidence of unemployment for the full sample, even without controls for ability, leaves a question mark over the findings. This is not consistent with most other studies, but is supported by previous analyses of LSAY cohorts suggesting time in full-time work may offer greater benefits during a cohort's early years of labour force participation than further time in school (Marks *et al*

2003, McMillan and Marks 2003). Further, the estimation methods used in this analysis to exploit the longitudinal nature of the data means that the estimates are likely to take greater account of fixed, individual effects ("unobservables") than many other studies, particularly those based on cross-sectional data. Note also that the data apply only to earnings in the very early years of the individuals' careers, particularly in the case of individuals who went on to post-secondary education. The effect of further years of schooling and subsequent education may manifest itself in higher future earnings growth and the results may be different with the benefit of a longer evaluation horizon. Perhaps greatest store can be held in the results from the instrumental variables estimations, which accord more closely with existing estimates of the effect of schooling for the full sample, and suggest, with one notable exception, that there is no significant gain from additional years of schooling for the less academically inclined.

Even if we dismiss the possible detrimental effects of further schooling upon some individuals, there are theoretical grounds to expect that general efficiency of the labour market may suffer from across-the-board increases in schooling attainment. This will occur where years of education provides a substantive signalling role for employers and in allocating youth to alternative pathways. More importantly, the assessment has been based on "point-in-time" indicators of success, rather than cumulated earnings. The costs of additional years of schooling in terms of foregone earnings and direct education costs have not been considered. If the effect on wages and employment opportunities is zero or marginal for some group when observed in later years, it is highly likely that net impact of remaining in school will be negative in terms of a net-present-value calculus.

I am not advocating that young people who are unhappy in school or performing badly should simply drop out. However, in such situations, other alternatives such as reasonable job openings, traineeships and apprenticeships should not be ignored for the sake of accumulating years of schooling. From a policy perspective I do not believe that there is sufficient empirical evidence to support mandated increases in the level of schooling beyond current levels, as is often claimed to be the case. Heterogeneity in the returns to schooling exists because individuals are heterogeneous. The objective of policy should be to ensure there are alternative pathways and institutional arrangements available to meet the varying needs, abilities and preferences of young people, and to make available the information they require to make informed decisions on what is optimal for them.

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APPENDICES: STATISTICAL METHODS AND ADDITIONAL RESULTS

APPENDIX A: LOGISTIC REGRESSION - PROBABILITY OF COMPLETING SCHOOL/BEING IN EMPLOYMENT

The probability of an individual completing Year 12 is estimated using the standard logistic regression model of the form:

(A1) $$Log\, P(C_i) \big/ 1 - P(C_i) = \alpha + \beta X_i' + \mu$$

where C_i is the completion indicator and $P(C_i)$ the probability that individual i will complete high school. X_i' is a vector of individual characteristics with associated vector of coefficients β to be estimated. The coefficients reported in Table 5 represent the effect of the variables on the log of the odds of completing Year 12.

In the estimation of the conditional probability of being in employment, $P(C_i)$ is replaced in Equation (A1) with the probability of the individual being employed, with the estimating sample reduced to those participating in the labour force (either employed or unemployed). Multiple observations for the one individual will also now be present in the data. Each individual can contribute a maximum of 6 observations on their employment status, corresponding to the 1997 to 2002 surveys. The classical assumption in estimating equation A1 is that the error terms between observations are uncorrelated, that is $\mathrm{cov}(u_i, u_j) = 0$ for all $i \neq j$. The models of employment status are estimated using the SAS Genmod procedure, which allows for a structure in the variances of the error terms for observations for the one individual at different timepoints. That is, the within-subject covariances, $\mathrm{cov}(u_{i,t}, u_{i,t+k})$, k=1 ..6, are not assumed to be zero but are additional parameters to be estimated. In this case unstructured option within PROC Genmod is used for the structure of the variance matrix, and the results are not sensitive to alternative options. The allowance for correlations between the error terms for individuals does not effect the parameter estimates, only the estimates of their standard errors and thus the significance levels associated with the parameter estimates.

The full results for the estimates for logit models of employment outcomes across the restricted samples of “non-academically inclined” youth using PROC Genmod are reported in the following table.

Table A1 Probability of employment, conditional on participating in the labour force; logit estimates for sub-samples of non-academically inclined youth

	Lowest 50% of predicted likelihood to complete school (A1.1)		School leavers not entering post-school education (A1.2)	
Variable	$\widehat{\beta}$	Pr > |t|	$\widehat{\beta}$	Pr > |t|
Intercept	1.9165	0.036	0.2160	0.823
Male	-0.0233	0.811	0.0717	0.542
Married	0.2665	0.175	0.0957	0.634
Has Children	-0.9239	0.004	-0.8411	0.009
Cohort age (yrs)	0.1425	0.000	0.1041	0.007
Non-Eng sp. background	-0.9076	0.000	-0.6711	0.005
Had disability (1995)	-0.4926	0.038	-0.5756	0.043
Aboriginal or Torres St. Islander	-0.4639	0.087	-0.2567	0.453
Wealth Index [0-100]	0.0064	0.001	0.0066	0.007
Living Status (1997):				
— home, two parents				
- *at least one working*	—		—	
- *neither working*	-1.0578	0.000	-1.0340	0.000
— home, sole parent				
- *parent working*	-0.6015	0.000	-0.7821	0.000
- *parent not working*	-0.9813	0.000	-0.5594	0.029
— had left home	-0.7108	0.001	-0.8262	0.000
1995 test scores [0-20]				
— maths	0.0484	0.002	0.0610	0.001
Factors (1997) - extrovert	0.1566	0.005		
Factors - attitudes towards school (1995)				
— values what is taught	0.0841	0.145		
— happy & safe place	0.1987	0.000	0.1730	0.006
Years of schooling	-0.2462	0.003	-0.0361	0.678
Completed app'ship	0.5207	0.069	0.3139	0.326
Subjects	2433		3255	
Max obs per subject	6		6	
Observations used	7900		5979	
Fit statistics:				
Deviance (value/deg of freedom)	0.5795		0.5390	
Pearson χ^2 (value/ deg of freedom)	1.0003		0.9974	
Log Likelihood	-2283		-1607	

Note: Factor scores have a sample mean of 0 and standard deviation of 1.

APPENDIX B: INSTRUMENTAL VARIABLES

The determination of earnings and years of schooling can be seen as jointly determined by a system of two equations:

(B1a) $\ln Y_i = \alpha_0 + \delta S_i + \beta_0 X_i + \mu_i$

(B1b) $S_i = \alpha_1 + \beta_1 Z_i + v_i$

where Y_i is the wage and S_i the number of years of schooling attained for individual i. Many of the explanatory variables determining wages (vector X) will be common to those determining years of schooling (vector Z). Unbiased estimates of δ, the effect of schooling on earnings, can be obtained using the method of instrumental variables (or 2-stage least squares) provided some of the determinants of schooling do not also determine wages; that is, there are some elements of Z that are not elements of X. The components of Z have been already identified in Section 5 in the estimation of the probability of completing high school. Instrumental variables models are estimated using the IVREG procedure in Stata Version 7. The cluster option is specified to take account for repeated observations on individuals, and thus the standard errors used in calculating the t-statistics are robust standard errors.

APPENDIX C: PROPENSITY SCORE MATCHING

Consider the evaluation of the impact of a certain intervention on an outcome variable Y. In an experimental evaluation, all individuals could be randomly assigned to either a treatment group or a control group. Random assignment would ensure that the characteristics of the individuals that might also affect the outcome are randomly distributed across the two groups, and thus the average difference in outcomes between the treatment and control groups, $E(Y_i) - E(Y_0)$, provides an unbiased estimator of the effect of the treatment.

In the absence of an experimental evaluation design the characteristics of the two groups, and thus the outcomes, may vary systematically. Assuming outcomes are completely determined by treatment status and a vector of observable characteristics (X) then one way of recovering an unbiased estimate of the effect of the treatment is to compare $E(Y_1) - E(Y_0)$ only for individuals with identical X's. Of course, when X represents an extensive vector of individual characteristics, not all persons in the treatment group will have an exact match in the control group with whom their outcome can be compared. In these situations the individual can be excluded from the analysis; matched to their nearest neighbour (most similar individual) in the control group; or matched only if, by some rule, there is a reasonably close match.

Where the concern with the differences in characteristics between the treatment and the non-treatment groups is that those more likely to receive the treatment are in any case also more likely to achieve superior outcomes, then one way of matching is to condition on the likelihood of receiving the treatment, rather than on the set of all possible Xs. This is done by first estimating a propensity score based on the values of X for each individual:

C1 $P(T=1)_i \left| X_i \right.$

Again, there may not be an exact propensity score match in the control group for each individual in the treatment group, and instead individuals can be matched to their nearest neighbour according to the propensity score.

The matching estimators in this report are calculated using nearest neighbour matching on the propensity score. More specifically, the “psmatch27” command in STATA Version 7 was employed (See Leuven and Sianesi 2003). The model used to generate the propensity score (C1) is simply the existing model of the probability of an individual competing Year 12 estimated in Section 5 (Equation A1). The average effect of the treatment on the treated is given by:

C2 $\frac{1}{n}\sum_{i=1}^{n}(Y_{1i} - Y_{0j})$

where Y_{1i} is the outcome observed for each individual i in the treatment group and Y_{oj} the outcome observed for their nearest match in the non-treatment group. The summation is across the n individuals in the treatment group — in this case the group of individuals who completed Year 12. Note that the matching is not one-to-one. The same individual in the control group may serve as the "nearest neighbour match" for multiple individuals in the treatment group.

The average effect of the treatment on the untreated is given by:

C3 $$\frac{1}{n}\sum_{j=1}^{n}(Y_{oj} - Y_{1i})$$

Where the summation is now across the individuals in the control group (those who did not complete school).

To ascertain the significance of the estimators, the "bootstrapping" method is applied in which repeated drawings from the sample are used to generate an estimate of the standard error of the estimates (see Stata Reference Manual, Release 7, Volume 1 (A-G), pp. 165-174).