DO COMPULSORY SCHOOL ATTENDANCE LAWS ALONE EXPLAIN THE ASSOCIATION BETWEEN QUARTER OF BIRTH AND EARNINGS?

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ABSTRACT

Economists have been reluctant to interpret as purely causal the relationship between educational attainment and earnings. In an influential paper in which they use quarter of birth as an instrument for educational attainment in wage equations, Angrist and Krueger interpret their estimates as the causal impact of education on earnings. To support this interpretation, they argue that compulsory school attendance laws alone *account for the association between quarter of birth and earnings. In this work we present new evidence suggesting that this interpretation may not be well-founded. We document an association between quarter of birth and earnings in cohorts that were not bound by compulsory school attendance laws. Moreover, we find that the association between quarter of birth and educational attainment was weaker in more recently-born cohorts while no similar pattern existed in the association between quarter of birth and earnings. Our results call into question the validity of any causal inferences based on Angrist and Krueger's estimates regarding the effect of education on earnings.*

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The association between educational attainment and earnings is strong and persistent. Economists and other social scientists have been reluctant, however, to interpret as purely causal the relationship between education and earnings. Educational attainment is not randomly distributed across the population. Rather, individuals who do well in school or are from privileged backgrounds are much more likely to continue their education. While it is possible to write down economic models in which the association between education and earnings understates the causal impact of education on earnings (Griliches, 1977; Willis, 1986), most economists have presumed that the observed association actually exaggerates the causal effect of educational attainment on earnings.

Even though the endogeneity of educational attainment has long been recognized, plausible instruments for education that would allow for estimation of the causal effect of education on earnings have been difficult to find. In a recent provocative and influential paper, 'Does Compulsory School Attendance Affect Schooling and Earnings?' Angrist and Krueger (1991, henceforth AK) present results in which they use information on quarter of birth to form instruments for educational attainment. AK's instrumental variables (IV) estimates tend to be close to and often larger than their ordinary least squares (OLS) estimates. They interpret these results as "cast[ing] doubt on the importance of omitted variables bias in OLS estimates of the return to education . . . ."

This counter-intuitive interpretation has far-reaching implications and deserves careful scrutiny. For quarter of birth to be a valid instrument for education in wage equations, its effect on educational attainment must be the *only* reason for its association with earnings. AK argue that the association between quarter of birth and education is due exclusively to compulsory school attendance laws. Since it seems unlikely that compulsory school attendance laws would induce any direct association between quarter of birth and earnings, AK's IV estimates would seem, *prima facie*, to be credible.

In this chapter we present evidence that suggests that compulsory school attendance laws are *not* the only reason for an association between quarter of birth and educational attainment or earnings. As such, this evidence calls into question the validity of any causal inferences based on estimates which employ information on quarter of birth to construct instruments for educational attainment in earnings equations.[1]

In what follows, we first review AK's framework. In Section 2 we present evidence suggesting that the associations between quarter of birth and both educational attainment and earnings are too strong to be explained solely by the compulsory school attendance law mechanisms detailed by AK. In Section 3

we present results that suggest that an association between quarter of birth and labor market outcomes predated the enactment of compulsory school attendance laws in the U.S. In Section 4, we review the large literature documenting associations between quarter of birth and a host of factors that could plausibly affect earnings. Finally, in Section 5 we review the validity of arguments that Angrist and Krueger use to support the use of quarter of birth to form instruments for educational attainment.

1. COMPULSORY SCHOOL ATTENDANCE LAWS, QUARTER OF BIRTH, AND EDUCATIONAL ATTAINMENT

AK find significant associations between quarter of birth, educational attainment, and earnings for cohorts of men born during the 1920s, 1930s and 1940s. In their data, men born during the first quarter of the year obtain less education and have lower earnings than those born during the remainder of the year. AK argue that the association between quarter of birth and education is due to compulsory school attendance laws. The typical compulsory school attendance law requires students to start first grade in the autumn of the calendar year in which they turn 6 and to stay in school until their 16th birthday. Individuals born in the early months of the year will usually enter first grade when they are near seven years of age and will turn 16 in the middle of tenth grade. Individuals born in the third or fourth quarter will typically start school either just before or just after their sixth birthday and will finish tenth grade before they turn 16. Thus, the law induces those born late in the year to obtain somewhat more schooling than those born early in the year.

AK present several tabulations in support of their assertion that compulsory school attendance laws are responsible for the relationship between quarter of birth and educational attainment. First, the observed relationship between educational attainment and season of birth is weaker in more recent cohorts that were less likely to have been constrained by the law. Second, they find a weaker relationship between quarter of birth and education for individuals with higher levels of education. Third, among individuals who recently turned 16, enrollment rates are higher in states that require students to remain in school through their 17th or 18th birthday than in states that allow 16-year-olds to leave. Each of these findings is consistent with the argument that compulsory school attendance laws are responsible for an association between quarter of birth and educational attainment.

AK leave us with little doubt that compulsory school attendance laws are at least partially responsible for the observed correlation between quarter of birth

and educational attainment. For AK's IV estimates to be valid, however, it is crucial that there be no direct association between quarter of birth and earnings. It is important, therefore, that compulsory school attendance laws fully account for the association between quarter of birth and earnings, if we are to interpret AK's estimates as the causal effect of education on earnings.

2. THE ASSOCIATION BETWEEN QUARTER OF BIRTH, EDUCATION, AND EARNINGS

Quarter of Birth and Completion of the Highest Grade Attended

Compulsory school attendance laws typically require students born during the summer to finish tenth grade, while permitting those born during the winter to drop out in the middle of the tenth grade. If compulsory schooling laws were the only reason for the relationship between quarter of birth and educational attainment, we would expect that most of the association would be accounted for by a larger fraction of individuals born in the summer completing the highest grade they attended. Moreover, we would predict an association between quarter of birth and educational attainment only for individuals with a high school education or less, but would not expect any association between season of birth and educational attainment above the high school level.[2]

In Table 1, we use the same samples as AK and report the fraction of men born during the 1920s, 1930s and 1940s who did and did not complete the

Table 1. Completion of Highest Grade Attended by Quarter of Birth

Quarter of Birth	Men Born 1920–29		Men Born 1930–39		Men Born 1940–49	
	Comp.	Not Comp.	Comp.	Not Comp.	Comp.	Not Comp.
Jan.–Mar.	0.812	0.177	0.835	0.139	0.806	0.139
Apr.–June	0.814	0.174	0.837	0.137	0.805	0.138
July–Sep.	0.816	0.171	0.838	0.136	0.806	0.136
Oct.–Dec.	0.814	0.174	0.837	0.136	0.801	0.139
χ^2	14.439 [0.108]		14.350 [0.110]		42.538 [0.000]	

Source: 1920–29: 1970 Census, 1930–39 and 1940–49: 1980 Census.
Note: Fraction never attended and currently enrolled not shown. χ^2(df = 8) test is for independence of completion status and quarter of birth. p values in brackets. Sample sizes are 245,299 for men born 1920–29; 329,509 for men born 1930–39; and 486,926 for men born 1940–49

highest grade of school they attended.[3] The residual category includes both respondents who reported that they were attending school at the time of the Census as well as a negligible portion that reported never having attended school. The χ^2 tests reported are for the full 4×4 table of quarter of birth $\times$ each of the four responses to the grade completion question. The association between grade completion rates and quarter of birth is very weak. For the cohorts born during the 1920s and 1930s, individuals born during the winter months were slightly less likely to finish the highest grade of school they attended, but the magnitude of the difference is trivial and accounts for little of the difference in the total number of years of education completed. For the cohorts born during the 1940s, men born in the winter quarter were actually more likely to have finished the highest grade they attended.

Men born during the 1940s would have been in high school during the late 1950s and early 1960s. It is possible, therefore, to use the 1960 Census to directly examine the effect of compulsory school attendance laws on the high school enrollment patterns for this cohort. Restricting attention to men born in 1944 (who would turn 16 some time during 1960) and residing in states that required students to attend school through their 16th birthday, we calculate that the fraction of those who had already turned 16 who were still enrolled as of the April 1, 1960 Census date was 0.894. For those who had yet to turn 16, the fraction was higher: 0.930. Together, these numbers suggest that, of those who turned 16 during the school year, roughly 3.5% (standard error of 0.7) dropped out without finishing the grade they were in. This small percentage is too small to account for the difference (approximately 0.07 years) in educational attainment between those born during the first and the third quarters for men born during the 1940s.[4]

In contrast to what we would expect if compulsory school attendance laws fully accounted for the association between quarter of birth and educational attainment, there also appears to be an association between quarter of birth and post-secondary educational attainment. AK report that those born in the first quarter of the year are between 0.4 and 0.7% less likely to finish college than those born later in the year, while Angrist & Imbens (1995) show that the entire distribution of educational attainment is shifted downward for those born in the first quarter.[5]

Beyond these considerations, it is not clear that the pattern of educational attainment by quarter of birth is what we would expect if compulsory schooling laws were the only cause for the association between quarter of birth and educational attainment. These laws *would* lead us to expect that individuals born in the first quarter of the year would obtain a higher level of education than those born during the third quarter. At least as of 1955, however, most

states required children to be 6 by the October of the year they started first grade (Angrist & Krueger, 1992). We might therefore expect the educational attainment of those born in the fourth quarter to be lower than those born in the third quarter. For at least the cohorts analyzed by AK the reverse seems to be true (see Table 2).

While the evidence that AK present regarding the association between quarter of birth and educational attainment supports the notion that compulsory school attendance laws contribute to the association between quarter of birth and educational attainment, the evidence presented above casts doubt on AK's maintained assumption that compulsory schooling laws are the *only* reason for

Table 2. Reduced Form: Quarter of Birth Effects on Log Weekly Earnings and Educational Attainment

	Educational Attainment			Log Weekly Earnings		
Qtr of Birth	Men Born 1920–29	Men Born 1930–39	Men Born 1940–49	Men Born 1920–29	Men Born 1930–39	Men Born 1940–49
Jan.–Mar.	–0.062	–0.049	–0.028	–0.004	–0.007	–0.007
	(0.011)	(0.010)	(0.007)	(0.002)	(0.002)	(0.002)
Apr.–June	–0.047	–0.015	–0.000	–0.003	–0.004	–0.001
	(0.011)	(0.010)	(0.007)	(0.002)	(0.002)	(0.002)
July–Sep.	0.049	0.020	0.005	0.004	0.005	0.006
	(0.011)	(0.009)	(0.007)	(0.002)	(0.002)	(0.002)
Oct.–Dec.	0.060	0.045	0.023	0.002	0.005	0.002
	(0.011)	(0.010)	(0.007)	(0.002)	(0.002)	(0.002)
Q3–Q1	0.111	0.069	0.033	0.008	0.012	0.013
	(0.018)	(0.016)	(0.012)	(0.003)	(0.003)	(0.003)
$\Sigma\lvert Q_i\rvert$	0.218	0.129	0.056	0.013	0.021	0.016
	(0.024)	(0.020)	(0.015)	(0.004)	(0.004)	(0.003)
F	23.660	13.486	6.256	2.436	7.154	9.661
	[0.000]	[0.000]	[0.000]	[0.063]	[0.000]	[0.000]
N	245,299	329,509	486,926	245,299	329,509	486,926

Source: 1920–29: 1970 Census, 1930–39 and 1940–49: 1980 Census.

Note: All models estimated with OLS. Standard errors in parentheses, p values in brackets. Coefficients on quarter of birth dummy variables are restricted to sum to zero. F is for joint significance of quarter of birth effects. Age is measured in quarter years. All models include age, age squared, race (1 = black), SMSA (1 = central city), married (1 = married, living with spouse), and 8 regional dummies as control variables.

this relationship. As we will discuss below, quarter of birth is associated with a host of factors, many of which could plausibly affect educational attainment. Unlike compulsory school attendance laws, many of these factors could also affect earnings. Thus, the fact that compulsory school attendance laws would not seem capable of fully explaining the association between quarter of birth and education casts some doubt on the validity of AK's IV estimates of the effect of education on earnings.

Quarter of Birth and Earnings

As we noted earlier, AK find that the association between quarter of birth and educational attainment is substantially weaker for cohorts born more recently. If compulsory school attendance laws are the only cause for the association between quarter of birth and earnings, we would expect a similar pattern for earnings. The opposite appears to be the case, however.

Table 2 reports estimates of the effect of quarter of birth on educational attainment and the logarithm of weekly earnings for AK's samples.[6] The sum of the quarter of birth coefficients has been constrained to sum to zero. Because there is an association between age and educational attainment and earnings and because, in a cross section, quarter of birth and age are related by definition, it is important to carefully control for age when estimating the effect of quarter of birth on educational attainment or earnings. It seems natural to assume that earnings are a continuous and smooth function of age and therefore to model this effect using polynomials of age.[7] On the other hand, the effects of compulsory school attendance laws will induce an association between age and earnings that includes discrete jumps in earnings between those born in the fourth quarter of one year and those born in the first quarter of the next. It is, in fact, differences in the pattern of expected impacts that allow for separate identification of the effect of age and quarter of birth on education and earnings.[8]

In addition to the quarter of birth estimates themselves, we report two measures of the overall strength of the association between quarter of birth and education and earnings: the difference between the coefficients on the third and first quarters and the sum of the absolute values of the coefficients on all four quarters of birth indicators.

Consistent with the results reported by AK, Table 2 shows that the strength of the association between quarter of birth and educational attainment declines considerably between cohorts born in the 1920s and those born in the 1940s. In contrast, the strength of the association between quarter of birth and earnings is actually somewhat larger for more recently-born cohorts – though here the

differences are not statistically significant at conventional levels. Figure 1 shows the third quarter-first quarter difference for both educational attainment and log weekly earnings, and makes clear the change in the strength of these

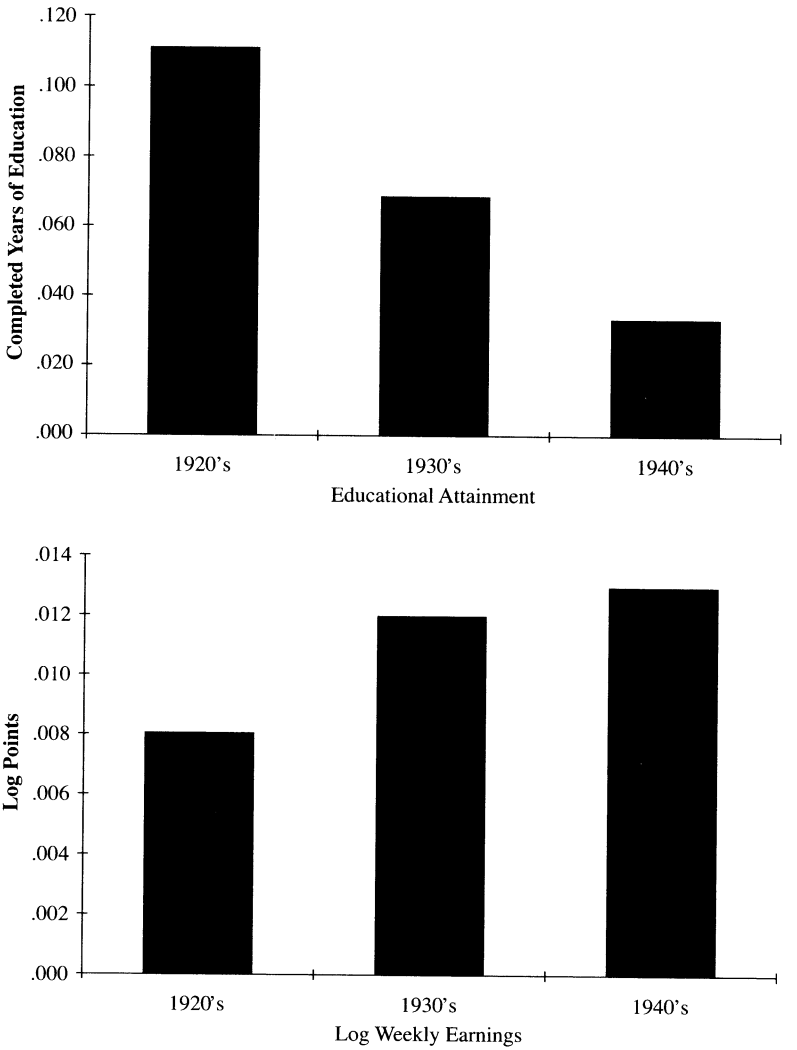

Fig. 1. Third Quarter-First Quarter Reduced Form Effect of Quarter of Birth on Educational Attainment and Log Weekly Earnings by Birth Decade. Source: Table 2.

relationships across AK's three birth cohorts. The relative constancy or even strengthening of the association between quarter of birth and earnings suggests that factors other than compulsory school attendance laws must explain a significant part of the association between quarter of birth and earnings. Moreover, the fact that the strength of the association between quarter of birth and earnings increased while the strength of the association between quarter of birth and educational attainment declined is indicative of a possible direct association between quarter of birth and earnings.[9]

Instrumental Variables Estimates of the Returns to Education

The rise in the strength of the association between quarter of birth and education relative to that between quarter of birth and earnings has clear implications for instrumental variable estimates of the returns to education. In particular, we would expect IV estimates of the effect of education on earnings to be substantially larger for the more recent cohorts. Table 3 explores this issue using AK's three samples. Columns (1), (3) and (6) contain OLS estimates while the remaining columns present results from a variety of IV specifications that include both different age controls and different instruments for education. Control variables are the same as in Table 2. As indicated, columns (3), (5), (6), and (8) replicate estimates reported by AK's in columns (5) through (8), Tables IV through VI. In addition to the coefficients on educational attainment and their standard errors, we also report the partial R^2 of the excluded instruments and F statistic from the test of statistical significance of the excluded instruments, both from the first stage regression, for each IV specification.

The simplest and perhaps most plausible specification is shown in column (2). This model controls for age using a simple quadratic and uses three quarter of birth dummies as the excluded instruments. While the OLS estimates for any of the specifications remain quite stable across cohorts, the IV estimates vary substantially, more than tripling between the cohort born in the 1920s and that born in the 1940s. This dramatic increase in the estimates can be accounted for by the substantial weakening of the relationship between quarter of birth and educational attainment while the strength of the relationship between earnings and quarter of birth increased. The IV estimates from this model for cohorts born during the 1930s and 1940s seem implausibly large.

One possible explanation for these results is the increasing selectivity, from the 1920s to the 1940s, of the population induced by compulsory schooling laws to continue their education. For example, while the *average* return to a year of education may have been no higher for the cohorts born later, it is possible that the returns to education for those affected by compulsory

attendance laws were substantially higher in those cohorts.[11] For this to be the case requires that there have been tremendous variation in the effect of education on earnings *and* that the effect of education on earnings would have

Table 3. Estimated Return to Education

	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) IV	(6) OLS	(7) IV	(8) IV
				Men Born 1920–29				
AK Table (Column)			IV(5)		IV(6)	IV(7)		IV(8)
Coefficient	**0.070**	**0.059**	**0.070**	**0.061**	**0.066**	**0.070**	**0.090**	**0.089**
	(0.000)	(0.021)	(0.000)	(0.017)	(0.015)	(0.000)	(0.062)	(0.033)
F		23.660		37.078	4.415		4.171	1.043
Partial R^2 ($\times$ 100)		.029		.045	0.054		0.003	0.012
				Men Born 1930–39				
AK Table (Column)			V(5)		V(6)	V(7)		V(8)
Coefficient	**0.063**	**0.142**	**0.063**	**0.099**	**0.081**	**0.063**	**0.677**	**0.060**
	(0.000)	(0.033)	(0.000)	(0.021)	(0.016)	(0.000)	(1.536)	(0.029)
F		13.486		30.526	4.747		0.088	1.613
Partial R^2 ($\times$ 100)		.012		0.028	0.043		0.000	0.014
				Men Born 1940–49				
AK Table (Column)			VI(5)		VI(6)	VI(7)		VI(8)
Coefficient	**0.052**	**0.201**	**0.052**	**-.073**	**0.039**	**0.052**	**-.116**	**0.078**
	(0.000)	(0.059)	(0.000)	(0.027)	(0.014)	(0.000)	(0.255)	(0.024)
F		6.256		26.315	6.849		0.546	2.736
Partial R^2 ($\times$ 100)		0.004		0.016	0.042		0.000	0.016
Age Controls								
Age, Age^2	×	×				×	×	×
9 Yr of Birth Dummies			×	×	×	×	×	×
Instruments								
Qtr of Birth		×		×	×		×	×
Qtr of Birth × Yr of Birth					×			×

Source: 1920–29: 1970 Census, 1930–39 and 1940–49: 1980 Census.

Note: Coefficients are in bold, standard errors in parentheses. F and partial R^2 are for excluded instruments in first stage. Age is measured in quarter years. All specifications include race (1 = black), SMSA (1 = central city), married (1 = married, living with spouse), and 8 regional dummies as control variables. Sample sizes are 245,299 for men born 1920–29; 329,509 for men born 1930–39; and 486,926 for men born 1940–49.

been at least three times higher for those on the margin of dropping out of high school during the late 1950s and early 1960s (the cohorts born during the 1940s) than for those on the margin of dropping out during the late 1930s and early 1940s (the cohorts born during the 1920s).

This interpretation of the rise in the estimated effect of education on earnings does not seem particularly plausible to us. An extensive literature documents the fact that those who drop out of high school tend to be doing poorly academically and are disaffected with school long before doing so (Rumberger, 1987). Simple evidence on this score can be obtained by comparing test scores for those who do and do not drop out of high school. Using data from the National Educational Longitudinal Survey of 1988 (NELS) data we found that over half of the original eighth graders who had dropped out and not returned to school by the 1992 survey scored in the bottom quartile of the mathematics test they had been given as part of NELS in 1988. By comparison, under five percent of the dropouts had scored in the top quartile. The notion that the effect of education on earnings would be particularly high for this population, therefore, seems implausible.[12]

AK do not report our column (2) estimates, but control for the effect of age on earnings by either employing 9 year of birth indicators or including these indicators as well as a quadratic in age. The specifications that include only year of birth dummies are problematic, as they do not control for age variation within birth year, despite the fact that age (measured in quarter years) and quarter of birth are perfectly collinear within single-year birth cohorts. While this specification does not seem very sensible to us, for comparison purposes we present results from this model in columns (4) and (5).

Column (7) adds year of birth dummies to the specification of column (2), yielding large effects on both the coefficient and standard error estimates. This implies that much of the identification for the coefficients in column (2) derives from variation in the effects of quarter of birth across single-year cohorts. Adding quarter of birth $\times$ year of birth interactions as instruments (column (8)) increases the precision of the estimates substantially. While there is some theoretical justification for including these interaction terms, we are concerned that most of the identifying information on the effect of education comes from them. In addition, the standard errors on the specifications that include a quadratic in age as control variables are large enough to imply that the IV estimates have very little power to distinguish between plausible alternative point estimates.

At a minimum, these IV estimates of the effect of education on earnings appear to be extremely sensitive to the details of the specification used. Moreover, estimates based on what is, in our opinion, the most defensible

model specification give implausibly large estimates of the returns to education for cohorts born during the 1930s and 1940s.

3. QUARTER OF BIRTH AND LABOR MARKET OUTCOMES FOR MEN BORN 1840–1875

If compulsory school attendance laws do, in fact, account for all of the association between quarter of birth and both education and earnings, we would expect not to observe this association in cohorts educated before such laws were enacted.[13] Most states enacted their first compulsory attendance laws between 1870 and 1915 (Eisenberg, 1988). Initial laws usually required school attendance through at least age 14.

To test the hypothesis that no relationship existed between quarter of birth and earnings or other labor market outcomes in cohorts educated before the enactment of compulsory school attendance laws, we employ data from the 1900 Census. The public use files from this Census provide both the month and year of birth. While data on educational attainment and earnings are not available in the 1900 Census, information on the nature of work performed by the individuals in the sample has been recoded to 1950 occupational codings. To crudely proxy 1900 earnings, we calculated the mean log weekly earnings by occupation in the 1950 Census and used those means to impute earnings data to the 1900 microdata.[14] These mean earnings were then matched to the microdata from 1900 by 1950 occupation code.[15] The samples from both Censuses are limited to native-born white men.

In columns (1) through (4) of Table 4 we present reduced form regressions of (imputed) log weekly earnings on four quarter of birth dummy variables (with coefficients constrained to sum to zero), and a quadratic in age (measured in months). Columns (2) and (4) also include demographic control variables similar to those used above: indicator variables for eight regions of residence, married with spouse present, and living in a city with more than 50,000 in population.[16] The first two columns are estimated using a sample of white native men born in 1840–55 who were neither living nor born in Massachusetts or Vermont, the two states that enacted compulsory schooling laws before 1870. Virtually no one represented by this restricted sample would have been subject to compulsory school attendance laws while growing up. The resulting sample is quite small, however. In the third and fourth columns we add to the original sample white native men born in 1856–1875 who would not have been affected by compulsory schooling laws.[17] Because the imputation procedure induces heteroskedasticity in the error terms of the models, we present

heteroskedasticity-consistent standard errors estimated using White's (1980) method.[18]

Despite the fact that the men represented by these two samples from the 1900 Census were unlikely to be affected by compulsory school attendance

Table 4. Reduced Form: Quarter of Birth Effects on Imputed Log Weekly Earnings and I (Agriculture) for White Men Educated Prior to Compulsory Schooling Laws

	OLS: Imputed Log Weekly Earnings				Logit: I (Agriculture)			
	Men Born 1840–55		Men Born 1840–75		Men Born 1840–55		Men Born 1840–75	
Qtr of Birth	Age only	Age & Demo.	Age only	Age & Demo.	Age only	Age & Demo.	Age only	Age & Demo.
Jan.–Mar.	–0.019	–0.023	–0.050	–0.038	0.043	0.070	0.127	0.115
	(0.026)	(0.022)	(0.016)	(0.014)	(0.070)	(0.080)	(0.044)	(0.051)
Apr.–June	0.014	–0.006	0.023	0.005	–0.042	0.016	–0.059	–0.011
	(0.026)	(0.023)	(0.017)	(0.015)	(0.071)	(0.082)	(0.044)	(0.051)
July–Sep.	0.049	0.027	0.016	0.021	–0.129	–0.090	–0.046	–0.073
	(0.026)	(0.024)	(0.017)	(0.015)	(0.071)	(0.081)	(0.045)	(0.052)
Oct.–Dec.	–0.044	0.002	0.011	0.012	0.128	0.004	–0.021	–0.031
	(0.027)	(0.024)	(0.017)	(0.015)	(0.073)	(0.082)	(0.045)	(0.052)
Q_3–Q_1	0.068	0.049	0.066	0.058	–0.172	–0.160	–0.173	–0.188
	(0.042)	(0.037)	(0.027)	(0.024)	(0.115)	(0.131)	(0.072)	(0.083)
$\Sigma\|Q_i\|$	0.126	0.058	0.101	0.075	0.342	0.180	0.254	0.229
	(0.060)	(0.078)	(0.039)	(0.029)	(0.164)	(0.161)	(0.087)	(0.101)
χ^2	5.258	1.799	9.522	7.290	5.259	1.545	8.664	5.787
	[0.154]	[0.615]	[0.023]	[0.063]	[0.154]	[0.672]	[0.034]	[0.128]
N	2,464		6,185		2,464		6,185	

Source: 1900 Census, with earnings imputed using 1950 Census.

Note: Men Born 1840–55 sample excludes individuals born or living in Massachusetts or Vermont. Men Born 1840–75 sample includes all individuals from the 1840–55 sample plus individuals born 1856–75 were unlikely to have been constrained by compulsory school attendance laws in their state (see the text for details). All models estimated with OLS. Standard errors in parentheses, *p* values in brackets. Standard errors and covariances calculated using White's (1980) method. Coefficients on quarter of birth dummy variables are restricted to sum to zero. c2 is for joint significance of quarter of birth effects. Age is measured in months. All models include age and age squared. Demographic controls variables are lives in city with 50,000 or more inhabitants, married (1 = married, living with spouse), and 8 regional dummies.

laws, their imputed earnings show a distinct pattern of seasonality, with wages being higher for those born in the summer than for those born in the winter.[19] This pattern is similar to the pattern for men born during the first half of the 20th century, but, as the summary measures indicate, the amplitude of these effects was substantially larger for the earlier cohorts.

More than half of these samples were employed in agriculture, and the relationship between quarter of birth and the probability of being an agricultural worker drives, in part, the results in columns (1) through (4). In columns (5) through (8) of Table 4 we present logit estimates of the association between quarter of birth and employment in agriculture for these two samples. These estimates show that a winter birth raised the odds that a man was employed in agriculture by more than 10%.

We are cautious about over interpreting these results from the 1900 Census, for several reasons. First, the factors at work in creating an association between labor market outcomes during the 19th century might not be operative or as important during the 20th century.[20] Second, the labor market 100 years ago was very different than it was in 1970 and 1980. It is possible that the factors inducing an association between quarter of birth and labor market outcomes might have had a more important role in the labor market in 1900 than in the latter part of the century. Nevertheless, unless the factors at work at the the turn of the century were completely inoperative 70 and 80 years later, our results imply that the association between quarter of birth and labor market outcomes is not simply due to the effect of compulsory schooling attendance laws.

4. THE ASSOCIATION BETWEEN QUARTER OF BIRTH AND VARIOUS FACTORS

We have argued that the available evidence suggests compulsory school attendance laws are not the only factor behind the association between quarter of birth and either educational attainment or earnings. Nothing in these results proves, however, that there is a direct association between quarter of birth and earnings. While we know of no incontrovertible evidence on the direct effect of quarter of birth on earnings, it seems quite conceivable that such effects exist. Researchers have documented associations between quarter of birth and a variety of factors that either are known to affect earnings or might plausibly do so. We briefly outline some of these factors below.

Performance in School. There is some evidence that quarter of birth is related to performance in school. School attendance rates (Carroll, 1992), the likelihood that a student will be evaluated as having behavioral difficulties (Mortimore et al., 1988), the probability that a student will be referred for

mental health services (Tarnowski et al., 1990), and performance in reading, writing, and arithmetic (Mortimore et al., 1988; Williams et al., 1970 summarizes the earlier literature) have all been shown to vary by quarter of birth. The evidence regarding differences in the IQs of children born at different times of the year (Whorton & Karnes, 1981) is somewhat inconclusive.

Health differences. There is a substantial literature documenting that individuals born early in the year are more likely to suffer from schizophrenia (see, for example, Watson et al. (1984), O'Callaghan et al. (1991), and Sham et al. (1992)). Other health factors that vary by quarter of birth are the incidence of mental retardation (Knoblock & Pasamanick, 1958), autism (Gillberg, 1990), dyslexia (Livingston et al., 1993), multiple sclerosis (Templer et al., 1991), and manic depression (Hare, 1975).

Regional patterns. Clear regional patterns in birth seasonality show that there is substantially more seasonal variation in births in the southern states (Lam & Miron, 1991). For example, using data on white births in Georgia between 1948 and 1986, they found that there were 16% more births in September than in May. In contrast, whites were only 4% more likely to be born in September than in May in New York during the same period.

Race. Lam and Miron (1991) also document clear racial patterns in birth seasonality in the U.S., with blacks showing more seasonal variation in births than whites. For example, they find that (nationally) whites are 11% more likely to be born in September than May, compared to 16% for nonwhites.[21] These patterns cannot be explained by the overrepresentation of blacks in the South, as racial differences persist when Lam and Miron focus on specific states.

Family income. Kestenbaum (1987), using the 1980 Census, reports simple bivariate associations showing that children born to families with high incomes are more likely to be born in the spring. In Table 5 we confirm Kestenbaum's finding in a multivariate setting using data drawn from several Censuses and birth cohorts.[22] The table presents results of regressions of log per capita family income on quarter of birth dummy variables, with the sum of the coefficients constrained to be equal to zero. The second, fourth, and sixth columns add controls for race and geographic region of residence to the models. In general, we find that children born in the second quarter tend to be born into families with incomes that are statistically significantly above average, even after controlling for race and region of residence.

Personality. There is also some evidence that personality traits vary by season of birth. Using data from the National Longitudinal Survey of Youth,

Gortmaker et al. (1997) report a strong association between shyness and month of birth, with those born in the spring being 50% more likely to suffer extreme shyness. There is also a possible association between quarter of birth and overall happiness; those born in the first quarter of the year being are less likely to report being happy than those born later in the year.[23]

It cannot reasonably be disputed that there are relatively strong and robust associations between quarter of birth and various social outcomes. The degree of bias that these associations induce in the estimated return to education is less clear. Not all of the factors we have considered would appear to work in the same direction. While the evidence for some of the effects mentioned (e.g. the association between season of birth and schizophrenia) is overwhelming, the evidence on other factors is only suggestive. Moreover, some phenomena, such as schizophrenia, affect a small number of individuals in the population and would not be expected to account for much of the observed association between season of birth and education and earnings. The very weak relationship between quarter of birth and education attainment indicates,

Table 5. Variation in Log Per Capita Household Income, by Child's Quarter of Birth

	Boys Born 1944–59		Boys Born 1954–69		Boys Born 1964–79	
Quarter of Birth	No Controls	Controls	No Controls	Controls	No Controls	Controls
Jan.–Mar.	–0.0077	–0.0042	–0.0056	–0.0026	–0.0015	0.0014
	(0.0029)	(0.0026)	(0.0013)	(0.0012)	(0.0014)	(0.0013)
Apr.–June	0.0116	0.0018	0.0121	0.0055	0.0148	0.0087
	(0.0029)	(0.0026)	(0.0013)	(0.0012)	(0.0014)	(0.0013)
July–Sep.	–0.0073	–0.0031	–0.0049	–0.0025	–0.0008	–0.0054
	(0.0028)	(0.0025)	(0.0013)	(0.0012)	(0.0013)	(0.0013)
Oct.–Dec.	0.0034	0.0055	–0.0049	–0.0003	–0.0054	–0.0034
	(0.0028)	(0.0025)	(0.0013)	(0.0012)	(0.0013)	(0.0013)
F	7.757	2.318	29.313	7.100	42.954	18.044
	[0.002]	[0.073]	[0.000]	[0.000]	[0.000]	[0.000]
N	280,324		1,176,484		1,191,536	

Source: 1944–59: 1960 Census, 1954–69: 1970 Census, 1964–79: 1980 Census.
Note: Samples are limited to whites and blacks. All models estimated with OLS. Standard errors in parentheses, *p* values in brackets. The sum of the quarter of birth effects is restricted to sum to zero. *F* is for joint significance of quarter of birth effects. All models include a constant term. Models with controls also include race (1 = black) and 8 regional dummies.

however, that even minor associations between quarter of birth and other seasonal factors could produce large effects on the estimated coefficients (Bound, Jaeger & Baker, 1995).

To illustrate this point we have done a number of calculations. Using a specification that includes age and age squared as control variables while instrumenting for educational attainment with three quarter of birth indicators, we find that the addition of the 'black' dummy variable lowers the estimated return to education by between 0.6 and 1.7 percentage points in the three samples from AK. Using the same base specification, inclusion of 8 region of birth indicators affects the estimated return by between –0.1 and 2.2 percentage points.

It is also possible to crudely estimate the impact of leaving out family income from the IV estimates. Using the seasonal patterns of per capita family income reported in Table 5 in conjunction with this base specification estimated in AK's three birth cohorts, we crudely estimate that the inclusion of family income would actually raise the estimated return from anywhere between 0.1 and 6.6 percentage points, depending on which birth cohort and Table 5 results were used.[24]

Our calculations suggest that the seasonal patterns in some of the factors discussed above are sufficiently large to have a quantitatively important, if unpredictable, impact on IV estimates of the return to education in which quarter of birth is employed as an instrument for educational attainment.

5. ANGRIST AND KRUEGER'S ARGUMENTS FOR THE EXOGENEITY OF QUARTER OF BIRTH

AK are aware that the validity of their estimates of the causal effect of education on earnings rests on the assumption that there is no direct association between quarter of birth and earnings, and they present a number of arguments to support this notion in Section III of their paper.

In their discussion AK specifically discuss a number of the factors mentioned in Section 4 of this chapter. In particular, they briefly discuss the effect of age at school entry on educational achievement, noting that research in the area is weak and inconclusive. We feel that this characterization of the literature is accurate, but we do not think this fact validates the use of information on quarter of birth to form instruments for educational attainment. Children mature a good deal between the age of $5\frac{3}{4}$ and $6\frac{3}{4}$. Thus, it seems plausible that the exact age at school entry (and thus quarter of birth) could affect school performance and, potentially, earnings. Weak and inconclusive research evidence on the subject does not imply that quarter of birth does not affect

school performance or earnings, but rather that the direction and magnitude of such effects are unknown. This is a comfort only if we take the position that instruments are valid unless proven otherwise.

AK also discuss possible correlations between quarter of birth and parental socioeconomic status. In discussing this issue AK summarize the (at the time) unpublished work of Lam and Miron (1987):

> . . . [They] present a variety of evidence suggesting that season of birth is unrelated to the socioeconomic status (and other characteristics) of the parents . . . [T]hey find that the seasonal pattern of birth is similar . . . across regions of the United States . . . , and within countries before and after dramatic economic transitions (p. 1008).

This summary of Lam and Miron's work is at odds with their results that we reported in Section 4. Lam and Miron do find a summer peak and a winter trough in births for various U.S. populations, but (as we noted above) the amplitude of these cycles varies considerably across the population.

Towards the end of their Section III, AK present what would appear to be simple and clear evidence that there is no direct association between quarter of birth and earnings. Some of this evidence rests on problematic assumptions, however. AK report that, in their samples, the coefficients on quarter of birth indicators in regressions of earnings on education are statistically insignificant.[25] But this is a valid test of the exclusion restrictions necessary for quarter of birth indicators to be valid instruments in wage equations only if education itself is exogenous! Similarly, they report that while there is an association between quarter of birth and earnings in samples of men at all levels of educational attainment, no such association exists for college graduates. As long as education is endogenous, however, stratifying on education is problematic, introducing selection bias in the estimates from the self-selected sample.

6. CONCLUSION

We have presented evidence that calls into question the notion that compulsory school attendance laws are the only reason for the observed association between quarter of birth and educational attainment and earnings. For cohorts of men born during the first half of the 20th century, the association between quarter of birth and both educational attainment and earnings would appear to be too strong to be explained solely by compulsory school attendance laws. Moreover, we find an association between quarter of birth and labor market outcomes in cohorts whose education predates the existence of effective compulsory attendance laws.

Even if compulsory schooling attendance laws cannot account for the strong associations between quarter of birth and either educational attainment or earnings, this does not necessarily imply a direct association between quarter of birth and earnings. However, we have summarized a large literature documenting strong associations between quarter of birth and a variety of factors that could plausibly have direct effects on earnings. Moreover, IV estimates of the effect of education on earnings appear to be extremely sensitive to the details of the specification used. Sensible specifications give implausible results.

Perhaps the most striking result we presented is that the association between quarter of birth and educational attainment has declined (between cohorts born in the 1920s and those born during the 1940s) while at the same time no similar decline occurred for the association between quarter of birth and earnings. This finding alone is inconsistent with the notion that compulsory schooling attendance laws fully account for the association between quarter of birth and earnings and strongly suggests an association between season of birth and earnings, independent of the effect through education.

Our results do not conclusively *prove* that any possible direct association between quarter of birth and earnings is strong enough to seriously bias AK's IV estimates. Nevertheless, we believe that the evidence presented here, taken as a whole, makes it difficult to have any confidence in the validity of causal inferences drawn from the estimation of wage equations in which information on quarter of birth is used to instrument for educational attainment.

AK has stimulated a substantial amount of research using various alternatives to quarter of birth to form instruments for education in wage equations (see Card, 1995a and 1999 for reviews).[26] Butcher and Case (1994) use the gender composition of a woman's siblings; Evans and Montgomery (1994) use smoking history; Kane and Rouse (1993), Card (1995b), and Conneely and Uusitalo (1999) use distance to institutions of higher education; Maluccio (1999) uses distance to nearest high school; Harmon and Walker (1995) and Ichino and Winter-Ebmer (1998) use cohort dummies; and Ichino and Winter-Ebmer (1998), Lemieux and Card (1998), and Duflo (1999) all use interactions between cohort and location dummies. It is striking that most of these estimates of the effect of education on earnings exceed the corresponding OLS estimates, sometimes by large amounts. The validity of each of these estimates is open to *at least* as much question as the validity of AK's estimates that use quarter of birth to instrument for educational attainment. For example, the existence of human capital externalities would imply that locales with well-developed systems of higher education would not only produce but also attract a disproportionate number of college graduates. At the same time, these

externalities would imply that the higher average educational attainment in an area would have a positive effect on earnings in the area. The evidence in favor of human capital externalities (Rauch, 1993; Moretti, 1998) therefore calls into question the causal validity of estimates based on college proximity. In each of the other cases the estimates are plausibly biased upward.[27] We conclude that this literature, taken as a whole, contains little information regarding the causal effect of education on earnings.

NOTES

1. Even were one to accept AK's premise that there is no direct association between quarter of birth and earnings, it would be possible to question whether AK's instrumental variable methods estimate 'economically interesting evaluation parameters' (Heckman, 1997). The points we make in this chapter are quite seperate from this methodological criticism of AK's instrumental variables strategy.

2. Compulsory school attendance laws undeniably work to induce individuals to stay in school until they reach their 16th (or, in some cases 17th or 18th) birthday and, as a result, we would expect these laws to induce some of those who were born in the summer to finish a grade when they would not have in the absence of compulsory schooling laws. It is also possible that such laws would induce some individuals to continue their education past the compulsory age, including obtaining some post-secondary education when they would not otherwise have done so. For example, if the perceived benefits (at age 16) from post-secondary education were higher than the perceived benefits (at age 15) from secondary school education, then an individual on the margin of completing a grade at age 15 might be induced to obtain some post-secondary education. While this scenario is possible, we do not find it very plausible.

3. Using their Appendix 1, we were able to exactly replicate AK's 1930–49 and 1940–49 birth cohort samples from the 1980 Census. We were unable to exactly replicate their 1920–29 birth cohort from the 1970 Census, however, using information from the same Appendix. Our sample is slightly smaller than theirs (245,299 versus 247,199).

4. AK report similar tabulations in Table II of their paper. For men and women combined they estimate that for the cohort born during 1944, compulsory school attendance laws were binding on about 4% of the population.

5. If compulsory school attendance laws not only induce individuals born in the summer to complete 10th grade, but also induce them to continue their education past 10th grade, such laws could still fully account for the observed differences in educational attainment across seasons of birth. As noted above, we do not find such effects to be very plausible, but we cannot rule them out as impossible. It is worth noting, however, that the interpretation of AK's results is affected if compulsory school attendance laws did not simply induce individuals to stay in school until they were 16. Under the assumption that information on quarter of birth is a valid instrument (i.e. under the assumption that there is no direct association between quarter of birth and earnings), the estimates AK report are a weighted average of the effect of education on earnings for those induced by the compulsory attendance laws to continue their

education, with the weights being proportional to years of education gained (Angrist & Imbens, 1995). To illustrate the implication for AK's results, suppose that compulsory attendance laws raise the educational attainment for some individuals born late in the year by negligible amounts, but raise the educational attainment for others born at the same time by years. If those induced to obtain several additional years of education tended to be those with higher returns, then the estimated average effect would overestimate the actual average value of an extra year of education for those affected by the compulsory attendance law.

6. We employ the same control variables as AK use in much of their analysis: indicators for black, married with spouse present, lives in an SMSA, and 8 regions of residence.

7. In the results we report, we use a quadratic, but have experimented with cubics and quartics. In no case did the inclusion of these higher order terms have any significant effect on the estimated association between quarter of birth and either educational attainment or earnings. These results are available from the authors.

8. These estimates can be viewed as an example of Cook and Campbell's (1979) regression-discontinuity design.

9. Dramatic rises in the returns to education between 1970 and 1980 could explain the rising quarter of birth differentials observed in Table 2. We have good reason to believe, however, that the returns to education actually fell over this period (Freeman, 1975; Katz & Murphy, 1992).

10. AK purport to estimate the effect of an extra year of education for those induced to get more education by the compulsory schooling attendance laws. It might, therefore, make sense to restrict the samples for the OLS regressions, e.g. to men with no more than a high school education. The returns to education in these samples are approximately linear (unless there are sheepskin effects for those finishing high school or college), however, and doing so makes little difference to the estimates. For comparability with AKs tabulations we therefore retain men with more than a high school education.

11. Recent methodological work that was partially inspired by AK has emphasized the possible heterogeneity of the effect of education on earnings (Angrist & Imbens, 1995; Card, 1995a, 1999). In this context, IV estimates should be interpreted as "the average causal effect of treatment for those whose treatement status is affected by the instrument" (Angrist & Imbens, 1995).

12. Within the framework outlined by Becker (Becker, 1975; Card, 1995a), variation across the population in educational attainment is induced by a combination of the variation in the productivity of human capital investments and in the variation in the costs of obtaining such capital. For the effect of education on earnings to be substantially higher for those induced to obtain extra education by compulsory schooling attendance laws, the high costs of continuing their education rather than the low benefits of doing so would have to explain why some would want to drop out of high school early (before the age of 16) while others would not. Given labor laws that restrict the employment of those under the age of 16 as well as the public provision of high school education in the U.S., we see little reason to think that some individuals might want to drop out from school before they turned 16 because of particularly high costs of continuing their education. For individuals doing poorly in school, the psychic

costs of continuing might be quite high, but for them the productivity of continued time spent in school would also tend to be low.

13. This idea arose in a conversation between Peter Klenow and Gary Solon. We are indebted to Gary Solon for mentioning it to us.

14. Only 'sample line' individuals had information on earnings in the 1950 Census. Individuals whose sex, age, race, earnings, or occupation were allocated or who had unknown or missing occupational data were dropped from the 1950 sample. Categorical earnings data are given values in the midpoint of the interval of the category.

15. The validity of using data from 1950 to impute earnings in 1900 might be questioned. *Any* finding of a correlation between even this measure and quarter of birth, however, casts doubt on the maintained assumption that without compulsory schooling laws there would be no association between quarter of birth and earnings.

16. Only those individuals with a valid month of birth and valid occupational code were kept in the samples. Individuals living in 'Indian territories' or military reservations were dropped from the samples.

17. Men were excluded from this sample if they might have been subject to compulsory school attendance laws, according to their year of birth, their state of birth, and their state of residence in 1900. For example, Ohio enacted a law in 1877 requiring individuals to attend school through their fourteenth birthday. We, therefore, excluded from the sample men born after 1862 if they were born in Ohio or lived there in 1900. This algorithm eliminates many, but not all, of those men bound by compulsory school attendance laws. In particular, our algorithm would not eliminate men who grew up in states that enacted compulsory school attendance laws early but who were born and eventually settled in states that were late to adopt such laws. During the 19th century, migration was primarily east to west (Steckel, 1983, 1989), however, and states in the Midwest tended to adopt compulsory school attendance laws later than those in the Northeast (Eisenberg, 1988). It seems unlikely, therefore, that there are many men constrained by compulsory schooling laws who were not excluded by our algorithm.

18. While imputing earnings based on occupation is a common procedure, it will tend to bias regression coefficients downward because by ignoring within-occupation heterogeneity, it introduces mean-reverting measurement error into the dependent variable. Unlike classical measurement error, mean reverting measurement error in a dependent variable will tend to attenuate regression coefficients (Bound et al., 1994).

19. The large range in age in these samples (particularly the 1840–1875 cohort) made us particularly concerned about how we controlled for age in these models. In addition to parametrically controlling for age with a quartic, we also estimated generalized additive models (Hastie & Tibshirani, 1990) in which age was modeled as a locally-weighted running line smooth (loess) and the other variables in the model were entered parametrically. We set the span of the smooth so that it had approximate degrees of freedom of 5. Both the quartic and loess models produced estimates of the quarter of birth coefficients that were virtually identical to those presented in Table 4.

20. For example, the decline in infant mortality would have lessened the potential for selective survivorship to explain seasonal patterns in labor market outcomes.

21. These results are for first births between 1960 and 1986. A similar pattern holds for second births. Lam and Miron do not report results for all or higher order births for whites and non-whites in the U.S.

22. Per capita family income is defined as the total household income divided by the number of persons in the household. Income is a categorical variable in the 1960 and 1970 public use files. In those data we use the midpoint of the interval represented by the category.

23. A simple cross tabulation shows that respondents to the General Social Survey born in the first quarter of the year were roughly one percentage point less likely to report themselves as not too happy than those born in the rest of the year (11% vs. 12%). This association persists even after one controls for age, education, year of birth, religious affiliation, family income, marital status and labor force status. We thank David Blanchflower for providing us with these tabulations. The data and samples are similar to those used in Blanchflower and Oswald (in press).

24. In this calculation, we assumed that a 1% rise in family income would raise a son's weekly earnings by 0.4% and his educational attainment by 0.014 years. The 0.4% figure is based on Solon's (1992) finding that the intergenerational correlation in income between fathers and sons was about 0.4. The 0.014 year figure represents an unreported result based on the same samples used in Solon (1992).

25. In contrast, we find that when quarter of birth indicator variables are added to the OLS specifications in Table 3 they often have statistically significant coefficients. For example, a test of the joint significance of two quarter of birth indicator variables added to the specification in Table 3, column 6 for the 1940–49 cohort yields and F statistic of 5.965.

26. Another strand in the literature has used comparisons between monozygotic twins to estimate the returns to education. As a number of authors (Griliches, 1979; Card, 1995a; Bound & Solon, 1999) discuss, however, there is no a priori reason to believe such estimates are less biased than OLS estimates.

27. Butcher and Case rationalize their finding that women with male siblings obtain more education than do women with only female siblings in terms of the role model male siblings provide their sisters. To the extent that having male role models affects women's *educational aspirations*, however, it seems natural that such role models would also have a direct effect on women's *labor market aspirations*. Evans and Montgomery rationalize the association between smoking and education in terms of subjective time discount rates. Presumably, rational and forward-looking individuals who do not smoke will not only be more likely to invest only in formal education, but also in on-the-job training. They would then also have more reason to fear the consequences of being caught shirking. We have good reason to suspect, therefore, that subjective discount rates have a direct association with earnings. Harmon and Walker's and Ichino and Winter-Ember's IV estimates are too large to be credible, but here there is good reason to believe that the cohort effects they are estimating capture more than just changes in educational attainment (see Card, 1999).

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