The Australian National University

Centre for Economic Policy Research

DISCUSSION PAPER

Estimating Returns to Education: Three Natural Experiment Techniques Compared*

Andrew Leigh and Chris Ryan

DISCUSSION PAPER NO. 493
August 2005

ISSN: 1442-8636
ISBN: 0 7315 3563 4

Economics Program, Research School of Social Science, The Australian National University, Australia
Address for Correspondence Andrew Leigh, Economics Program, Research School of Social Sciences, ACT 0200, Australia. Tel: (612) 6125 1374; Fax: (612) 6125 0182; Email: Andrew.Leigh@anu.edu.au

*This paper uses unconfidentialised unit record file from the Household, Income and Labour Dynamics in Australia (HILDA) survey. The HILDA Project was initiated and is funded by the Commonwealth Department of Family and Community Services (FaCS) and is managed by the Melbourne Institute of Applied Economic and Social Research (MIAESR) The findings and views reported in this paper, however, are those of the author and should not be attributed to either FaCS or the MIAESR. Since the data used in this paper are confidential, they cannot be shared with other researchers. However, the Stata do-file is available from the authors upon request.

Acknowledgements
Thanks to Jeff Borland, Deborah Cobb-Clark, Paul Miller and Justin Wolfers for valuable comments on an earlier draft.

CONTENTS

ABSTRACT

We compare three quasi-experimental approaches to estimating the returns to schooling in Australia: instrumenting schooling using month of birth, instrumenting schooling using changes in compulsory schooling laws, and comparing outcomes for twins. With annual pre-tax income as our measure of income, we find that the naïve (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. Finally, we review estimates from twins studies. While we estimate a higher return to education than previous studies, we believe that this is primarily due to the better measurement of income and schooling in our dataset. 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. Our estimates of the ability bias in OLS estimates of the rate of return to schooling range from 9% to 39%. Overall, our findings suggest the Australian rate of return to education, corrected for ability bias, is around 10%, which is similar to the rate in Britain, Canada, the Netherlands, Norway and the United States.

Keywords: returns to education, instrumental variables, compulsory schooling, twins, Australia

JEL Classifications: I21, I28, J24

1. Introduction

What is the economic return to an additional year of schooling? Over the past decade, a spate of papers has sought to answer this question for various developed countries. Simple ordinary least squares estimates are affected by two biases: ability bias may bias upwards or downwards the observed returns to schooling, while measurement error might bias the OLS returns downwards.

Among the approaches that have been proposed for addressing the problem of ability bias, three natural experiment techniques stand out.[1] 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 is 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 discussion in Angrist and Krueger, 1999 for example). In a heterogeneous returns world, 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

[1] Other researchers have used different instruments. Ichino and Winter-Ebmer (2004) use the effect of World War II on various cohorts of German students. Becker and Siebern-Thomas (1995) use the quality of schooling infrastructure across German states, and similarly Duflo (2002) uses the quality of school infrastructure across Indonesian provinces. Card (1995) uses geographic proximity to a US college. A number of non-experimental approaches have also been proposed. For example, Blackburn and Neumark (1995) attempt 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) use a rank-order instrumental variables estimator. Hogan and Rigobon (2002) use 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.

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 returns to schooling. Instead, we set out to compare the rate of returns 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 outlines the institutional context, and section 3 describes the data. Section 4 presents the OLS returns to schooling for several different measures of income. Section 5 presents estimates instrumenting schooling with month of birth, and section 6 shows results instrumenting schooling with changes in school leaving laws. Section 7 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. Institutional Background

Responsibility for regulation regarding schooling lies with the Australian states. Over the past one hundred years there have been three waves of changes to minimum school leaving legislation of relevance for our study (see Barcan, 1980 for a history of Australian education, including minimum schooling). First, in the early years of the twentieth century, Australian states adopted a minimum school leaving age of 14 years. Second, most states enacted legislation to increase the minimum leaving age during or just after World War II, but the legislation was only formally proclaimed in New South Wales (and the Australian Capital Territory, whose school system was administered by New South Wales until the mid-1970s), which increased the minimum leaving age to 15 years, and Tasmania, which increased it to 16 years. Finally, other states eventually increased the minimum leaving age to 15 years in the mid-1960s (South Australia and the

Northern Territory from 1963,Victoria from 1964, Queensland from 1965, and Western Australia from 1966).

As in other countries, not every child complied with the "compulsory" school leaving laws. New South Wales, the Australian Capital Territory and Tasmania, who were the first to raise their leaving ages, had systems of exemptions that allowed many students to leave school prior to the statutory limit. In New South Wales and Tasmania, about 6 per cent of school leavers left prior to their 15th birthdays in 1960, while in Tasmania, almost one half of them before their 16th birthday (Radford 1962). Another issue that reduces the relative impact of law-induced variation in school leaving ages between states is that New South Wales and the Australian Capital Territory added an additional year to high school for students who commenced it in 1962. Hence, educational attainment in New South Wales and the Australian Capital Territory increased at the same time as in other states, but the affected cohorts in NSW and the ACT were students who wished to complete high school, while the change in other states affected students who wished to drop out at the earliest opportunity. We do not code this NSW and ACT change in our data.

School commencement rules also differ between states and have changed through time. Grade cohorts essentially consist of those born in the same calendar year or financial year, depending on the state, with those who are too young at the cutoff date required to wait a full year before enrolling in school. Other states have multiple entry dates in a year, with those who are too young as at one date only required to wait for a few months before starting school.

Therefore, the critical 'cutoff' month differs between jurisdictions (and changed through time within jurisdictions). For most of those in the data used here, given the commencement rules that operated when they started school, the cutoff months were: the end of July in New South Wales and the Australian Capital Territory; end-June in Tasmania and Victoria; end-February in Queensland; and end December in Western Australia.[2]

[2] Changes through time in commencement rules were most dramatic in South Australia (and the Northern Territory, whose school system was administered by South Australia until the mid-1970s). South Australia introduced a system of continuous enrolment of recently-turned five year olds from the mid-1970s.

3. Data

For the purposes of the present study, we need a dataset that meets three criteria: first, it must contain measures of income and education; second, for the month of birth IV approach, it must identify month or quarter of birth; and third, for the changes in compulsory schooling laws IV approach, it must identify state and year of birth. In comparable studies for other countries, Census microdata files have been used, and we initially experimented with using the 1% samples of the Australian census (known as Confidentialised Unit Record Files). However, since the Australian census does not ask month or quarter of birth, it is not possible to use these data for the month of birth approach.[3] Moreover, age in the Census microdata files is collapsed into five-year bands, making it less precise for the changes in compulsory schooling laws approach.[4]

Given these drawbacks, we therefore chose instead to use the Household, Income and Labour Dynamics in Australia (HILDA) Survey, a household-based panel study containing about 12,000 respondents, which began in 2001. Our income and earnings data are primarily drawn from the third wave of the survey, conducted in 2003, and we include all respondents aged 25-64 with positive income who completed their schooling in Australia. We use a confidential version of the dataset, which allows us to identify respondents' month of birth. The one notable drawback of this dataset 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.

More information on variable construction may be found in the Data Appendix. Table 1 presents summary statistics.

[3] Apparently the 2006 Australian census will contain this question.
[4] The 1981 census microdata file is unusable for our purposes, since it does not identify the respondent's state/territory.

Table 1: Summary Statistics	Mean	SD	N
Log 3-year pre-tax income	11.338	0.872	6552
Log 3-year post-tax income	11.130	0.766	6552
Log annual pre-tax income	10.221	0.988	7211
Log annual post-tax income	10.019	0.875	7211
Log weekly earnings	6.528	0.704	4701
Log hourly wage	2.967	0.486	4673
Years of education	12.086	2.176	7211
Birth year	1960.557	10.885	7211
Female	0.516	0.500	7211
Married	0.727	0.446	7211
Female*Married	0.376	0.484	7211
Working full-time	0.545	0.498	7211

Note: Sample is restricted to adults aged 25-64, with positive annual income. All figures are weighted using person-weights, except three-year income, which is weighted using panel weights.

4. Naïve Returns to Schooling

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

$$Ln(Y)_i = \alpha + \beta Educ_i + \gamma Z_i + \varepsilon_i \qquad (1)$$

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 2: 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.

Table 2 shows the returns to education from an extra year of school in an OLS specification. All specifications are restricted to those aged 25-64, who are most likely to have completed schooling, and not yet be retired. The HILDA dataset allows us to test the returns to education using a variety of different measures of income: total income over a three 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. In this specification, the returns to an additional year of education are 13% for pre-tax three-year income and pre-tax annual income, 11% for post-tax annual income or post-tax three-year income, 10% for weekly earnings, and 8% for hourly earnings.

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 orthogonal 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 three-year income and pre-tax annual income, 9% for post-tax three-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, 2005) 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, Mulvey and Martin (1995), earnings are imputed as the average income in the respondent's two-digit occupation, while in Miller, Mulvey and Martin (2005), 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]

In the estimates that follow, we use as our measure of income the respondent's annual income, and we do not control for the respondent's marital status or whether s/he worked full-time. We chose this specification on the basis that it takes into account both participation and wage effects, and that it most closely accords with the existing

[5] Miller, Mulvey and Martin (2005) note that income in their survey was reported using a prompt card that contained weekly, fortnightly and annual amounts. They use 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 respondents. The twins sample in Miller, Mulvey and Martin (2005) 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%.

international literature (eg. Angrist and Krueger 1991; Ashenfelter and Krueger 1994; Oreopoulos 2003).

5. 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: it must be correlated with years of schooling (IV condition 1), and it must be uncorrelated with ability (IV condition 2). In this section, we use as an instrument an individual's month of birth, an approach first implemented in Angrist and Krueger (1991), who conclude that the rate of return to schooling in the US is 9%. Using a similar methodology, Webbink and van Wassenberg (2004) find a rate of return to schooling of 8% in the Netherlands (though Plug 2001 finds lower estimates, and argues that the Dutch effect operates through relative position, not total schooling). In the British context, Del Bono and Galindo-Rueda (2004) use 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, Victoria and Western Australia. Table 3 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 3: 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
Victoria	1955-78	30 June
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 dataset 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 six month "window", comprising those born three months before and three months after the cutoff date. However, the instrument has a different impact on years of schooling for an individual born one month before the cutoff date than for an individual born three 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. To the extent that this affects schooling, it may have a direct impact on the returns to education. 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:

$$Educ_i = \alpha + \beta(\textit{Months Before Cutoff})_i + \gamma Z_i + \pi(\textit{Relative Position})_i + \varepsilon_i \qquad (2)$$

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), $^{1}/_{11}$ for a student born two 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:

$$\text{Ln}(Y)_i = \delta + \zeta \hat{Educ}_i + \eta Z_i + \tau(\textit{Relative Position})_i + \upsilon_i \qquad (3)$$

The first column of Table 4 shows the OLS estimate, using the same methodology as in Table 2, Panel A, Column 3, but with state fixed effects, and restricting the sample to those born within three months of the cutoff dates listed in Table 3. Reassuringly, this OLS estimate is almost identical to the corresponding estimate in Table 2.

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

Table 4: Instrumenting Schooling with Month of Birth **Dependent variable: Log annual income**			
	(1)	**(2)**	**(3)**
	OLS	**IV Birthmonth**	**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 P=0.6605	554.89 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 3, 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.

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:

$$Educ_i = \alpha + \beta(Months\ Before\ Cutoff{*}Birthyear)_i + \gamma Z_i + \pi(Relative\ Position)_i + \varepsilon_i \quad (4)$$

$$\mathrm{Ln}(Y)_i = \delta + \zeta \widehat{Educ}_i + \eta Z_i + \tau(Relative\ Position)_i + \upsilon_i \quad (5)$$

Column 3 of Table 4 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 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 and Baker 1995; Staiger and Stock 1997; Cruz and Moreira 2005). 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. Comparing this coefficient to the OLS estimate suggests that ability bias accounts for about one-third of the total return to schooling.

6. 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 (IV I), and if these increases are uncorrelated with the ability distribution of residents in that state (IV II). If compulsory schooling laws are not enforced by state education officials, then this will violate IV I, 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 IV II.

Do changes in school leaving laws tend to increase educational attainment? Most studies have concluded that there is an effect in the US (Angrist and Acemoglu 2000; Oreopoulos), though Goldin and Katz (2003) warn that the expansion of state compulsory schooling and (child labor laws) in the period 1910-39 accounted for, at best, 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 and Walker 1995; Oreopoulos 2003), Canada (Oreopoulos 2003), Norway (Aakvik, Salvanes and Vaage 2003) and Sweden (Meghir and 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 1930-60, though Fertig and Kluve (2005) found that changes in the school starting ages had no impact on total schooling for those born in 1960-74. 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, Angrist and Acemoglu (2000) estimate a private rate of return to schooling of 10%. Oreopoulos (2003) uses changes in school leaving laws in states/provinces in three countries. His central estimates are that an additional year of schooling boosts earnings by 16% in Britain, 8% in Canada and 13% in the US.[7] In Norway, Aakvik, Salvanes and Vaage (2003) report a return to schooling of 10%. Looking at outcomes other than earnings, Black, Devereux and Salvanes (2004) also show that law changes in Norway and the US had the effect of decreasing the rate of teenage childbearing; while Milligan, Moretti and Oreopoulos (2003) show that law changes in Britain and the US increased political interest and involvement.

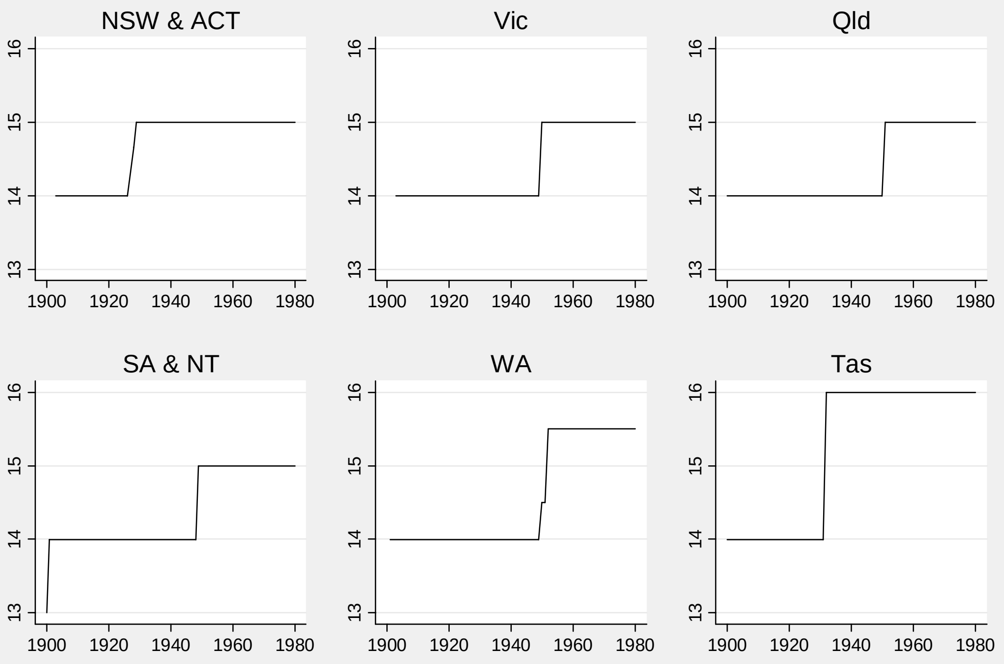
Much smaller estimates of the economic returns to schooling have been found in Germany using this methodology. Pischke and Von Wachter (2004) found that the introduction of a compulsory ninth grade boosted school attainment by 0.17 to 0.6 years, but that this rise in educational attainment did not have 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 *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. Figure 1 shows the compulsory school leaving age by birth cohort for each of the Australian states and territories over the relevant period. Note that our coding is based on

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

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.

Figure 1: Compulsory School Leaving Age by Birth Year



How much of an impact did raising compulsory schooling laws have on educational attainment? Panel A of Table 5 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, plus state and birth year fixed effects. We find that a one-year increase in the leaving age raises educational attainment about 3/10ths of a year.

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

$$Educ_i = \alpha + \beta(\textit{Compulsory School Law})_i + \gamma Z_i + \varepsilon_i \qquad (6)$$

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:

$$Educ_i = \alpha + \beta(\textit{Compulsory School Law*Birthyear})_i + \gamma Z_i + \varepsilon_i \qquad (7)$$

In both cases, the second stage equation is:

$$\mathrm{Ln}(Y)_i = \delta + \zeta \hat{Educ}_i + \eta Z_i + \upsilon_i \qquad (8)$$

Table 5: 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 LeavingAge**	**IV LeavingAge* 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 P=0.0002	8.1e+11 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 5 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.

7. 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, Mulvey and Martin 1995, 2005), Sweden (Isacsson 1999) and the United Kingdom (Bonjour et al 2003). The main problems with the twins approach are that between-twin differences in schooling may not be random, but rather be endogenous 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 (Neumark 1999, Bound and Solon 1999).

As we have discussed in section 4, Miller, Mulvey and Martin (1995, 2005) observe a lower OLS rate of return to education in their Australian twins samples (6.0% and 6.4%) 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 6 compares our OLS estimator (from section 4), month of birth IV estimator (from section 5) and changes in school leaving laws IV estimator (from section 6) with the identical twin estimates presented in Miller, Mulvey and Martin (2005). 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 (the difference between the naïve and causal rates of return). The month of birth IV method 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 indicates a higher rate of return (12%), implying that the OLS estimator is essentially unbiased. The identical twins estimator is between these two results, with an implied rate of return of 5%, which implies that the OLS schooling estimator has only a 10% ability bias, while the IV estimator has an abilility bias of 28%.

Table 6: Three Estimators Compared
Dependent variable: Log annual income

	Leigh & Ryan			Miller, Mulvey & Martin		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	OLS	**IV Birthmonth * Birthyear**	**IV LeavingAge * Birthyear**	**OLS**	**IV**	**IV with twin-pair fixed effects**
Years of education	0.130***	0.079**	0.118***	0.060***	0.075***	0.054**
	[0.005]	[0.032]	[0.035]	(0.005)	(0.006)	(0.023)
Female	-0.626***	-0.602***	-0.629***	-0.169***	-0.179***	-
	[0.022]	[0.057]	[0.024]	(0.034)	(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)

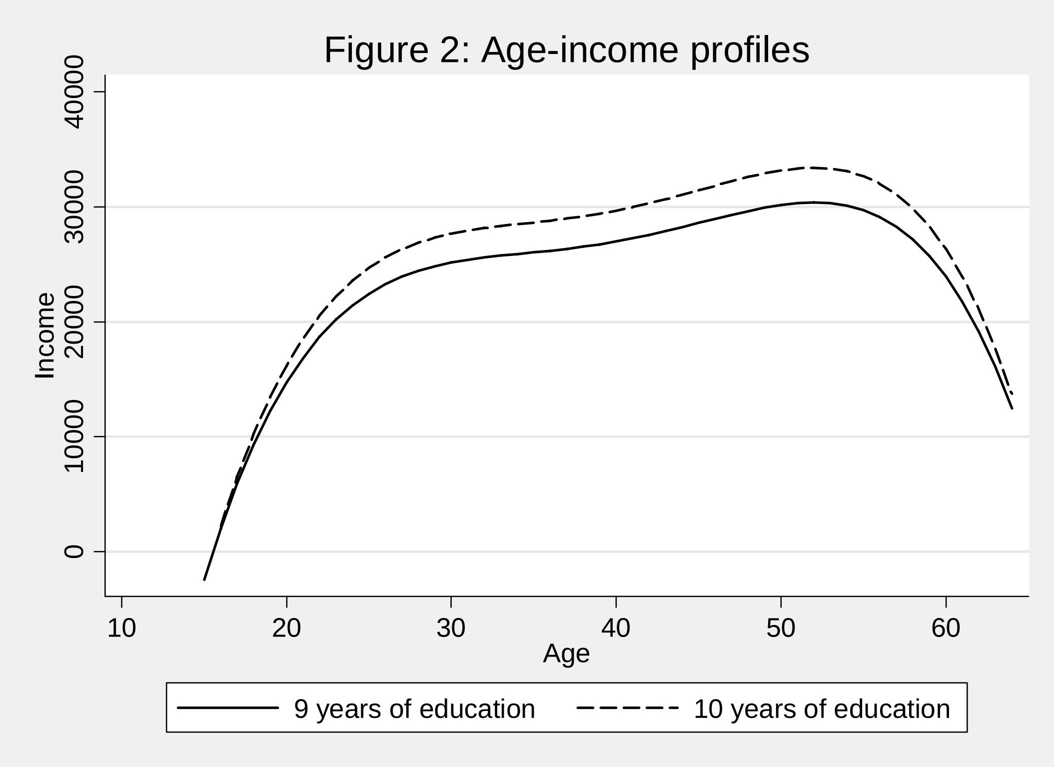
Note: In columns 1 and 3, sample is restricted to those aged 25-64 with positive annual income. In column 2, sample is restricted to those aged 25-64 with positive weekly earnings, in the states and years listed in Table 3, born within 3 months of the cutoff date for school entry. Column 2 also includes 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 (2005, 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 is the IV estimate divided by the OLS estimate in column 1; in column 6 it is the fixed effects IV estimate divided by the estimates in columns 4 and 5.

8. 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 evaluating the effect of raising school leaving ages, the other two approaches 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 4, 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, Figure 4 follows Oreopolous (2003), in comparing 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 (we choose 10% since it is midway between our two instrumental variable estimates). 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).



Note: Age-income profile is constructed by fitting a fourth-order polynomial to the income of those with 9 years of education, aged 15-64, using HILDA data. The profile for those with 10 years of education assumes zero income at age 15, and annual income 10% higher than those with 9 years of education in ages 16-64.

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 $5,578.[8] Table 7 compares this amount with the discounted increase in future earnings, using annual discount rates of 0%, 3%, 5% and 7%, and rates of return to schooling of 6%, 8% and 10%. Assuming a 10% rate of return on schooling, the expected value of an additional year of school is between $26,779 and $116,842. 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. In the last row of Table 7, we estimate for each rate of return to schooling what the discount rate would have to be in order to justify dropping out one year early. We find

[8] 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 assumes 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%).

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.

Table 7: Discounted Present Value of an Additional Year of Schooling (in 2003 dollars)

	(1)	(2)	(3)	(4)
		Rate of return to schooling		**Foregone earnings**
Discount rate	**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.

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.[9]

[9] 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.

Data Appendix

Educational attainment

We use two variables to construct years of schooling. The first is highest years of school completed (CEDHISTS). 12, 11, 10, 9 and 8 years are coded as in the survey, while 7 or fewer years is coded as 8 years (since it is only separately identified for respondents in certain states). The other education variable is highest educational level achieved (CEDHIGH). Where a respondent holds a post-school qualification, his or her years of schooling are coded as postgraduate degree=17, graduate diploma/certificate=16, bachelor's degree=15, advanced diploma/diploma/certificate=12.

Income and earnings

Annual pre-tax income: Previous financial year gross income after imputation, including market income, private transfers, Australian and foreign pensions and benefits, family tax transfers and child care benefits. Windfall (irregular) income is excluded (CTIFEFP)
Annual post-tax income: Financial year disposable income, including family tax benefits and child care benefit, but excluding windfall income (CTIFDIP).
Three year pre-tax income: The sum of annual pre-tax income over the three waves (ATIFEFP+BTIFEFP+CTIFEFP). This is coded as missing if income in any wave is missing.
Three year post-tax income: The sum of annual post-tax income over the three waves (ATIFDIP+BTIFDIP+CTIFDIP). This is coded as missing if income in any wave is missing.
Weekly earnings: Current weekly gross wages & salary from all jobs (CWSCE).
Hourly earnings: Weekly earnings divided by combined hours per week currently worked in all jobs (CJBHRUC).

Other controls

Married: Married variable is CMRCURR. Respondents in de-facto relationships are coded as married.
Full-time: Respondents are coded as full-time workers if their total weekly hours in all jobs (CJBHRUC) is equal to or greater than 35.

Weights

All estimates are weighted using enumerated person population weights (CHHWTE), except for the estimates using three-year income, which are weighted using the enumerated person longitudinal weight (CLNWTE).

Sample restrictions

All estimates are restricted to respondents aged 25-64 in 2003 (born 1939-78). Estimates in sections 5 and 6 of the paper are further restricted to those who completed their last year of schooling in Australia (identified from variable CEDCLY), and have positive annual income.

References

Aakvik, A., Salvanes, K. and Vaage, K., 2004. 'Measuring Heterogeneity in the Returns to Education in Norway Using Educational Reforms', *mimeo*

Acemoglu, D. and Angrist, J.D., 2000. 'How Large Are Human Capital Externalities? Evidence from Compulsory Schooling Laws', *NBER Macroeconomics Annual 2000*

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

Angrist, J.D. and Krueger, A.B., 1999. 'Empirical strategies in labor economics', Chapter 23 in Ashenfelter, O. and Card, D. (eds), *Handbook of Labour Economics*, Volume 3A, Holland: Elsevier Science

Ashenfelter, O. and Krueger, A., 1994. 'Estimates of the Economic Return to Schooling from a New Sample of Twins', *American Economic Review*, 84(5): 1157-73

Ashenfelter, O. and Rouse, C., 1998. 'Income, Schooling, and Ability: Evidence from a New Sample of Identical Twins', *Quarterly Journal of Economics*, 113: 253-284

Barcan, A., 1980. *A History of Australian Education*, Oxford University Press, Melbourne

Becker, S. and 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. and 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., and Salvanes, K., 2004. 'Fast Times at Ridgemont High? The Effect of Compulsory Schooling Laws on Teenage Births', NBER Working Paper 10911, NBER: Cambridge, MA

Bonjour, D., Cherkas, L.F., Hashel, J.E., Hawkes, D.D. and Spector, T.D., 2003. 'Returns to Education: Evidence from U.K. Twins', *American Economic Review*, 93(5): 1799-1812

Bound, J., Jaeger, D.A. and 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

Card, D., 1995. 'Using Geographic Variation in College Proximity to Estimate the Return to Schooling' in *Aspects of labour market behaviour: Essays in honour of John Vanderkamp*, University of Toronto Press, Toronto, Buffalo and London, 201-222

Cruz, L.M., and 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. and Galindo-Rueda, F., 2004. 'Do a few months of compulsory schooling matter? The education and labour market impact of school leaving rules', *mimeo*, Centre for Labor Economics, University of California

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. and Kluve, J., 2005. 'The Effect of Age at School Entry on Educational Attainment in Germany', *mimeo*, RWI-Essen and IZA-Bonn

Goldin, C. and Katz, L., 2003. 'Mass Secondary Schooling and the State', NBER Working Paper No. 10075, NBER: Cambridge, MA

Harmon, C. and Walker, I., 1995. 'Estimates of the Economic Return to Schooling for the United Kingdom', *American Economic Review*, 85: 1278-1286

Hogan, V. and Rigobon, R., 2002. Using heteroskedasticity to estimate the returns to schooling, NBER Working Paper No. 9145, NBER: Cambridge, MA

Ichino, A. and 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

Meghir, C. and Palme, M., 2003. 'Ability, Parental Background and Education Policy: Empirical Evidence from a Social, Experiment', Institute for Fiscal Studies Working Paper 03/05, IFS: London

Miller, P.W., Mulvey, C. and 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. and Martin, N., 2005. 'The Return to Schooling: Estimates From a Sample of Young Australian Twins', *Labour Economics*, forthcoming

Milligan, K., Moretti, E. and Oreopoulos, P., 2003. 'Does Education Improve Citizenship? Evidence from the U.S. and the U.K.', NBER Working Paper 9584, NBER: Cambridge, MA

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, NBER, Cambridge, MA

Organisation for Economic Cooperation and Development, 2003. *Education at a Glance*, OECD: Paris

Pischke, J. and 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

Radford, W.C., 1962. *School Leavers in Australia*, Australian Council for Educational Research, Educational Research Series No. 75, ACER Press, Melbourne

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

Staiger, D. and Stock, J.H., 1997. 'Instrumental Variables Regressions with Weak Instruments' *Econometrica*, 65(3): 557-586

Vella, F. and 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

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