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The Impact of Early Schooling on Subsequent Literacy and Numeracy Performance – Estimates from a Policy Induced 'Natural' Experiment

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ABSTRACT

This paper exploits a policy-induced natural experiment that occurred in South Australia in the mid-1980s to generate a 'causal' estimate of the effect of schooling on the literacy and numeracy performance of school students in their middle years of secondary school (in Year 9 for most students). The *Early Years of School* policy changed the way that an identifiable subset of students progressed through junior primary school, causing them to obtain an additional year of schooling for any completed Grade or level compared with their predecessors. The impact of the policy change on the age-grade structure of student cohorts in South Australia is captured between two waves of longitudinal data. Based on the analysis of the impact of this policy change, it appears that an additional year of junior primary school increased the numeracy and literacy performance in mid-secondary school significantly, by around one half of a standard deviation. These effects were the same for boys and girls and were similar across the distribution of ability – they were the same for low school achievers as high ones.

JEL Classification: I21, I28.

Keywords: natural experiment, school achievement.

1. Introduction

Despite clear evidence that education and the subsequent labour market and life outcomes of individuals are positively correlated, economists worry about the extent to which the education process imparts additional skills to students. Screening or signalling theories can explain the positive correlation with outcomes as the reflection of another relationship – that between schooling and ability (Spence 1973, Stiglitz 1975). Empirical studies of the relationship between resources and student performance have been interpreted as showing little or no positive effect. This and the related literature on the impact of reductions in class size are strongly contested (Hanusheck 1986 and 2003 compared with Krueger 2003). The concerns about education's effect remain despite the existence of studies that use arguably exogenous variations in schooling to identify its impact on the wages earned by individuals subject to the variations (for example, Angrist and Krueger 1991, Harmon and Walker 1995, Ichino and Winter-Ebmer 1999). Doubts about the value of education also exist in other disciplines (see, for example, Wolf 2002).

Not surprisingly, educators show much less concern. Substantial literatures deal with issues about the way individual schools affect the performance of students, the impact of different pedagogical approaches on learning and how changed administrative arrangements might affect the outcomes achieved by individuals or groups of students.

Economists have used a number of approaches to identify the effect of schooling on subsequent outcomes independent of the effect of ability and other factors. One is the method mentioned above that exploits the existence of some external phenomenon that 'causes' a group of individuals to obtain a different level of schooling from their peers and predecessors to estimate how that different schooling influences their later outcomes. This approach is pursued in this paper. A policy-induced natural experiment is exploited here to estimate the effect of an additional year of schooling on the subsequent literacy and numeracy performance of young Australians.

In this case, the extra schooling undertaken by a subset of students was the outcome of a change of policy by one Australian State government. The *Early Years of School* policy influenced the way that some students in South Australia progressed through junior primary school from the mid-1980s. The policy affected individuals whose birthdays occurred in specific months of the year. Individuals born at other times were unaffected by the policy and provide a natural comparison group for assessing its impact on literacy and numeracy outcomes.

The policy intervention required students to undertake an additional year of junior school. In the data analysed in this paper, the literacy (specifically, reading comprehension) and numeracy performance of most students was measured prior to their fifteenth birthdays – hence they were required to remain at school by the legislated minimum school leaving age.[1] Most students in these data had not yet had an opportunity to "choose" their years of education. The amount of schooling they had completed at the time of the test was largely determined by exogenous government rules, which are used here to estimate the impact of an additional year of junior schooling on subsequent literacy and numeracy performance.[2]

Since the policy intervention affected the amount of schooling students undertook at the outset of their school life, the interpretation of its estimated effect should be informed by the early childhood development and junior school enrichment program literature. This literature, summarised in Currie (2001) for example, suggests that the effects of such programs can be pervasive, substantial and enduring.

Such studies are often limited by either the data used to evaluate the impact the programs or the nature of the programs themselves. The programs are typically directed towards educationally or socially disadvantaged students and those with sound evaluation designs are often small in their scale. The extent to which the findings from such programs can be generalised is unclear. The policy change analysed here is more general – it involved students from all social classes and ability levels born in particular months of the year undertaking an additional year of school.

Three substantive issues are addressed in the remainder of this paper.

First, what, if any, was the magnitude of the additional junior primary schooling on the student achievement – that is, literacy and numeracy performance – almost a decade later?

[1] Eighty six percent of the 2091 students in the samples used here were aged less than 15 years at October 1 in the year when they were tested.

[2] Students and their advisers were able to "choose" other dimensions of their education to that point, however, such as their grade when tested – they could have chosen to repeat years – and aspects of the quality of their schooling, for example the type of school they attended.

Second, was this effect common for both genders?

Third, was the effect the same across the distribution of 'ability'? That is, did the additional junior schooling have the same effect on the test scores of those at the top end of the achievement distribution as those at the bottom?

The paper is structured as follows. The next section contains a preliminary analysis of the data intended to motivate much of the remainder of the paper. The following section contains a summary of the literature on the association between early schooling and subsequent educational outcomes. Section 4 contains a description of the *Early Years of School* policy change that was implemented in South Australia in the mid-1980s and how it is exploited in this study. The following section contains a description of the data and methodology used in this paper. Section 6 contains the empirical results and Section 7 some concluding remarks.

2. Setting the Scene

This section contains summary discussion of the policy change and the literacy and numeracy outcomes of two cohorts of school students in South Australia. Information is provided about two cohorts of school leavers – one that was in mid-secondary school in 1989 and another in 1995. The first group pre-dated the *Early Years of School* policy change (described in Section 4), while a subset of the second was affected by it.

Figure 1 shows how the average literacy and numeracy performance measured in mid-secondary school in South Australia changed between the two cohorts. It shows the changes between the cohorts for individuals born at different times of the year. Individuals are split into three birth groups based on when in the year they were born: those born in the September quarter; those born between October and February (inclusive); and those born between March and June. Only the junior schooling of the first two groups was changed by the *Early Years of School* policy change.

In Figure 1, literacy and numeracy performance are measured as the standardised scores from tests undertaken by a sample of mid-secondary school students from all Australian jurisdictions in 1989 and 1995. The standardisation was performed separately for both cohorts, so any differences in the relative difficulty of the tests administered to the cohorts are ignored.[3] The average test score in both cohorts is set to zero and the distribution of scores constructed to have a standard deviation of one. Any change in performance between the cohorts shown in Figure 1 reflects a change in the relative performance of South Australian students born in specific months of the year compared to all Australian students. This change is measured in terms of 'standard deviations' of the standardised test scores.

Average relative literacy and numeracy performance changed little between the cohorts for the March to June birth group (the changes are not significantly different from zero at the 10 per cent level). The junior schooling of this group was not affected by the *Early Years of School* policy change. The changes in performance for the September quarter birth group, almost *half* of whose members spent more time in junior primary school as a result of the policy change, were not different from that of the March to June birth group. In contrast, the increases in literacy and numeracy performance of those in the October to February birth group were significant (at the 1 per cent level) and were significantly greater than those of the March to June comparison group (at the 10 per cent level). *Most* members of this birth group spent more time in junior primary school as a result of the policy change.[4, 5]

Figure 1 suggests that the average literacy and numeracy outcomes in mid-secondary school were higher for at least one of the groups affected by the *Early Years of School* policy change than the comparison group whose schooling was not affected. The evidence of any beneficial effect for the second group affected by the policy change is less clear. The remaining empirical sections of this paper are designed to inform and refine the estimate of the relationship between the early schooling of individuals and their subsequent literacy and numeracy outcomes.

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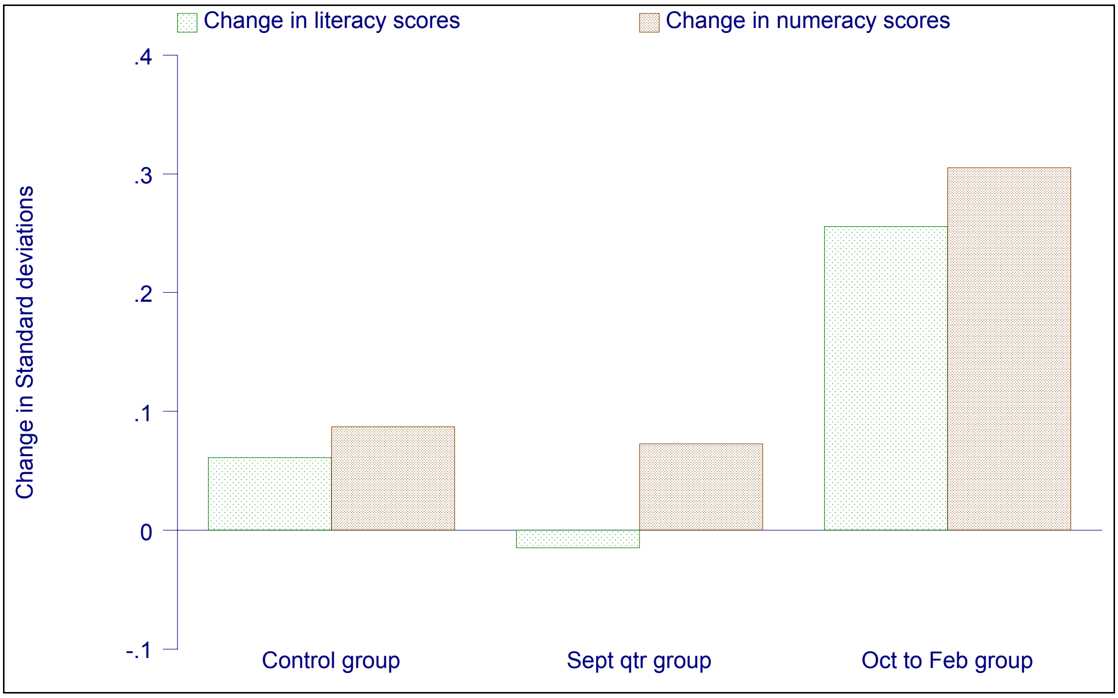
[3] These differences are captured in the performance scales used later in the paper, but are likely to be small. A substantial number of the items in the tests were common.

[4] Kolmogorov-Smirnov tests of the equality of the pre- and post-policy literacy and numeracy distributions using the standardised scores suggest that only the literacy performance of the October to February group generally improved between the cohorts, but that the numeracy performance of both the October to February and the March to June birth groups improved.

[5] These effects were specific to South Australia. In the two States with comparable pre-policy school structures to South Australia, Queensland and Western Australia, the literacy and numeracy performance of the October to February birth group fell relative to those born in March to June. This was partly due to the interaction of differences in the survey designs (see Section 5 below) and the age-grade structure of cohorts in the school systems of those jurisdictions. In jurisdictions with different school structures the change in survey design had no impact on the make-up of the October to February and March to June birth groups. There, the literacy and numeracy performance of the October to February birth group did not fall significantly relative to those born in March to June.

Figure 1: Change in average literacy and numeracy outcomes between the 1989 and 1995 cohorts in South Australia



3. Schooling and Literacy and Numeracy Performance

The *Early Years of School* policy change is described in more detail in the next Section. It was a policy change that required a subset of school students in South Australia to spend an additional year in junior primary compared with their predecessors. It affected students born in particular months of the year. The policy change is treated here as a completely 'exogenous' shock to students' schooling at the time their literacy and numeracy performance outcomes were measured, for a number of reasons. First, at the time of measurement most individuals were still required to be in school and had completed similar 'years of schooling'. Second, the principal determinant of whether individuals were affected by the policy change, their birth date, is not likely to be strongly correlated with ability and motivations, or other factors that affect school achievement. Therefore, the policy change can be interpreted as 'causing' any differential change in school achievement between those subject to the policy change and those unaffected by it.[6]

[6] This statement should not be taken too literally. The additional year of junior schooling may have aided student learning in subsequent years or somehow acted to change the experience of schooling of those who

3.1 Literature on the impact of early childhood education programs

The *Early Years of School* policy change required a subset of school students in South Australia to spend an additional year in junior primary compared with their predecessors. That is, it was an additional year of schooling for students at the outset of their school life. Therefore its impact might share similarities with other early childhood education and junior school enrichment programs (Raban 2000 reviews this literature). Unlike many of those programs, however, this policy was not directed towards students who were educationally or socially disadvantaged.

International evaluations of the impact of early childhood education interventions have focussed on four main issues: the type of effects such programs have; the magnitude of those effects; the duration or persistence of the effects; and the financial savings to government of the programs. The programs have typically been of two types: high cost demonstration programs with evaluation methodologies built into them, involving control groups for comparison with the groups receiving the program intervention; and lower cost, large scale public programs without identified comparison groups.

Studies that evaluate the first type of program indicate that their positive effects range from improving measured IQ, performance in school achievement tests, higher school completion and college participation rates, lower teenage pregnancy rates, better mental health outcomes, lower arrest rates, increased employment probabilities and higher levels of earnings (Currie 2001, Karoly, Greenwood, Everingham, Hoube, Kilburn, Rydell, Sanders and Chiesa 1998). Moreover, these effects can be large and are persistent, since some of the outcome measures relate to individuals in their late twenties (Karoly *et al.* 1998). That the effects of such programs might be large is consistent with research about the role of positive stimulation and other environmental factors on early childhood brain development (McCain and Mustard 1999).

Evaluations of larger scale public programs are more difficult to assess in the absence of appropriate comparison groups (see Currie and Thomas 1995 for an attempt to construct one in such circumstances). The effects appear to be smaller (like the per capita costs of the programs – Barnett 1998), but may act to reduce socio-economic status

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undertook it, possibly in the way they were treated by their teachers and others. That is, the additional year may have generated or 'caused' the observed effect, but in an indirect manner.

related disparities in school achievement. The effects may not persist, however, in the face of later, intervening factors such as low school quality (for example, Lee and Loeb 1995 and Currie and Thomas 2000, but see also Barnett 2002). Nevertheless, commentators such as Currie (2001) estimate the cost savings from such programs to warrant their expansion.

There are not comparable studies or programs in Australia. There are related literatures on the effect school-based programs can have on the development of literacy and numeracy skills among school entrants and on the social and academic development of boys (see de Lemos 2002 for a review of the literacy literature and Lingard, Martino, Mills and Bahr 2002 and Alloway, Freebody, Gilbert and Muspratt 2002 for interventions targeted at boys). Even here, however, de Lemos (2002) observed that there have been few 'systematic evaluations of specific teaching approaches or interventions on student outcomes in Australia' (2002: 13).

One exception is Ainley, Fleming and McGregor (2003) who assessed the impact of literacy programs undertaken in Catholic primary schools in Victoria on students in junior primary school. Schools were able to implement one of a menu of literacy programs. The Victorian Catholic Education Office had developed one of the programs, while the others had been developed elsewhere. Ainley *et al*. (2003) found that children in schools that adopted the local program experienced greater growth in literacy performance in Years 1 and 2 than children who did not undertake that program. Moreover, these performance differentials remained apparent at the end of Year 3. In addition, the literacy performance of a second cohort of Year 1 students from all Catholic schools who had experienced the new literacy programs only in their Preparatory Year was about one quarter of a year more advanced than the earlier cohort who did not start the programs until Year 1. More generally, however, Foley, Goldfeld, McLoughlin, Nagorcka, Oberklaid and Wake (2000) have observed that 'it appears that few Australian early childhood programs have been studied using rigorous research methods' (2000: 30).

3.2 Studies of the determinants of mid-secondary school achievement

The other literature of relevance to this study is that relating to the determinants of student achievement in the middle years of secondary school. Australian studies include

Williams and Carpenter (1990) and Rothman and McMillan (2003). Analysis of international student achievement projects, such as the Organisation for Economic Co-operation and Development's (OECD's) Programme for International Student Assessment (PISA) study and the Trends in Mathematics and Science Study (TIMSS) studies are also relevant.

These studies tend to show that student and school characteristics, as well as factors associated with the school 'environment' are important determinants of student performance.

Williams and Carpenter (1990) found that school achievement was affected positively by social background (parental education, occupation and family wealth levels), school type and academic aspirations[7] and negatively by family size and for students from non-English speaking language backgrounds.

Recent studies have addressed more directly the issue of school-based effects. Rothman and McMillan (2003) found that differences in the student composition of schools (differences in average student socio-economic status (SES) at schools and language backgrounds) and the school 'climate' accounted for about one half of the between-schools variation in performance, which in turn accounted for about one sixth of the between-student variation in performance. The report by the OECD that uses the PISA data (OECD 2001) found that student characteristics such as parental education, occupation and family wealth levels had positive effects on school achievement, while students from 'non-native' speaking backgrounds performed worse than those from native speaking backgrounds. School level characteristics, such as teacher 'support' (the extent to which they assisted students in their learning), student motivation, teacher performance (assessed by school principals) and time spent on homework were associated with improved school achievement performance, while poor student behaviour (absenteeism, bullying and alcohol use and drug taking) was associated with poorer performance. Analysis of the Australian PISA data by Lokan, Greenwood and Cresswell (2001) found that teacher support of students, a positive disciplinary climate and high teacher morale were all school-level variables with a positive impact on student performance in literacy, numeracy and science.

[7] As held by parents and teachers for the student, and among the student's friends.

4. The *Early Years of School* Policy

The *Early Years of School* policy was announced in 1984 in South Australia, with implementation to start in 1985. The elements of the policy and its rationale were set out in Education Department of South Australia (1983) and the final report of the Committee of Enquiry into Education in South Australia (Keeves Enquiry, 1982). The objective of the policy was to provide a better foundation for children's subsequent educational achievement by extending and enriching their junior primary education (that is, levels below Year 3). Its effect was to add an additional year of introductory schooling to students born at specific times of the year. The cohorts affected by the policy change reached secondary school (which commences in Year 8 in South Australia) in 1990 and Year 9 in 1991.[8]

South Australia has a 'continuous admission' policy for 5 year olds (see Trethewey 1997 for a description of the history of this policy). It involves regular (not less than once a term) admission of recently turned five year olds into individual schools over the school year. The way it operated prior to the *Early Years of School* policy meant that those five year olds who began school at the start of the school year moved directly into Year 2, having compressed Reception (a pre-Year 1 year of schooling) and Year 1 into just one year.

Figure 2 captures the key aspects of the *Early Years of School* policy change on the years of primary schooling undertaken by students. Those born between October and February who commenced school at the beginning of the school year completed eight years of primary school after the policy change. Prior to the change, about two thirds of that group completed just seven years. About one half of those born in the September quarter undertook over eight years of primary school after the policy change. Prior to it, almost the entire group undertook less than eight years of primary school. The *Early Years of School* policy did not affect the required years of primary schooling of those born between March and June.

[8] The policy changed induced some other changes in the South Australian school system. Resources were committed to the development of additional curriculum to support the *Early Year of School Policy*. If it supported studies where little curriculum existed applicable for the group that formerly did three years of school in two calendar years, this curriculum simply filled a gap that some curriculum must have been used for. If it was of a better 'quality' than existing curriculum, it seems most likely that it would have been used for the entire grade cohort and, hence, affected both the birth groups affected by the policy and the comparison group.

5. Data and Methodology

5.1 Data used in this study

This paper exploits data from two Longitudinal Surveys of Australian Youth (LSAY) cohorts to assess the impact of the *Early Years of School* policy change. The Youth in Transition 1975 birth cohort (YIT 75) and the Longitudinal Surveys of Australian Youth Year 9 cohort (LSAY 95) fall either side of the policy change and should capture its impact.

Figure 2: Changed junior primary school arrangements from the implementation of the *Early Years of School* policy

	Years of Primary school[a]: Before change[b]		Years of Primary school[a]: After policy change[b]	
October to February birth group	7	8	8	
September quarter birth group	7.3		7.3	8.3
March to June comparison group	7.5		7.5	

Notes: (a) Primary school in South Australia consists of Reception plus Years 1 to 7; Secondary school Years 8 to 12.
(b) The size of the dark shaded areas reflect the proportion of the group who completed the alternative number of years of primary school: about one-third for the October to February group before the policy change and one-half for the September quarter group after it.

The cohorts affected by the policy reached secondary school in South Australia after 1990 and Year 12 from 1994 onwards. The grade cohorts in YIT 75 reached Year 12 from 1991 through 1993. The LSAY 95 cohort started school after the policy change took effect and reached Year 8 in 1994. South Australian children in Year 9 in the LSAY 95 cohort should be older for their grade level than those in the YIT 75 cohort. Hereafter, the YIT 75 cohort is generally referred to as the 'late 80s' cohort and the LSAY 95 cohort as the 'mid-90s', which reflects the time when their members undertook the literacy and numeracy tests.

These cohorts are drawn from two-stage cluster samples of Australian school children. In the first stage, schools were randomly selected. In the second stage, students from those schools were randomly selected. In the late 80s cohort, individual 14 year old students were randomly selected; in the mid-90s cohort intact classes were randomly selected. The samples were stratified by school sector (government, Catholic or independent private schools – the latter two sectors were over-sampled). Population means in this paper are estimated with weighted data to account for this stratification, but the regression equations are estimated over un-weighted data. The literacy and numeracy tests were completed by students at their schools, along with a short questionnaire to elicit background information. Participants were surveyed in subsequent years by mail and/or telephone questionnaires.

Analysis of data from the mid-90s cohort in Appendix 2 of Ryan (2003) indicates that the *Early Years of School* policy change is reflected in the age-grade structure of individuals in the data in exactly the way it would be anticipated to be, once aspects of the survey design of the later sample are taken into account.

The major difference in the design of the two surveys is summarised in Figure 3. The first collection was an age-based sample of young Australians, the second a grade-based one. Consequently, individuals aged fourteen years (as of 1 October in the year they were surveyed) were distributed across grades or levels in South Australia in the first cohort as follows: 5 per cent were in Year 8; 70 per cent in Year 9; and 25 per cent in Year 10. For the second cohort, individuals in Year 9 were distributed across single years of age (on 1 October) as follows: 2 per cent were aged thirteen; 79 per cent were aged fourteen; and 19 per cent were aged fifteen. Had the *Early Years of School* policy not changed the age-grade structure in South Australia, these proportions would have been approximately 25, 70 and 5 per cent respectively, that is, the (reverse order) proportions from the earlier cohort.

Figure 4 contains more information about the two cohorts of South Australian students analysed here. It shows the proportion of students born in each month of the calendar year in the various school Years in the late 80s cohort (these students were all aged 14 years) and the proportion aged 14 years old in the mid-90s cohort (students in this cohort were all in Year 9). If the policy change had not taken place, these two representations would have been mirror images of one another, reflecting the differences in the design of the two surveys.[9]

The purpose of showing these proportions is principally to justify inclusion of the month on February among the months where students were affected by the policy change. In the late 80s cohort, 44 per cent of students born in February were in Year 10, somewhat higher than might be expected in only students who strictly started school a the beginning of the school year progressed to Year 2 after just one year of school. It seems that prior to the policy change a substantial proportion of those born in February were allowed to progress through Reception and Year 1 in just one calendar year. In the results presented throughout this paper, those born in February are included among the group affected by the policy change, though the section on the results also reports on the impact of excluding those students from the measurement of the impact of the policy change.

Figure 3: Age, grade and years of schooling relationships in the two surveys

		YIT 75	**LSAY 95**		
		Age	Age		
		14	13	14	15
			Years of Schooling		
			8 to < 9	9 to <10	≥ 10
Grade or Level	8	5			
	9	70	2 (25)	79 (70)	19 (5)
	10	25			

Figure 4: Comparison of the two cohorts in South Australia: age and grade structure and the effect of the *Early Years of School* policy

[9] In other jurisdictions these figures were mirror images of one another – see Ryan (2003).

	Grade of those aged 14 at 1 October			Age at 1 October if in Year 9		
	Year 8	Year 9	Year 10	Age 13	Aged 14	Aged 15
January		40	58		92	
February		52	44		96	
March		89			91	
April		93			87	
May		84			86	
June		88			88	
July		95			69	31
August		86			45	54
September		93			28	71
October		31	69		88	
November			88		89	
December			80		92	

	Sample size - based on unweighted data	Legend:
	SA	
YIT 75	645	most common classification
LSAY 95	1636	significant numbers

5.2 The literacy and numeracy scales

The literacy and numeracy scales used in this paper have two forms. The first, used in Section 2 of this paper is just a scale that reflects individual students' standardised performance – that is relative to the average number of questions all students in their specific sample answered correctly and scaled by the standard deviation of correct answers by students. In the student achievement literature, such scales tend to be heavily (negatively) skewed – with many students getting all or nearly all questions correct. The scale does not taken any account of the relative difficulty of questions, nor does it allow comparisons of performance over time, since the scale is measured relative to average performance in the students' specific sample.

Consequently, the testing literature has developed alternative approaches to the construction of test scales that incorporate the degree of difficulty of questions. The main methodological tool used in international studies to construct such scales is known as Item Response Theory (IRT).[10] It allows estimation of the probability that a person will answer specific items correctly from a pool of test items. This probability incorporates both the difficulty of the task and the proficiency of the individual in terms of the specific

[10] For example, the literacy and numeracy scales in the Organisation for Economic Co-operation and Development's Programme for International Student Assessment (PISA), the literacy scales used in the

ability tested through the questionnaire items. The resulting scales are less skewed and allow comparisons between student performances where they do not answer identical sets of questions. This means it is possible to compare literacy and numeracy performance over time.

The IRT literacy and numeracy scales used in this paper were developed in Rothman (2002). They have been constructed to have a mean of 50 and standard deviation of 10. The scales were used in Rothman (2002) to compare literacy and numeracy performance across cohorts of Australian mid-secondary school students from the mid-1970s to the late 1990s. Regression analysis based only on the IRT scales are reported in this paper – results based on the standardised scores were similar to those reported below.

5.3 Methodology

Exogenously determined variations in schooling, like the *Early Years of Schooling* policy change, can be exploited in one of two ways to provide 'causal' estimates of the effect of education on some other phenomenon. First, they allow estimation of difference in differences effects. These involve the comparison of changes in the mean values of key outcome variables of those affected by the policy change with those of a natural control group (as in Section 2 of this paper). This type of estimator is described in Angrist and Krueger (1999), Meyer (1995) and Heckman, Lalonde and Smith (1999). This report makes limited use of such a difference in differences estimator.

There are two potential problems for the difference in differences estimator for the application analysed in this paper (see discussion in the next section and analysis contained in Appendix 4 of Ryan 2003). The first is that a selection process of individuals into different grade cohorts after the policy changes appears to have taken place for the July – September birth group. This weakens the case for arguing the policy change had an 'exogenous' effect on this group, so the results for this group need to be interpreted with care. The second is that while members of the October to February birth group in the two cohorts were the same age, they were in Year 10 in the late 80s cohort,

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International Adult Literacy Survey (IALS) and the mathematics and science achievement scales used in but Year 9 in the mid-90s cohort. Hence any difference in differences estimate will conflate potentially offsetting effects – the positive effect of their longer entry experience and the potentially negative impact of being in a lower grade. The difference in differences estimates are likely to understate the true impact of the additional junior schooling on subsequent literacy and numeracy performance.

Consequently, the main way the policy change is used here is to use regression estimation of the effect of additional junior schooling on literacy and numeracy outcomes to account for these other education effects. The additional years of junior school are treated as exogenous effects on literacy and numeracy, though in the case of the September quarter group this may be questionable.

Let the literacy or numeracy test score, Y_{ij}, of individual i from school j be written as

(1) $$Y_{ij} = X_i{}'\beta + S_i{}'\delta + a_j + u_{ij}$$

where X_i is a set of individual background characteristics that influence the test score, a_j is a school effect and u_{ij} is a random error term. S_i is a set of five dummy variables designed to capture whether students were affected by the policy change. These variables capture whether individuals were in the October to February birth group, the September quarter group or March to June comparison group in either the late 80s or mid-90s cohorts. The omitted category is the March to June comparison group from the late 80s cohort. The parameter vectors, β and δ, are assumed to be common across individuals.

Note that the S_i variables reflect whether individuals belong to the particular birth groups. Hence differences in the δ parameters for relevant birth groups between the two surveys measure the effect of the policy change on the average numeracy and literacy performance of the birth group. They do not measure the impact of an additional year of junior school. This effect is given by:

(2) $$[(\delta_{k,90} - \delta_{k,80}) - \delta_{mj,90}]/\, p_k\,, \qquad k = \text{Oct-Feb, Sept quarter}, \; mj = \text{Mar-June}$$

where p_k is the proportion of students in the various birth groups of the second cohort whose schooling was affected by the policy change. For the October to February group

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the Trends in Mathematics and Science Study (TIMSS) were all developed using IRT.

this was about two thirds of all students; for the September quarter group it was just under one half of students.

Equation (1) might be re-written as

(3) $Y_{ij} = X_i{}'\beta + S_i{}'\delta + T_j\pi + \tilde{a}_j + u_{ij}$

if information about school characteristics, T_j are available. In this case, the residual school effects are denoted by $\tilde{a}_j$.

In this study, the variables covered among the *X*'s include:

- the level of school students were in and their age when tested;
- student's socio-economic background, as measured by their father's occupation;
- parental education levels, which reflect possession of university-level qualifications by students' parents;
- whether students lived in the metropolitan region or not;
- students' gender; and
- students' language background, measured by the language of their mother's country of birth.

The school characteristics included in the T_j variables include:

- the type of school students attended (government, Catholic or independent private);
- the ranking within their cohort of the average school socio-economic background;
- the proportion of students in the sample from the school whose mothers were born in predominantly non-English speaking countries;
- the proportion of students in the sample from the school who were male; and
- the proportion of students in the sample from the school who indicated that they intended to complete Year 12 while in mid-secondary school.

The impact of the policy is estimated here with the dummy variables for the different birth groups. From the data, it is not possible to identify those individuals who spent more time in Reception as a result of the policy change, though it is clear most people in October to February group in mid-90s cohort did so. As noted above, the effect estimated by the δ parameters here is that of the policy on the birth groups affected by it, not that of an additional year of Reception.

Equations (1) and (3) have the structure of a panel data model. They might be estimated assuming that the school effects are either fixed or random. In tests described in the next section, the estimates of the β and δ vectors from the random effects estimator of (3) cannot be distinguished from either the least squares estimation of (3) or the fixed effects estimator of (1).[11] Therefore, the presentation of the results focuses on the results of the least squares estimator.[12] This facilitates comparison of the results with those of the quantile regression estimator used to estimate the impact of the policy change at different points in the school achievement distribution (see Koenker and Bassett 1978 and Buchinsky 1998). This estimator is given by:

(4) $$Y_{ij} = X_i'\beta_\theta + S_i'\delta_\theta + T_j\pi_\theta + \tilde{a}_{\theta j} + u_{\theta ij} = Z_{ij}'\varphi_\theta + \tilde{u}_{\theta ij} \text{ , Quant}_\theta\,(Y_{ij}|Z_{ij}) = Z_{ij}'\varphi_\theta$$

where Quant $_\theta$ $(Y_{ij}|Z_{ij})$ denotes the conditional quantile of Y_{ij} conditional on the regressor vector Z_{ij}. The distribution $\tilde{u}_{\theta ij}$ of is not specified, but it is assumed $\tilde{u}_{\theta ij}$ satisfies the quantile restriction Quant $_\theta$ $(\tilde{u}_{\theta ij}|Z_{ij}) = 0$.[13] In this application, the quantile regressions are estimated at the 10th, 25th, 50th, 75th and 90th percentiles, with tests undertaken of whether the parameters on the birth group variables differ at these percentiles. Levin (2001) used quantile regression techniques to assess the impact of class size and peer effects on school achievement.

The issues of interest in this paper are examined with the use of the regression estimates from equations (3) and (4) in the following way. First, the parameter vector from equation (3) is used to test the impact of the policy change on literacy and numeracy outcomes. Second, the parameter vector from equation (3) is allowed to be differ between males and females to estimate whether the policy change had a different impact on the literacy and numeracy outcomes of males compared with its effect on females. Third, equation (4) is estimated at differing percentiles of school achievement to test

[11] The fixed effects estimator does not allow estimation of the π matrix in equation (2), so it estimates only equation (1).

[12] The clustering of the data by school is taken into account in estimation of standard errors.

[13] The clustering in the data are not taken into account directly in the estimation of the standard errors, which are bootsrapped, based on 200 replications.

whether the impact of the policy change varied across the distribution of school achievement.

Not all students in mid-secondary school in South Australia need necessarily have commenced their schooling in that State, though here it is implicitly assumed that most did. Those born overseas are excluded from the analysis that follows. Those who started their schooling in other States cannot be separately identified, so all Australian-born students observed in South Australia in mid-secondary school are treated as though they commenced their schooling there. So long as migrating students are distributed randomly by birth date, this should have few implications for the analysis that follows.

6. Results

6.1 The effect of additional junior schooling on literacy and numeracy performance

Both the difference in differences and regression-based estimates suggest that the *Early Years of School* policy change had a significant effect on the literacy and numeracy performance of students in South Australia, for at least one of the groups affected by it. The improvement in performance for the October to February birth group was significantly greater than that of the March to June comparison group. The results with both estimators appear in Table 1. In the difference in differences estimates for both numeracy and literacy, this effect was an increase of 2 in the IRT scale, corresponding to 0.2 of a standard deviation.

However, as noted earlier, these estimates are likely to be biased down, because the students in the later cohort were in a lower grade. The regression estimates take this grade change into account. The regression estimates suggest that the effects were larger for the October to February birth group; corresponding to an increase in literacy performance of 5.2 and numeracy performance of 3.5 units (or 0.52 and 0.35 of a standard deviation respectively).

To get some idea of what these magnitudes imply in terms of performance rankings, an individual with an average score exhibits a higher level of school achievement than 50 per cent of her peers. If her performance improved by 0.52 and 0.35 of a standard deviation in literacy and numeracy, her performance would exceed 70 and

64 percent of her peers respectively in those areas, using the standard normal distribution. This amounts to a very substantial improvement in relative performance.[14]

Students in the September quarter birth group did not achieve any increase in outcomes between the cohorts relative to the March to June group. Those who undertook the additional year of junior school arising from the *Early Years of School* policy among that group appear to have been a below average ability group to commence with. The group aged 15 in the September quarter birth group in the mid-90s cohort had lower average literacy and numeracy scores than the 14 year olds born in the same months of the year (lower by about three units or 0.3 of a standard deviation in both cases). Obviously, a selection process based on initial school performance must have taken place for students born in that quarter, such that completion of the extra year of junior school was not random. When the regression equation was estimated with that group excluded, the estimated parameters on the remaining variables changed little from those presented.

[14] The estimated change in relative rankings is identical if the average literacy and numeracy scores of individuals from the October to February birth group in the late 80s cohort are used.

Table 1: Changes in literacy and numeracy performance between the two cohorts

	Control group	Sept qtr group	Oct-Feb group
Literacy			
	Difference in differences estimates		
Change between cohorts	0.5	0.0	2.6
't-value' of change	0.69	0.04	3.13***
Difference from control group		-0.5	2.0
't-value' of difference		-0.41	1.79*
	Regression-based estimates		
Change between cohorts	2.3	2.5	7.5
't-value' of change	2.65***	2.79***	4.13***
Difference from control group		0.2	5.2
't-value' of difference		0.20	4.13***
Numeracy			
	Difference in differences estimate		
Change between cohorts	2.1	2.1	4.1
't-value' of change	2.71***	2.26**	5.01***
Difference from control group		0.0	2.0
't-value' of difference		-0.01	1.76*
	Regression-based estimates		
Change between cohorts	3.4	4.1	7.0
't-value' of change	3.31***	4.13***	4.53***
Difference from control group		0.4	3.5
't-value' of difference		0.39	3.15***

"***", "**" and "*" indicate significant at the 1, 5 and 10 per cent levels respectively.

The full set of regression results appear in Table 1.2 of Appendix 1. Other aspects of the results presented there of note include that individual socio-economic status, as measured through parental education and occupation, has a positive impact on school achievement. School grade effects are important – school achievement increases with students' current level of study. Age also matters – students older than 'typical' in Year 9, all else equal, have poorer literacy and numeracy performance levels. The performance of males relative to females may have deteriorated between the cohorts in both literacy and numeracy.

Finally, school effects, in terms of the background of students, are important in explaining individual student performance. Students at schools with, on average, other students from high socio-economic status, English-speaking backgrounds and where a high proportion of students intend to undertake Year 12, perform individually better than students with similar background characteristics who attend schools with less advantaged or ambitious student bodies.

Tests of the regression specifications indicated that the results were robust. The results reported in Tables 1 and 1.2 are based on least squares estimates.[15] The β and δ vectors from least squares estimation of equation (3) for both literacy and numeracy outcomes were not significantly different from those estimated by random effects estimation of equation (3).[16] The equations for both literacy and numeracy survived RESET tests for non-linearities in the function forms.[17] The estimated policy effects for the October to February group relative to the comparison group changed only marginally when those born in February were excluded from the analysis.[18]

6.2 Differences in the junior schooling effect between males and females

The impact of the additional junior schooling from implementation of the *Early Years of School* policy change was similar for males and females. The estimated effects are summarised in Table 2. The test involved estimation of equation (3) with a common set of β estimates, but allowing the δ parameters to differ for males and females.[19]

From Table 2, the estimated effect of the additional junior schooling on literacy outcomes was similar for boys and girls in the October to February group. The difference in the point estimates of the numeracy effects was greater, but it was not significant.

[15] The standard errors of the equation take account of the school-based clustering of observations.

[16] And the random effects estimates were not rejected by a Hausman (1978) test against the fixed effects estimates from equation (1). The p-values were 0.85 and 0.79 for the literacy and numeracy equations respectively. The random effects estimates provided similar policy change effects to those reported in Table 1. The estimated effects for the October to February group relative to the comparison group were 5.0 in the literacy equation and 3.1 in the numeracy equation. The relevant estimated effects for the September quarter group were 0.2 and 0.5.

[17] The p-values for the tests against inclusion of squared, cubic and quartic terms were 0.26 and 0.70 for the literacy and numeracy equations respectively.

[18] The literacy effect increased to 5.3 and the numeracy effect to 4.0.

[19] When equation (3) was estimated separately for boys and girls, the estimates of δ generated similar values to those in Table 2. The differences in the male and female point estimates tended to be slightly higher than those reported in the table, but none were significant. Formal tests rejected the equivalence of the complete set of parameters between the equations for both the literacy and numeracy variables. The rejection stemmed from differences in the π parameters – the school effects were much larger for boys than girls. For example, the proportion of students who were male had a significant negative effect on performance in both literacy and numeracy for males, but no effect in the female equations.

Therefore, males and females born in the October to February group and hence subject to the policy change experienced a similar increase in their literacy and numeracy skills compared with those of the March to June comparison group.

For the September quarter group, neither the estimated effects, nor the differences between the male and female estimates were significantly different from zero.

6.3 Differences in the junior schooling effect across the ability distribution

Like the results for males and females, the impact of the additional junior schooling from implementation of the *Early Years of School* policy change was broadly similar across the distribution of achievement. Tests of significance indicated that there were few significant differences in the effect of the additional schooling between those with high levels of literacy and numeracy performance and those with low levels of performance. The results of the estimation of quantile regression analysis are summarised in Table 3.

Table 2: Junior school effects on literacy and numeracy for males and females[a]

	Literacy	Numeracy
October to February group		
Males	4.8***	4.3**
Females	4.8**	2.6
Total	5.2***	3.5***
P-value of test of differences between male and female parameters	0.98	0.42
September quarter group		
Males	0.7	0.9
Females	0.1	0.4
Total	0.2	0.4
P-value of test of differences between male and female parameters	0.78	0.82

(a) Comparison of change in performance of the Oct – Feb birth group with the Mar – June comparison group. Effect based on regression estimates.
"***" and "**" indicate significant at the 1 and 5 per cent levels respectively.

A common interpretation of quantile regression results is that variation in the parameters across different quantiles reflects unobserved heterogeneity in the way the outcome variable responds to the independent variables across the distribution, often interpreted as incorporating the interaction of the independent variables and unobserved ability (see Aias, Hallock and Sosa-Escudero 2001 and Machado and Mata 2001, for example). In this application, such parameter variation may reflect the interaction of the policy change effect with unobserved general ability on the specific abilities tested through the literacy and numeracy instruments.

Of note from Table 3 is that the estimates from median regression analysis (the reported 50th percentile results) show broadly similar point estimates to those of the least squares regression equations the October to February group. If anything, the literacy effects for the October to February group appear to be more pronounced at the lower end of the performance distribution, though the differences across the quintiles are not significant. The point estimates for numeracy performance are largest at the 75th percentile, which is significantly different from the estimated effect at the 50th percentile at the 5 per cent level.

Similar patterns are apparent in the estimated effects for the September quarter group. The point estimates for the literacy effects are greatest at the 10th percentile and the numeracy effects largest at the 75th percentile, which is significantly greater than the estimated effect at the 50th and 25th percentiles at the 5 per cent level and than the effect at the 10th percentile at the 10 per cent level.

In general, the effect on literacy achievement was not statistically different across the distribution of performance within both groups affected by the policy change, while the effect on numeracy may have been more pronounced around the 75th percentile for both groups.[20] Potential interactions between the policy effect and unobserved general ability do not appear to have played a major role in this application.

[20] The impact of the policy change was also similar across socio-economic background. Fathers' occupations were used to assign individuals to the top, middle and lowest thirds of the socio-economic scale using the ANU 3 occupational prestige scale. Once more, the estimated policy effects for both birth groups affected by the policy change relative to the comparison group were not significantly different across the three SES groups.

Table 3: Junior school effects on literacy and numeracy – quantile regression estimates(a)

	Literacy	Numeracy
<u>October to February group</u>		
Least squares regression	5.2***	3.5***
Quantile regression		
10th percentile	5.7***	2.6
25th percentile	6.5***	5.3***
50th percentile	5.6***	2.6
75th percentile	4.8***	5.9***
90th percentile	2.8	2.8
P-value of test of differences between 10th and 90th percentile effects	0.40	0.96
<u>September quarter group</u>		
Least squares regression	0.2	0.4
Quantile regression		
10th percentile	1.3	-1.5
25th percentile	0.5	-0.9
50th percentile	-0.5	-0.7
75th percentile	-0.1	2.6*
90th percentile	0.7	1.3
P-value of test of differences between 10th and 90th percentile effects	0.80	0.24

(a) Comparison of change in performance of the Oct – Feb birth group with the Mar – June comparison group. Effects based on regression estimates.
"***", "**" and "*" indicate significant at the 1, 5 and 10 per cent levels respectively.

6.4 Why does the estimated impact of the policy differ between the two groups affected by it?

The impact of the *Early Years of School* policy is evident for the October to February group, but not for the September quarter group. Tests that the regression-based literacy and numeracy effects reported in Table 1 for the two birth groups were the same were rejected at the 5 per cent level.[21] Why would additional junior schooling have a positive impact on the literacy and numeracy performance of one group of students but not another?

One potential explanation would arise if the impact of the additional year differed across the distribution of performance and those who undertook the additional year were concentrated in particular regions of the distribution. For example, if those who undertook the additional year in the September quarter group were predominantly below average in terms of their school achievement and the impact of the policy was smallest in that region, the average estimated effect might be understated. Such an explanation may have some power for the numeracy results described in the preceding sub-section, but seems less compelling for the literacy estimates.

An alternative explanation for why the estimates for the two groups affected by the policy change should depart is that differing proportions of the groups were affected by the change. In the October to February group, about two thirds of the group undertook an additional year of junior schooling – in the September quarter group about one half of student undertook the additional year.

If the effect of interest, which should be similar for the two birth groups, is the effect of an additional year of junior school on school performance, then the estimated birth group effects need to be adjusted in line with equation (2) to account for the varying proportion affected. However, the estimated September quarter effects are so low, such an adjustment suggests even greater divergence in the effects on the two groups. The estimates appear in the top panel of Table 4.

One further reason for the difference between the two groups may be that the additional year was largely mandatory for the October to February group, but applied selectively for poorer performing students from the September quarter group. This selection process may potentially mask any positive impact for that group. While such an explanation may be plausible *prima facie*, it is not entirely satisfactory. The comparison made in the difference in differences estimates and the regression equations is between the entire groups born in the relevant months. If the performance of half of them was

[21] The p-values were 0.0003 and 0.0206 for the tests based on the literacy and numeracy estimates respectively.

positively affected by the additional year, while the school performance of the other half was unchanged, an increase in average performance should still be apparent in the estimates.

However, Figure 4 indicates that inclusion of those born in July in the September quarter group may be problematic. Only around 31 per cent of those students appear to have done the extra year of junior school and there is no clear pattern by day of birth within July with completion of the extra year. This suggests that the 'selection' based on ability may be more pronounced for those born in July than others in the September quarter group. When the equations were re-estimated with those born in July excluded (as well as those born in February)[22], the estimated effects on literacy and numeracy performance of the September quarter group were larger (see Table 4), but still not significant at conventional levels (p-values of about 0.15 in both equations). However, the estimated difference between the September quarter and October to February group effects in numeracy was no longer significant at the 5 per cent level. Moreover, the implied 'year of junior school' literacy and numeracy effects were not significantly different between the groups at the 5 per level.

The estimated 'year of junior school' effects are closer for the two birth cohorts in the lower panel of Table 4. Hence a substantial part of the divergence in the estimated birth group effects may reflect a selection process particularly prevalent for those born in July. The magnitudes of the additional year of junior school effect for the October to February birth group in Table 4 are broadly similar to those that appear for the Year 8 and Year 10 effects in Table 1.2. This implies that the additional year of junior school had a similar impact on school performance as the grade in which students were tested.

[22] Under half of those born in February were in Year 10 in the late 80s cohort. Unlike the July birth group, this was related to the day in February in which individuals were born. Those born in the first half of February were much more likely to be in Year 10 than those born in the second half. Those born in February were excluded in this equation as a robustness test, but the exclusion had little impact on the estimated effects.

Table 4: Junior school effects on literacy and numeracy: cohort and year effects[(a)]

	Literacy	Numeracy
Birth group effects		
October to February group	5.2***	3.5***
September quarter group	0.2	0.4
Adjusted year of school effects		
October to February group	8.5***	5.8***
September quarter group	0.4	0.9
Effects excluding July and February		
Birth group effects		
October to February group	5.2***	4.0***
September quarter group	1.8	1.7
Adjusted year of school effects		
October to February group	7.9***	6.0***
September quarter group	3.4	3.2

(a) Comparison of change in performance of the Oct – Feb birth group with the Mar – June comparison group. Effects based on regression estimates.
"***" and "*" indicate significant at the 1 and 10 per cent levels respectively.

7. Conclusion

This paper has used two cohorts of data on Australian school students to analyse the impact of a policy change that occurred in South Australia. The policy change caused some students to undertake more years of junior school there than their predecessors had done. The conclusion of this study is that the additional junior school had a positive impact on the literacy and numeracy outcomes achieved by those required to undertake it. Moreover, the effect was substantial. The results support the proposition that schooling, specifically entry-level schooling in this case, adds to the skills of students.

The policy implications of the paper are not that everyone should do more junior schooling. The main group affected by the policy change, the October to February birth group, clearly got too little of it – their school performance lagged behind that of their peers. The additional year of junior school they completed led to an improvement in their performance so that it was comparable to that of the comparison group.

The aim of the policy change, as originally promoted in the Keeves (1982) report was to improve the school achievement of students in South Australia. The evidence provided here indicated that it did and that the effect endured for almost a decade, such that it was evident among Year 9 students.

The Keeves Committee noted the strong cross-sectional relationship between school achievement and completion, and hoped that the policy change would also lead to an increase in school completion. The evidence on that is less clear. In the data from the same cohorts analysed in the body of the paper, the observed school completion rate increased by 5 percentage points for the March to June comparison group in South Australia between the cohorts, 4 percentage points for the September quarter group (neither change was significant) and by 13 percentage points for the October to February birth group (which was significant at the 5 per cent level). However, the difference between the October to February birth group change and that of the March to June comparison group was not significant.

This paper provides evidence that supports the productivity of the schooling process. Those students who had a longer junior school experience subsequently had better skills of the type that schooling produces – namely, literacy and numeracy skills. That the effects of the additional year were common across genders and the distribution of performance provides encouragement for the proposition that general (non-targeted) educational reforms and initiatives can provide substantial benefits in terms of student performance.

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Appendix 1: Data and Results

Table 1.1: Descriptive statistics

Variable	Mean	Std dev.
Year 8 when tested	0.01	0.11
Year 10 when tested	0.07	0.26
Aged 13 years when tested	0.02	0.12
Aged 15 or more years when tested	0.14	0.34
Male in 80s cohort	0.15	0.36
Male in mid-90s cohort	0.34	0.47
Father's occupation - manager	0.22	0.42
Father's occupation - professional	0.16	0.37
Father's occupation - associate professional	0.05	0.22
Father's occupation - trades	0.17	0.37
Father's occupation - clerk	0.04	0.20
Father's occupation - sales	0.06	0.24
Father's occupation - machine operator	0.06	0.23
Father has degree	0.18	0.38
Mother has degree	0.16	0.36
Both parents with degree	0.09	0.28
Metropolitan region	0.62	0.49
Catholic school	0.14	0.35
Independent school	0.22	0.42
Mother born o/s, English-speaking	0.11	0.32
Mother born o/s, non-English-speaking country	0.12	0.32
Average class SES	0.50	0.30
Proportion of class intending to do Year 12	0.80	0.13
Proportion of class with mother born in NESB country	0.12	0.11
Proportion of class male	0.49	0.25
Oct - Feb group in mid-90s cohort	0.26	0.44
Oct - Feb group in late 80s cohort	0.10	0.30
Sept qtr group in mid-90s cohort	0.18	0.39
Sept qtr group in late 80s cohort	0.08	0.27
Mar - June group in mid-90s cohort	0.27	0.44
Number of observations	2091	

Table 1.2: Least squares regression results

	Literacy		Numeracy	
	Coef.	't-value'	Coef.	't-value'
Year 8 when tested	-7.17	-5.55***	-7.29	-4.56***
Year 10 when tested	6.87	6.60***	5.64	5.69***
Aged 13 years when tested	1.98	0.91	1.53	1.10
Aged 15 or more years when tested	-4.37	-5.79***	-4.42	-5.51***
Male in 80s cohort	1.01	1.31	3.20	3.30***
Male in mid-90s cohort	-1.52	-2.71***	1.96	4.11***
Father's occupation - manager	2.78	4.25***	2.54	4.21***
Father's occupation - professional	3.92	3.79***	4.17	4.80***
Father's occupation - associate professional	2.98	3.49***	3.21	3.55***
Father's occupation - trades	0.99	1.60	1.23	1.59
Father's occupation - clerk	1.73	1.77*	2.10	2.06**
Father's occupation - sales	3.16	3.21***	2.25	2.28**
Father's occupation - machine operator	1.86	1.89*	0.76	0.88
Father has degree	3.26	3.71***	2.71	3.63***
Mother has degree	1.27	1.92*	1.24	1.95*
Both parents with degree	-0.94	-0.89	-0.65	-0.66
Metropolitan region	0.68	1.08	0.65	0.93
Mother born o/s, English-speaking	1.53	2.59***	0.94	1.54
Mother born o/s, non-English-speaking country	-1.75	-2.34**	-0.06	-0.10
Average class SES	4.37	3.04***	4.74	4.08***
Proportion of class intending to do Year 12	7.06	2.68***	5.56	1.84*
Proportion of class with mother born in NESB country	-6.26	-1.94*	-7.46	-2.71**
Proportion of class male	-0.66	-0.66	-0.61	-0.59
Oct - Feb group in mid-90s cohort	2.20	2.76***	2.90	3.12***
Oct - Feb group in late 80s cohort	-5.30	-4.95***	-4.33	-4.30***
Sept qtr group in mid-90s cohort	3.63	4.27***	4.79	4.90***
Sept qtr group in late 80s cohort	1.14	1.33	0.70	0.73
Mar - June group in mid-90s cohort	2.29	2.65***	3.68	3.78***
Constant	40.17	20.97***	39.21	16.46***
Observations	2091		2091	
R-squared	0.20		0.23	

Notes: the equation also included whether individuals attended a Catholic or an Independent school.