ELSEVIER

Estimating returns to education using different natural experiment techniques

Andrew Leigh*, Chris Ryan

Social Policy Evaluation, Analysis and Research Centre, Research School of Social Sciences, Australian National University, Canberra, ACT 0200, Australia

Abstract

How much do returns to education differ across different natural experiment methods? To test this, we estimate the rate of return to schooling in Australia using two different instruments for schooling: month of birth and changes in compulsory schooling laws. With annual pre-tax income as our measure of income, we find that the naïve ordinary least squares (OLS) returns to an additional year of schooling is 13%. The month of birth IV approach gives an 8% rate of return to schooling, while using changes in compulsory schooling laws as an IV produces a 12% rate of return. We then compare our results with a third natural experiment: studies of Australian twins that have been conducted by other researchers. While these studies have tended to estimate a lower return to education than ours, we believe that this is primarily due to the better measurement of income and schooling in our data set. Australian twins studies are consistent with our findings insofar as they find little evidence of ability bias in the OLS rate of return to schooling. Together, the estimates suggest that between one-tenth and two-fifths of the OLS return to schooling is due to ability bias. The rate of return to education in Australia, corrected for ability bias, is around 10%, which is similar to the rate in Britain, Canada, the Netherlands, Norway and the United States.
© 2007 Elsevier Ltd. All rights reserved.

1. Introduction

What is the economic return to an additional year of schooling? Over the past decade, a number of papers have sought to answer this question for various developed countries. Simple ordinary least squares (OLS) estimates are affected by two biases. First, ability bias may bias upwards the observed returns to schooling (e.g. because high-ability people find it easier to undertake education), or bias downwards the observed returns to schooling (e.g. if low-ability people compensate by completing more education). Second, measurement error might bias the OLS returns downwards.[1]

*Corresponding author. Tel.: +61 2 6125 1374; fax: +61 2 6125 0182.

E-mail addresses: andrew.leigh@anu.edu.au (A. Leigh), chris.ryan@anu.edu.au (C. Ryan).

URL: http://econrsss.anu.edu.au/~aleigh/, http://econrsss.anu.edu.au/Staff/ryan/contact_cr.htm.

---

[1] In a regression of one variable upon another, the effect of measurement error in the explanatory variable is to cause attenuation—biasing the coefficient towards zero (Greene, 2002).

Among the approaches that have been proposed for addressing the problem of ability bias, three natural experiment techniques stand out.[2] The first is to instrument for schooling using month of birth, taking advantage of the fact that school entry laws have a discontinuous effect on schooling in the presence of compulsory schooling laws. The second is to instrument schooling using changes in compulsory school laws. And the third approach is to use fixed-effects estimator on a sample of identical twins, for whom inherent ability and family background effects are assumed to be the same.

The first two instruments can be interpreted as correcting for ability bias directly in models where the effect of schooling on earnings is assumed to be linear and common across all individuals. In models where the effect of schooling is treated as heterogeneous, varying either across individuals or groups, these instruments identify a *local average treatment effect* (*LATE*)—that is, they allow estimation of the return to schooling among those whose schooling was influenced by the existence of the specific policy or its change (see the discussion in Angrist and Krueger (1999) for example). In a heterogeneous returns world, the one we consider most likely, the two policy instruments used here identify the effect of school policies or rules on the returns to schooling of early school leavers. While most of our discussion will treat these instruments as being informative about the impact of ability bias on estimates of the return to schooling generally, the last section of the paper exploits this *LATE* interpretation to assess the economic benefits of minimum school leaving legislation.

Our paper is novel in that we do not merely employ a single approach to estimate the rate of return to schooling. Instead, we set out to compare the rates of return using the three most prominent methodologies. We use data from Australia, a country for which the returns to education have been estimated using a large sample of twins, but where the other two instrumental variables approaches have not been employed. We find that there is little upward ability bias to the OLS estimate, and we estimate the ability-adjusted rate of return to schooling in Australia to be around 10%.

The remainder of this paper is structured as follows. Section 2 describes the data. Section 3 presents the OLS returns to schooling for several different measures of income. Section 4 presents estimates instrumenting schooling with month of birth, and Section 5 shows results instrumenting schooling with changes in school leaving laws. Section 6 compares these two estimates with the returns to schooling using twins studies. The final section concludes with a discussion of what our estimates imply for the cost of early school leaving.

2. Data

Our data are drawn from the Household, Income and Labour Dynamics in Australia (HILDA) Survey, an annual household-based panel study containing about 20,000 respondents, which began in 2001 (for more information on HILDA, see Watson, 2005).[3] Our income and earnings data are primarily drawn from the 2003 wave of the survey, and we include all respondents aged 25–64 with positive income who completed their schooling in Australia. We use a confidential version of the data set, which allows us to identify respondents' month of birth. The one notable drawback of this data set is that it does not identify the state in which the respondent attended school, and we proxy this by the current state of residence. Under most scenarios, this is likely to induce only attenuation bias into our estimates. Summary statistics, and information on variable construction and weighting, may be found in the working paper version of this paper (Leigh & Ryan, 2005). On average, respondents have 12.1 years of education (31% of respondents have less than 12 years of schooling).

Our variation in schooling arises from two sources: within-state variation in compulsory school

---

[2]Other researchers have used different instruments. Ichino and Winter-Ebmer (2004) used the effect of World War II on various cohorts of German students. Becker and Siebern-Thomas (2001) used the quality of schooling infrastructure across German states, and similarly Duflo (2002) used the quality of school infrastructure across Indonesian provinces. Card (1995) used geographic proximity to a US college. A number of non-experimental approaches have also been proposed. For example, Blackburn and Neumark (1995) attempted to solve the ability bias problem by including test scores in the estimating equation; while Vella and Verbeek (1997) and Rummery, Vella, and Verbeek (1999) used a rank-order instrumental variables estimator. Hogan and Rigobon (2002) used the structure of heteroskedasticity in wages and schooling to specify a generalized method of moments estimator that controls for unobserved ability, endogenous schooling and measurement error.

[3]The Census and the Survey of Income and Housing Costs were not usable for our purposes. For details, see Leigh and Ryan (2005).

leaving ages, and discontinuous cutoffs in school starting ages, which mandate that a child who is one day too young to be enrolled in school must wait another year to start school. In Leigh and Ryan (2005), we provide a detailed discussion of the relevant policies and policy changes.

3. Naïve returns to schooling

We begin by estimating the OLS returns to education, without correcting for ability bias. This involves estimating the regression:

$$\text{Ln}(Y)_i = \alpha + \beta\,\text{Educ}_i + \gamma Z_i + \varepsilon_i, \tag{1}$$

where Y is a measure of income, Educ is the individual's total number of years of education (taking into account schooling and post-secondary education), and Z is a vector of demographic characteristics. In this paper, we follow the existing literature in describing β as the "rate of return" to an additional year of education, notwithstanding the fact that β is an estimate of the pecuniary benefits of education, without subtracting the cost of education (in tuition fees and lost wages). We return to this issue in the conclusion.

Table 1 shows the returns to education from an extra year of school in an OLS specification. In each case, the sample is restricted to those aged 25–64, who are likely to have completed schooling, and not yet retired. The HILDA data set allows us to test the returns to education using a variety of different measures of income: total income over a 3-year period (pre- and post-tax), annual income (pre- and post-tax), weekly earnings, and hourly wages.

Panel A controls only for two fixed demographic characteristics: gender and year of birth. Using pre-tax annual income, the return to an extra year of schooling is 13% whether we use 3-year income or annual income, suggesting that year-to-year earnings fluctuations do not significantly bias downward the rate of return to education. Using post-tax annual income, the rate of return is slightly lower—around 11%—indicating that part of the gain from additional education is lost through progressive taxation. The rate of return is lower again when the income measure is weekly earnings (10% rate of return) or hourly earnings (8% rate of return). This indicates that those with more education not only earn higher hourly wages, they

also work more hours per week, and more weeks per year.[4]

Panel B controls for three additional choice variables that have been included in past Australian studies measuring the rate of return to education: married, female*married, and whether the respondent is working full-time. Note that if the decision to marry or work full-time is unrelated to ability and years of education, then these estimates should be identical to those in Panel A. By contrast, if ability or human capital accumulation has an effect on marital status or working full-time, then these estimates may differ. This indeed appears to be the case, with most of the estimates in Panel B being lower than the corresponding estimates in Panel A. In this specification, the returns to an additional year of education are 10% for pre-tax 3-year income and pre-tax annual income, 9% for post-tax 3-year income and post-tax annual income, 9% for weekly earnings, and 8% for hourly earnings. Only the hourly earnings estimate is unaffected by adding marital status and full-time controls.

These estimates are towards the high end of the comparable OLS estimates previously reported for Australia (for a survey, see Preston, 1997). For example, the OLS estimates of the return to education reported in Miller, Mulvey, and Martin (1995, 2006) for pre-tax annual income, controlling for marital status and full-time status, were 6.0% and 6.4%, respectively (our corresponding estimate is 9.8%).[5] However, in Miller et al. (1995), earnings are imputed as the average income in the respondent's two-digit occupation, while in Miller et al. (2006), earnings were coded into 12 bands. Both of these methods are likely to lead to more attenuation bias than the method used in the HILDA survey: asking for the respondent's precise income.[6]

---

[4] Conversely, since more educated workers have higher levels of labour supply, they may have lower levels of home production. If this were indeed the case, then estimates of the rate of return to education based only on market income would exceed the return to education from both market and non-market income. We are grateful to a referee for drawing this point to our attention.

[5] Miller et al. (2006) noted that income in their survey was reported using a prompt card that contained weekly, fortnightly and annual amounts. They used the annual income equivalents, and we interpret their results in the same manner here.

[6] There are two other possible explanations for the disparity. One is that schooling is better measured in HILDA (which contains precise information on whether the respondent completed 12, 11, 10, 9 or fewer years) than in the twins studies (which collapse 8–10 and 11–12 years of schooling). A second possibility is that the returns to schooling are lower for younger

Table 1
OLS returns to education

	(1)	(2)	(3)	(4)	(5)	(6)
Income definition	Log 3-year pre-tax income	Log 3-year post-tax income	Log annual pre-tax income	Log annual post-tax income	Log weekly earnings	Log hourly wage
Sample	25–64, positive income	25–64, positive income	25–64, positive income	25–64, positive income	25–64, positive earnings	25–64, positive earnings and hours
Panel A						
Years of education	0.128***	0.110***	0.130***	0.112***	0.099***	0.080***
	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]	[0.003]
Female	−0.594***	−0.512***	−0.626***	−0.541***	−0.510***	−0.111***
	[0.021]	[0.018]	[0.022]	[0.020]	[0.020]	[0.014]
Birth year FE?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6658	6658	7211	7211	4723	4694
R-squared	0.24	0.22	0.21	0.2	0.24	0.15
Panel B						
Years of education	0.101***	0.088***	0.098***	0.085***	0.085***	0.080***
	[0.005]	[0.004]	[0.005]	[0.004]	[0.004]	[0.003]
Female	0.019	0.017	0.067**	0.051*	−0.096***	−0.065**
	[0.030]	[0.026]	[0.032]	[0.028]	[0.031]	[0.026]
Married	0.203***	0.150***	0.168***	0.120***	0.142***	0.098***
	[0.028]	[0.024]	[0.030]	[0.026]	[0.024]	[0.023]
Female∗Married	−0.486***	−0.424***	−0.504***	−0.433***	−0.132***	−0.070**
	[0.038]	[0.033]	[0.040]	[0.035]	[0.037]	[0.031]
Full-time	0.700***	0.595***	0.836***	0.709***	0.857***	−0.018
	[0.022]	[0.019]	[0.024]	[0.021]	[0.025]	[0.019]
Birth year FE?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6658	6658	7211	7211	4723	4694
R-squared	0.38	0.35	0.37	0.35	0.47	0.15

Note: Robust standard errors in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively. Columns 1 and 2 use panel weights, other columns use person weights.

In the estimates that follow, we use as our measure of income the respondent's annual pre-tax income, and we do not control for the respondent's marital status or whether s/he worked full-time (i.e. the specification in column 3 of Panel A). We chose this specification on the basis that it takes into account the effect of education on hourly wages, hours worked per week, and weeks worked per year. Most importantly, using this specification most closely accords with the existing international literature (e.g. Angrist & Krueger, 1991; Ashenfelter & Krueger, 1994; Oreopolous, 2003), and allows us to compare rates of return to education in Australia with those derived from the leading studies of other countries.

(*footnote continued*)

respondents. The twins sample in Miller et al. (2006) were born in 1964–71. When we restrict our sample to this birth cohort, the estimated returns to schooling fall from 9.8% to 8.4%.

4. Instrumenting schooling with month of birth

One solution to the ability bias problem is to instrument for years of education. A suitable instrumental variable must meet two conditions: relevance and exogeneity. The relevance condition requires that the instrument be correlated with the number of years of schooling that an individual receives. The exogeneity condition requires that the instrument affects income only through the channel of schooling, and therefore that the instrument is uncorrelated with the error term in the income equation. For a general discussion of IV estimation, see Wooldridge (2002).

In this section, we use as an instrument an individual's month of birth. This instrument satisfies the relevance condition, since children who are born just before the school entry date will spend a full year more in school (at a common integer age) than children born just after the school entry date. Birth month will also satisfy the exogeneity

condition so long as it affects earnings only through the channel of school entry dates. We discuss below one possible way in which month of birth may violate the exogeneity condition, and our approach to deal with this potential problem.

Using birth date as an instrument for schooling was first implemented in Angrist and Krueger (1991), who found an ability-adjusted rate of return to schooling in the US of 9%. Using a similar methodology, Webbink and van Wassenberg (2004) found a rate of return to schooling of 8% in the Netherlands (though Plug, 2001 found lower estimates and argued that the Dutch effect operated through relative position, not total schooling). In the British context, Del Bono and Galindo-Rueda (2004) used variation in the way that month of birth interacts with the school-leaving age to show that increased schooling boosts the probability that an individual will be employed (they do not estimate the effect on earnings).

Australian states and territories typically operate in such a manner that they allow children to start school if they have attained a certain age (typically 5 years) by the cutoff date, and then permit children to leave school once they reach a certain age. School entry laws operate differently across states and years, but some have only a single entry date each year, meaning that a child who is too young to start school in one year is legally required to wait a full year before starting school.

Imagine two students: student A is born on the eligibility date for school entry, and student B is born one day after the eligibility date for school entry. Because of the discontinuous operation of the entry rules, student A will start school one year earlier than student B—despite being only one day older. If both students leave school as soon as they reach the school-leaving age, student A will have one year minus one day more schooling than student B.

If we regard month of birth as essentially random, it is possible to instrument for educational attainment using month of birth. So that our instrument has maximum power, we therefore restrict our sample to those states and birth cohorts for whom there was only one school entry cohort each year (other states had two or three entry cohorts per year). Our sample is therefore restricted to students born in Queensland, Tasmania, and Western Australia. Table 2 shows the relevant school entry dates. Note that since our focus is on those aged 25–64 in 2003, we focus only on those born in 1978 or earlier.

Table 2
School entry dates in states with a single entry date

State	Born	May start school if aged 5 years by:
Queensland	1945–51	31 December
Queensland	1952–78	28 February
Tasmania	1975–78	1 January
Western Australia	1945–78	31 December

As the above comparison between students A and B demonstrated, the month of birth instrument will have greatest effect on years of schooling if the sample is restricted to individuals born a few days before and a few days after the cutoff date. However, it is necessary to balance this additional precision against the reduction in sample size that this would necessitate. Given that the total HILDA data set contains only about 7200 individuals with positive annual income and that we have already restricted the sample to those born in certain states and years, it is necessary to include those born further away from the cutoff date.

We compromise on a 6-month "window", comprising those born 3 months before and 3 months after the cutoff date. However, the instrument has a different impact on years of schooling for an individual born 1 month before the cutoff date than for an individual born 3 months before the cutoff date. To take account of the fact, we therefore code whether the respondent is born 1, 2 or 3 months prior to the cutoff date, or 1, 2 or 3 months after the cutoff date.

Note however that month of birth may have an effect on educational attainment not only by influencing the amount of schooling received by an individual who leaves at the compulsory leaving age, but also via the "relative position effect". A child who is born just before the cutoff date will be the youngest person in her class, while a child born just after the cutoff date will be the oldest person in her class. If it is true that children learn more from being younger or older than their peers, then birth month may affect earnings not only through the *quantity* of schooling that a child receives, but also through the *quality* of that schooling (a violation of the exogeneity condition). Although we do not observe the ages of other children in an individual's class, we can include a linear term to control for her "expected relative position", assuming other students' birth months are uniformly distributed.

Our first stage equation is therefore

$$\text{Educ}_i = \alpha + \beta(\text{Months Before Cutoff})_i + \gamma Z_i + \pi(\text{Relative Position})_i + \varepsilon_i, \quad (2)$$

where Months Before Cutoff is an indicator variable taking six possible values (-3, -2, -1, 1, 2 or 3), Z is a vector of demographic characteristics—sex, indicator variables for year of birth, and indicator variables for state of birth, and Relative Position is a continuous variable taking the value 0 for a student born in the month prior to the cutoff (who we expect to be in the youngest twelfth of the class), $\frac{1}{11}$ for a student born 2 months before the cutoff, and so on, up to 1 for a student born in the month after the cutoff date (who we expect to be in the oldest twelfth of the class).

Our second-stage equation is

$$\text{Ln}(Y)_i = \delta + \zeta \hat{\text{E}} \text{duc}_i + \eta Z_i + \tau(\text{Relative Position})_i + \upsilon_i. \quad (3)$$

The first column of Table 3 shows the OLS estimate, using the same methodology as in Table 1, Panel A, column 3, but with state fixed effects, and restricting the sample to those born within 3 months of the cutoff dates listed in Table 2. Reassuringly, this OLS estimate is almost identical to the corresponding estimate in Table 1.

The second column shows the results using the *Months Before Cutoff* instrument. The *F*-test on the instruments shows that they are not jointly significant. Given that the instruments lack power in

the first stage regression, it is therefore unsurprising that the point estimate in the second stage regression is negative, with a 95% confidence interval ranging from -68% to 48%. We find no evidence of a relative position effect.

Note however that this approach constrains the effect of the instrument to operate equally for a respondent born 1 month before the cutoff date in 1945 and a respondent born 1 month before the cutoff date in 1978. However, over this period there has been a fall in the fraction of students dropping out of school at the earliest opportunity. In addition, it is possible that the extent to which the cutoff date was enforced may have changed over time.

To take account of these two possibilities, we interact the *Months Before Cutoff* indicator variable with the respondent's birth year, and use this new variable to instrument for years of education. Our first and second stage equations are therefore

$$\text{Educ}_i = \alpha + \beta(\text{Months Before Cutoff} \times \text{Birthyear})_i + \gamma Z_i + \pi(\text{Relative Position})_i + \varepsilon_i, \quad (4)$$

$$\text{Ln}(Y)_i = \delta + \zeta \hat{\text{E}} \text{duc}_i + \gamma Z_i + \tau(\text{Relative position})_i + \upsilon i \quad (5)$$

Column 3 of Table 3 shows the results of this estimation strategy. The *F*-test on the excluded instruments in the first stage regression shows that they are jointly statistically significant, at the 1% level. The high degree of statistical significance of

Table 3
Instrumenting schooling with month of birth dependent variable: Log annual income

	(1) OLS	(2) IV Birthmonth	(3) IV Birthmonth × Birthyear
Years of education	0.128***	−0.099	0.079**
	[0.013]	[0.295]	[0.032]
Female	−0.601***	−0.612***	−0.602***
	[0.051]	[0.069]	[0.057]
Relative position		−0.035	0.000
		[0.090]	[0.072]
Birth year FE?	Yes	Yes	Yes
State FE?	Yes	Yes	Yes
F-test for excluded instruments	—	0.65	554.89
		$P = 0.6605$	$P = 0.000$
Observations	998	998	998
R-squared	0.22	0.21	0.22

Note: Sample is restricted to those aged 25–64 with positive annual income, in the states and years listed in Table 2, born within 3 months of the cutoff date for school entry. Robust standard errors, clustered at the state*birth month*birth year level, in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

the excluded instruments makes it unlikely that we face the so-called "weak-instruments" problem (for a discussion of weak instruments in a similar context, see Bound, Jaeger, & Baker, 1995; Cruz & Moreira, 2005; Staiger & Stock, 1997). While some of the birth month–birth year interaction terms are not individually statistically significant, the high value of the *F*-statistic indicates that the set of interactions is jointly significant.

Using this IV strategy, the point estimate of the return to education is now 8%, which is significant at the 5% level (the 95% confidence interval ranges from 1% to 14%). Again, we find no evidence of a relative position effect. This IV estimate (8%) is about two-thirds as large as the comparable OLS estimate (13%), suggesting that ability bias accounts for about one-third of the OLS return to schooling.

5. Instrumenting schooling with changes in school-leaving laws

An alternative instrument to month of birth for completed schooling is to use changes in school-leaving laws. This will be a valid instrument if increases in compulsory schooling boost schooling attendance (the relevance condition), and if these increases are uncorrelated with the ability distribution of residents in that state (the exogeneity condition). If compulsory schooling laws are not enforced by state education officials, then this will violate the relevance condition, while if changes in school-leaving laws are driven by changes in ability, or if parents choose their state based on school-leaving laws, this will violate the exogeneity condition.

Do changes in school-leaving laws tend to increase educational attainment?[7] Most studies have concluded that there is an effect in the US (Acemoglu & Angrist, 2000; Oreopolous, 2003), though Goldin and Katz (2003) warn that changes in state compulsory schooling and child labour laws in the period 1910–1939 accounted for no more than 5% of the increase in the eventual educational attainment for the affected cohorts.

Studies in other countries have generally found that increasing the school-leaving age boosts educational attainment, including in Britain (Harmon & Walker, 1995; Oreopolous, 2003), Canada (Oreo-

polous, 2003), Norway (Aakvik, Salvanes, & Vaage, 2003) and Sweden (Meghir & Palme, 2003). In Germany, the results are more mixed. Pischke and von Wachter (2004) found that an increase in school-leaving laws boosted educational attainment for the cohort born 1930–1960, though Fertig and Kluve (2005) found that changes in the school-starting ages had no impact on total schooling for those born in 1960–1974. It is difficult to know whether this difference is due to the impact of school starting and leaving ages, or to the age of the two cohorts.

Using regional differences in compulsory-schooling laws as an instrument for educational attainment, several studies then estimate the rate of return to schooling. For the US, Acemoglu and Angrist (2000) estimated a private rate of return to schooling of 10%. Oreopolous (2003) used changes in school leaving laws in states/provinces in three countries. His central estimates were that an additional year of schooling boosts earnings by 16% in Britain, 8% in Canada and 13% in the US.[8] In Norway, Aakvik et al. (2003) reported a return to schooling of 10%. Looking at outcomes other than earnings, Black, Devereaux, and Salvanes (2004) also showed that law changes in Norway and the US had the effect of decreasing the rate of teenage childbearing; while Milligan, Moretti, and Oreopoulos (2003) showed that law changes in Britain and the US increased political interest and involvement.

Much smaller estimates of the economic returns to schooling have been found in Germany using this methodology. Pischke and von Wachter (2004) found that the introduction of a compulsory ninth grade boosted educational attainment by 0.17–0.6 years, but that this rise in educational attainment did not have a significant effect on earnings. They explain this on the basis that the alternatives after leaving school are very different in Germany from other developed nations, since "basic track" students tend to take apprenticeships rather than unskilled jobs when they leave school.

There are two ways in which compulsory-schooling laws can be coded. While most papers only code compulsory school leaving ages, an alternative is to also take account of changes in compulsory school

---

[7]In this section, we use the term "educational attainment" to mean total years of schooling. This is typically measured using surveys rather than administrative data.

[8]Harmon and Walker (1995) found a similar rate of return to schooling (15%) using increases in compulsory school attendance laws. However, unlike Oreopolous (2003), they did not include birth year fixed effects.

starting ages and use the difference to create a "total years of compulsory schooling" variable. However, for the birth cohorts upon which we focus (those born between 1939 and 1978), there were no changes in school-starting ages in any state or territory, and our analysis is therefore necessarily restricted to school-leaving ages. The relevant changes in school-leaving ages are shown in Leigh and Ryan (2005). Note that our coding is based on the way in which the leaving-age rule binds for the typical student. Hence a state where students can leave school on their 15th birthday is coded as 15, while a state where students can leave school at the end of the year in which they turn 15 is coded as 15.5.

How much of an impact did raising compulsory schooling laws have on educational attainment? Panel A of Table 4 shows the results from regressing total years of education on the school-leaving age in a given state and year, in a specification including gender, state-fixed effects, and birth year fixed effects. We find that a 1-year increase in the leaving age raises educational attainment about $\frac{3}{10}$ of a year.

To use compulsory schooling laws as an instrument for educational attainment, we estimate the following first stage regression:

$$\text{Educ}_i = \alpha + \beta(\textit{Compulsory School Law})_i + \gamma Z_i + \varepsilon_i \tag{6}$$

where *Compulsory School Law* is one of two indicator variables: the compulsory school leaving age, or the number of years of compulsory schooling.

As in the previous section, this approach constrains the effect of the instrument to operate equally for a 25-year old (born in 1978) and a 64-year old (born in 1939), despite possible changes in enforcement, and a reduction in the fraction of students dropping out of school at the earliest opportunity. We therefore also experiment with interacting the *Compulsory School Law* indicator variable with the respondent's birth year and use this new variable to instrument for years of education. This makes our first stage equation:

$$\text{Educ}_i = \alpha + \beta(\text{Compulsory School Law} \times \textit{Birthyear})_i + \gamma Z_i + \varepsilon_i. \tag{7}$$

In both cases, the second stage equation is

$$\text{Ln}(Y)_i = \delta + \zeta \hat{\text{E}} \, \text{duc}_i + \eta Z_i + \upsilon_i \tag{8}$$

Table 4
Instrumenting schooling with changes in school-leaving laws

	(1)	(2)	(3)
Panel A: Dependent variable is years of education			
School-leaving age	0.296***		
	[0.113]		
Female	−0.187***		
	[0.057]		
Birth year FE?	Yes		
State FE?	Yes		
Observations	7211		
R-squared	0.06		

Panel B: Dependent variable is log annual income

	OLS	IV Leaving age	IV Leaving age × Birthyear
Years of education	0.128***	0.191*	0.118***
	[0.005]	[0.098]	[0.035]
Female	−0.627***	−0.615***	−0.629***
	[0.022]	[0.029]	[0.024]
Birth year FE?	Yes	Yes	Yes
State FE?	Yes	Yes	Yes
F-test for excluded instruments	—	5.82	8.1e + 11
		P = 0.0002	*P* = 0.000
Observations	7211	7211	7211
R-squared	0.22	0.20	0.22

Note: Sample is restricted to those aged 25–64 with positive annual income. Robust standard errors, clustered at the state*birth year level, in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

Panel B of Table 4 shows the results of these two instrumental variable approaches. Using the compulsory school leaving age as an instrument, the rate of return is 19%, though this is only statistically significant at the 10% level. However, when the leaving age is interacted with birth year, the rate of return becomes 12%, which is statistically significant at the 1% level, and only slightly below the OLS rate of return. In each case, the *F*-test on the excluded instruments shows that they are statistically significant. In our view, the IV coefficient in column 3 (a rate of return of 12%) should be preferred on the basis that it is more precisely estimated than the estimate in column 2.

6. Three estimators compared

'Twins' studies exploit the idea that it is possible to estimate the causal effect of schooling on income by comparing the earnings received by twin pairs who obtain different amounts of schooling, but are assumed to have similar ability levels. By comparing the results from identical twin pairs and fraternal twin pairs, it is also possible to separately parse out the components of ability bias that are due to genetic characteristics and family background. Where subjects also record the education completed by their twin, it is possible to correct for measurement error in the education reported by individuals. Important studies of the return to education using US twins include Ashenfelter and Krueger (1994), Ashenfelter and Rouse (1998), Behrman, Rosenzweig, and Taubman (1994). This approach has also been implemented in other countries, including Australia (Miller et al., 1995, 2006), Sweden (Isacsson, 1999) and the United Kingdom (Bonjour, Cherkas, Hashel, Hawkes, & Spector, 2003). The main problems with the twins approach are that between-twin differences in schooling may not be random, but may be endogenous with respect to wages. In this event, IV estimates to correct for measurement error in reported schooling may exacerbate upward omitted ability bias in the estimated education effect (Bound & Solon, 1999; Neumark, 1999).

As we have discussed in Section 3, Miller et al. (1995, 2006) observe a lower OLS rate of return to education in their Australian twins samples (their estimates of the rate of return are around 6%) than we find using more precisely measured incomes from HILDA (using the same income measure and controls, our corresponding OLS estimate is 10%).

When they use the co-twin's education estimate to instrument for the twin's estimate of their own education, the rate of return rises to 7.5%, which is still below our OLS estimate. It is therefore conceivable that these studies have underestimated the true rate of return to education.

Table 5 compares our OLS estimator (from Section 3), month of birth IV estimator (from Section 4) and changes in school leaving laws IV estimator (from Section 5) with the identical twin estimates presented in Miller et al. (2006). We present three estimates from Miller, Mulvey and Martin—the OLS rate of return, the IV rate of return (using the co-twin's education report), and the IV rate of return with twin-pair fixed effects. For each approach, we show the estimated rate of return from schooling, and the ability bias, calculated as $1 - (\beta(\text{Ability Adjusted})/\beta(\text{Naive}))$.

The month of birth IV method (column 2) suggests an 8% rate of return to education, implying that ability bias amounts to 39% of the OLS rate of return. The changes in school leaving laws estimator (column 3) indicates a higher rate of return (12%), implying that the OLS estimator has very little bias. The identical twins estimator in column 6 suggests a lower rate of return to education (5%). The ability bias in this estimator depends upon whether it is compared with the naïve estimator in column 4 (ability bias of 10%) or the naïve estimator in column 5 (ability bias of 28%).

7. Discussion and conclusion

In this study, we have compared three estimators for separating the causal effect of education on income from any ability bias. We found that the naïve OLS returns to an additional year of schooling (controlling for age and gender) was around 13%. The implied ability bias is 9% when instrumenting with changes in school-leaving laws, 10–28% estimating a fixed effects model with identical twins, and 39% instrumenting with month of birth. Our preferred estimate of the ability-adjusted rate of returns to schooling in Australia is 10%, which is midway between our two new IV estimates.

For the purposes of separately identifying the components of ability bias that are due to genetic characteristics and family background, the twins estimator has a clear advantage over the other two estimators, which are not able to decompose the bias in this way. However, for the purposes of

Table 5
Three estimators compared dependent variable: Log annual income

	Leigh & Ryan			Miller, Mulvey & Martin		
	(1) OLS	(2) IV Birthmonth × Birthyear	(3) IV Leaving age × Birthyear	(4) OLS	(5) IV	(6) IV with twin-pair fixed effects
Years of education	0.130*** [0.005]	0.079** [0.032]	0.118*** [0.035]	0.060*** (0.005)	0.075*** (0.006)	0.054** (0.023)
Female	−0.626*** [0.022]	−0.602*** [0.057]	−0.629*** [0.024]	−0.169*** (0.034)	−0.179*** (0.033)	—
Additional demographic controls?	No	No	No	Yes	Yes	Yes
Birth year FE?	Yes	Yes	Yes	No	No	No
State FE?	Yes	Yes	Yes	No	No	No
Twin-pair FE?	No	No	No	No	No	Yes
F-test for excluded instruments	—	554.89 P = 0.000	8.1e + 11 P = 0.000	—	—	—
Observations	7211	998	7211	1518	1518	759
R-squared	0.21	0.22	0.22	0.40	—	—
Implied ability bias of naïve estimator		39%	9%			10% (OLS) 28% (IV)

Note: In columns 1 and 3, sample is restricted to those aged 25–64 with positive annual income. In column 2, sample is restricted to those aged 25–64 with positive weekly earnings, in the states and years listed in Table 2, born within 3 months of the cutoff date for school entry. Column 2 also includes a control for relative position in grade. Clustered robust standard errors in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively. Results in columns 4–6 are from Miller, Mulvey, and Martin (2006, Table 3, columns 1, 2, 4). Additional demographic controls column 4 are $e^{-0.1Age}$, Married, Married*Female and Employed Full-time. The Employed Full-time variable is responsible for the big difference in the Female parameter between the estimates of the two papers. Columns 5 and 6 also include all these controls, except Age and Female, which are absorbed by the twin fixed effect. Ability bias in columns 2 and 3 uses the column 1 estimate as β (Naïve). Ability bias in column 6 uses the estimates in columns 4 and 5 as β (Naïve).

evaluating the effect of raising school leaving ages, the other two approaches can be given a *LATE* interpretation and are likely to be more useful, since they are identified from the discontinuous impact of these laws, or from changes in the laws.

What do our results say about the effects of Australia's school leaving laws? As we noted in Section 3, what we have been describing as the "rate of return to education" is in fact only the benefit to an additional year of schooling, without taking into account the costs of education. To estimate the true rate of return to schooling, we compared age–income profiles for two individuals—one who obtains 9 years of schooling, and another who obtains 10 years of schooling, assuming a 10% rate of return to an additional year of schooling (for details of these calculations, see Leigh & Ryan, 2005). A person who left school at age 15 (with 9 years of education) and worked until age 64 could expect to earn $1,166,003 over his or her lifetime, while a person who left school at age 16 (with 10 years of education) and worked until age 64 could

expect lifetime earnings of $1,285,263 (both amounts in 2003 dollars).[9]

How does this compare to the foregone earnings from staying on at school for an additional year? Using data from the HILDA survey, we find that the average annual earnings of a high school dropout with 9 years of schooling, aged 15–17, were $5578.[10] One can then compare this amount with the discounted increase in future earnings, using various plausible estimates of the discount rate and the rate of return on schooling. Table 6

_____

[9] In 2005, 15% of Australian school students at left school before turning 16 years. In aggregate, about 25% left without completing Year 12, the highest grade.

[10] To avoid the problem that past-year annual earnings for dropouts may include months when they were in school, we use weekly earnings multiplied by 52. Note that our foregone earnings are calculated differently from Oreopolous (2003), who assumed that the dropout obtains full-time employment. If we do this, then our foregone earnings estimate rises to $12,095, which is still almost $4000 below the net present value of the return to schooling assuming a low rate of return (6%) and a high discount rate (7%).

Table 6
Discounted present value of an additional year of schooling (in 2003 dollars)

| Discount rate (%) | (1) | (2) | (3) | (4) |
| | Rate of return to schooling | | | Foregone earnings |
	6%	8%	10%	
0	$70,105	$93,473	$116,842	$5,578
3	$33,864	$45,152	$56,440	$5,578
5	$22,666	$30,222	$37,778	$5,578
7	$16,067	$21,423	$26,779	$5,578
Discount rate necessary for foregone earnings to exceed returns	16%	20%	23%	

Note: Projected income profiles are calculated by fitting a fourth-order polynomial to adults with 9 years of education in the HILDA data. Income increase from an additional year of schooling is calculated by increasing the annual income at each age from 16–64 by the given rate of return (6%, 8% or 10%), and discounting each year's income by the appropriate discount rate (0%, 3%, 5% or 7%). Foregone earnings are 52 times average weekly earnings for 15–17 year olds with 9 years of education.

shows the results of this exercise. Assuming a 10% rate of return on schooling, the expected value of an additional year of school is between $26,779 (7% discount rate) and $116,842 (0% discount rate). Even with a high discount rate (7%) and a low estimate of the rate of returns to schooling (6%), the lifetime gain to staying on at school is $16,067, which is nearly three times as large as expected foregone earnings. Indeed, we can even estimate for each rate of return to schooling what the discount rate would have to be in order to justify dropping out 1 year early. We find that the discount rate would have to be between 16% and 23% for the foregone earnings from not dropping out to exceed the additional earnings from staying on at school.

The above results suggest that Australian states that raised the school-leaving age in the 1960s substantially increased the lifetime earnings of individuals who grew up in states with higher school leaving ages. It also indicates that recently announced increases in the school-leaving age from 15 to 16 in Queensland and South Australia are likely to have a beneficial effect on individuals growing up in those states.[11]

Acknowledgements

[11]Queensland's reforms, to be implemented from 2006, also require that young people either be in full-time work or full-time study until they reach the age of 17.

References

Aakvik, A., Salvanes, K., & Vaage, K. (2003). *Measuring heterogeneity in the returns to education in Norway using educational reforms* IZA Discussion Paper 815. Bonn, Germany: IZA (Institute for the Study of Labor).

Acemoglu, D., & Angrist, J. D. (2000). How large are human capital externalities? Evidence from compulsory schooling laws. In *Proceedings of NBER Macroeconomics Annual 2000*. Cambridge, MA: NBER.

Angrist, J. D., & Krueger, A. B. (1991). Does compulsory school attendance affect schooling and earnings? *Quarterly Journal of Economics*, *106*, 979–1014.

Angrist, J. D., & Krueger, A. B. (1999). Empirical strategies in labor economics. In O. Ashenfelter, & D. Card (Eds.), *Handbook of labour economics*, vol. 3A. Holland: Elsevier Science (Chapter 23).

Ashenfelter, O., & Krueger, A. (1994). Estimates of the economic return to schooling from a new sample of twins. *American Economic Review*, *84*(5), 1157–1173.

Ashenfelter, O., & Rouse, C. (1998). Income, schooling, and ability: evidence from a new sample of identical twins. *Quarterly Journal of Economics, 113*, 253–284.

Becker, S., & Siebern-Thomas, F. (2001). *Returns to education in Germany: A variable treatment intensity approach*. EUI Working Paper ECO 2001/09.

Behrman, J. R., Rosenzweig, M. R., & Taubman, P. (1994). Endowments and the allocation of schooling in the family and in the marriage market: The twins experiment. *Journal of Political Economy, 102*, 1131–1174.

Black, S. E., Devereaux, P. J., & Salvanes, K. (2004). *Fast times at Ridgemont High? The effect of compulsory schooling laws on teenage births* NBER Working Paper 10911. Cambridge, MA: NBER.

Blackburn, M. L., & Neumark, D. (1995). Are OLS estimates of the return to schooling biased downward? Another look. *Review of Economics and Statistics, 77*(2), 217–230.

Bonjour, D., Cherkas, L. F., Hashel, J. E., Hawkes, D. D., & Spector, T. D. (2003). Returns to education: Evidence from UK twins. *American Economic Review, 93*(5), 1799–1812.

Bound, J., Jaeger, D. A., & Baker, R. M. (1995). Problems with instrumental variables estimation when the correlation between the instruments and the endogenous explanatory variable is weak. *Journal of the American Statistical Association, 90*(430), 443–450.

Bound, J., & Solon, G. (1999). Double trouble: On the value of twins-based estimation of the return to schooling. *Economics of Education Review, 18*(2), 169–182.

Card, D. (1995). Using geographic variation in college proximity to estimate the return to schooling. In L. N. Christodes, E. K. Grant, & R. Swidinsky (Eds.), *Aspects of labour market behaviour: Essays in honour of John Vanderkamp* (pp. 201–222). Toronto, Buffalo and London: University of Toronto Press.

Cruz, L. M., & Moreira, M. J. (2005). On the validity of econometric techniques with weak instruments: Inference on returns to education using compulsory school attendance laws. *Journal of Human Resources, 40*(2), 393–410.

Del Bono, E., & Galindo-Rueda, F. (2004). *Do a few months of compulsory schooling matter? The education and labour market impact of school leaving rules* IZA Discussion Paper 1233. Bonn, Germany: IZA (Institute for the Study of Labor).

Duflo, E. (2002). Schooling and labor market consequences of school construction in Indonesia: Evidence from an unusual policy experiment. *American Economic Review, 91*, 795–813.

Fertig, M., & Kluve, J. (2005). *The effect of age at school entry on educational attainment in Germany* IZA Discussion Paper 1507. Bonn, Germany: IZA (Institute for the Study of Labor).

Goldin, C., & Katz, L. (2003). *Mass secondary schooling and the state* NBER Working Paper 10075. Cambridge, MA: NBER.

Greene, W. H. (2002). *Econometric Analysis* (5th ed.). New York, NY: MacMillan.

Harmon, C., & Walker, I. (1995). Estimates of the economic return to schooling for the United Kingdom. *American Economic Review, 85*, 1278–1286.

Hogan, V., & Rigobon, R. (2002). *Using heteroskedasticity to estimate the returns to schooling* NBER working paper no. 9145. Cambridge, MA: NBER.

Ichino, A., & Winter-Ebmer, R. (2004). The long run educational costs of World War II: An application of local average treatment effect estimation. *Journal of Labor Economics, 22*, 57–86.

Isacsson, G. (1999). Estimates of the return to schooling in Sweden from a large sample of twins. *Labour Economics, 6*, 471–489.

Leigh, A., & Ryan, C. (2005). *Estimating returns to education: Three natural experiment techniques compared*. Australian National University Centre for Economic Policy Research Discussion Paper 493. Canberra, Australia: ANU.

Meghir, C., & Palme, M. (2003). *Ability, parental background and education policy: Empirical evidence from a social experiment*. Institute for Fiscal Studies Working Paper 03/05. London: IFS.

Miller, P. W., Mulvey, C., & Martin, N. (1995). What do twins studies reveal about the economic returns to education? A comparison of Australian and US findings. *American Economic Review, 85*(3), 586–599.

Miller, P. W., Mulvey, C., & Martin, N. (2006). The return to schooling: Estimates from a sample of young Australian twins. *Labour Economics, 13*(5), 571–587.

Milligan, K., Moretti, E., & Oreopoulos, P. (2003). *Does education improve citizenship? Evidence from the US and the UK*. NBER Working Paper 9584. Cambridge, MA: NBER.

Neumark, D. (1999). Biases in twin estimates of the return to schooling. *Economics of Education Review, 18*(2), 143–148.

Oreopolous, P. (2003). *Do dropouts drop out too soon? International evidence from changes in school-leaving laws*. NBER Working Paper 10155. Cambridge, MA: NBER.

Pischke, J., & von Wachter, T. (2004). *The effect of compulsory schooling in Germany, mimeo*. London School of Economics.

Plug, E. (2001). Season of birth, schooling and earnings. *Journal of Economic Psychology, 22*, 641–660.

Preston, A. (1997). Where are we now with human capital theory? *Economic Record, 73*, 51–78.

Rummery, S., Vella, F., & Verbeek, M. (1999). Estimating the returns to education for Australian youth via rank-order instrumental variables. *Labour Economics, 6*, 491–507.

Staiger, D., & Stock, J. H. (1997). Instrumental variables regressions with weak instruments. *Econometrica, 65*(3), 557–586.

Vella, F., & Verbeek, M. (1997). *Using rank order as an instrumental variable: an application to the return to schooling*. CES Discussion Paper 97.10, K.U. Leuven.

Watson, N. (Ed.). (2005). *HILDA user manual—Release 3.0*. Melbourne: Melbourne Institute of Applied Economic and Social Research, University of Melbourne.

Webbink, D., & van Wassenberg, J. (2004). *Born on the first of October: Estimating the returns to education using a school entry rule*. mimeo, University of Amsterdam.

Wooldridge, J. M. (2002). *Econometric analysis of cross section and panel data*. Cambridge, MA: MIT Press.