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Using Unemployment Rates as Instruments to Estimate Returns to Schooling

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I use state unemployment rates during a person's teenage years to estimate the returns to schooling. A higher unemployment rate reduces the opportunity costs of attending school. Using the same 1980 Census data set that Angrist and Krueger (1991) use, I also estimate returns to schooling with a modified version of their quarter-of-birth instrument. The estimates from the two-stage least squares (2SLS) model using the unemployment rate and the model using the quarter-of-birth instruments are almost identical. In addition, these 2SLS estimates are larger than the ordinary least squares (OLS) estimates, supporting this counterintuitive, yet prevalent, result in the literature.

JEL Classification: J31

1. Introduction

There have been significant societal efforts over time to keep children in school and to encourage people to pursue a college education. The basis of these efforts is the general consensus that schooling helps people develop the necessary skills that will help them compete in the labor market and reduce their chance of experiencing poverty. However, schooling involves an opportunity cost (from foregone earnings, in addition to any direct costs), so that understanding the true returns to schooling would be important for individuals making schooling decisions. Yet, estimating the returns to schooling has proved to be very difficult.

The conventional thought in labor economics is that ability bias causes ordinary least squares (OLS) to overstate the monetary returns to schooling. That is, the higher earnings for more educated people would reflect, in addition to the causal effects of schooling, the effects of higher innate ability and motivation that cause some to obtain more schooling. Thus, a correction for ability bias should produce lower estimated returns to schooling. This result has been confirmed with studies on twins (Card 1999). However, as described in more detail below, a consistent result from two-stage least squares (2SLS) models that attempt to correct for ability and other biases is that the corrected estimates are higher. While each instrument can be questioned about its validity, together the estimates suggest that the ability-bias story is not the complete story.

In this article, I introduce a new plausible instrumental variable: the state unemployment rate during a person's teenage years. The unemployment rate is indicative of the economic

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conditions in that state. A higher unemployment rate could affect one's educational attainment through an income effect and a substitution effect. The income effect would be that, with higher unemployment rates, family incomes would be lower. This would cause some families to have their teenage children quit school to work to help support the family. In addition, the lower income could make college unaffordable. The substitution effect would stem from the lower wages and fewer job opportunities for teenagers associated with higher unemployment rates. These factors would lower the opportunity costs of attending school, which should lead to an increase in educational attainment. It turns out that the substitution effect dominates. My findings support the contention that this is a valid instrument.

Card (1999) reviews several recent analyses that use data on twins or a variety of other instruments in instrumental variables models to correct for these biases in estimating the returns to schooling.[1] The articles using twins almost all show that the cross-sectional OLS estimate is higher than the estimate based on differences across twins. Behrman and Rosenzweig (1999) confirm, based on a study of twins, that there is a positive ability bias on the estimated returns to schooling. In contrast, the articles that Card (1999) reviews based on instrumental variables models tend towards the opposite conclusion: The instrumental variables or 2SLS estimates in these articles exceed their OLS counterparts in almost every study and by up to 150%. For example, Card (1995) uses proximity to a four-year college as the instrumental variable and finds a coefficient estimate on years of schooling of 0.132, compared to the OLS estimate of 0.073. In about one-half of the studies Card (1999) reviews, the differences are statistically significant at least at the 10% level.[2]

The analysis I consider the most persuasive is Angrist and Krueger (1991). They use quarter-of-birth dummy variables interacted with year-of-birth dummy variables as the instrumental variables, with the argument being that, given compulsory school attendance laws, individuals born earlier in the year turn 16 (the typical school-leaving age) earlier, and thus can quit school before their younger classmates. They report several sets of OLS and 2SLS estimates based on whether they include different sets of covariates. In four sets of OLS-2SLS estimates using the male 1930–1939 cohort from the 1980 Census, the 2SLS estimate is higher in all but one.[3]

Despite the quarter of birth seeming to be a perfectly reasonable instrumental variable for years of schooling, its validity was called into question. Bound, Jaeger, and Baker (1995) cite evidence indicating that the quarter of birth may be associated with student performance, physical and mental health, and parental incomes. These factors may then have independent effects on a person's earnings. Although these independent effects may be small, Bound, Jaeger, and Baker (1995) argue that a weak correlation between the instrument and the endogenous

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[1] The 2SLS papers Card reviews are: Angrist and Krueger (1991), Kane and Rouse (1993), Card (1995), Harmon and Walker (1995), Ashenfelter and Zimmerman (1997), Conneely and Uusitalo (1997), Maluccio (1997), Staiger and Stock (1997), Card (1999), and Isaacson (1999). Two studies (Card [1995] and Conneely and Uusitalo [1997]) have two separate analyses that Card examines. The twin studies are Behrman, Rosenzweig, and Taubman (1994), Miller, Mulvey, and Martin (1995), Ashenfelter and Rouse (1998), Isacsson (1999), and Rouse (1999).

[2] The differences between the OLS and 2SLS estimates were significant for three of the seven studies using aspects of the school system as instruments, four of the five studies using family background as instruments, and two of the six studies using twins. Some studies have both types of instruments.

[3] The one set of estimates from Angrist and Krueger (1991) that Card (1999) reports has the OLS estimate slightly higher than the 2SLS estimate. However, these estimates are based on a model that includes a "married" dummy variable, which is potentially endogenous. The 2SLS estimate is higher for the three other specifications for the primary cohort (born in 1930–1939).

variable would exacerbate any inconsistency. As an important side note, Bound, Jaeger, and Baker (1995) claim that Angrist and Krueger's estimates are marred by finite-sample bias due to overidentification, which occurs even if the instruments are exogenous in the population. The finite-sample bias always biases the estimates in the direction of OLS estimates.

Using the same data set as Angrist and Krueger (1991), I estimate the returns to schooling with the unemployment rate during teenage years as the instrument and also with quarter-of-birth dummy variables as instruments. The 2SLS estimates from these two models are very close to each other, and they are higher than their OLS counterparts. Statistical tests support the contention that the instruments are valid. This lends more weight to the argument that the simple ability-bias story for the returns-to-schooling estimates is not complete. Furthermore, the results suggest that the returns to schooling are quite high for people whose schooling would be affected by the instrument.

2. The New Instrument

The condition for a valid instrumental variable in the 2SLS models is that the variable affects the years of schooling, but has no impact on the earnings outcome other than through its effect on years of schooling. The estimated returns to schooling would then be based on variation in earnings due to variation in the instrumental variable.

The new instrument I use is the average state unemployment rates over the three years in which the respondent turns 15, 16, and 17 years old. The unemployment rates (obtained from *The Manpower Report of the President* [various years]) are for workers who are covered by unemployment insurance (UI). This is the only state unemployment rate available from the late 1940s to the late 1950s, but it turns out to be an ideal instrumental variable, as will be described below. As described in the next section, I control for the state of birth and the year of birth so that the exogenous variation in educational attainment comes from the within-state changes over time in unemployment rates relative to other states. Over the 13 years that unemployment rates are used (1948 to 1959), the average standard deviation of the unemployment rate within states is 1.36 percentage points. With the three-year moving average unemployment rate (for ages 15 to 17), the average standard deviation is 0.75.

State unemployment rates during one's teenage years can affect school enrollment and educational attainment through two forces. First, there is an income effect, or "additional worker" effect. With a higher unemployment rate, earnings will be lower so families may need their teenage children to quit school and work to help support them. Families experiencing spells of unemployment may also experience more difficulty in sending their children to college. As labor market conditions improve and unemployment rates come down, more families can afford to let their teenagers attend school; thus, the income effect results in higher unemployment rates being associated with lower enrollment rates and less educational attainment. Conversely, the substitution effect, or the "discouraged worker" effect, results in higher unemployment rates being associated with higher enrollment rates. In periods of high unemployment, wages are typically lower and jobs are scarcer. Thus, the opportunity cost of attending school is lower, which would, according to Barceinas-Paredes et al. (2001), increase the rate of return to schooling, and thus increase educational attainment. As shown in section 4, the substitution effect dominates the income effect.

Previous research on the cyclical effects of school enrollment supports the result that the substitution effect dominates. Black et al. (2005) observe this with location-specific data, as they find that higher wages for low-skilled workers (related to coal booms) in Kentucky and Pennsylvania contributed to significantly lower high school enrollment rates. Using national-level data, Betts and McFarland (1995) find that enrollment of full-time students at community colleges increases by 0.5% and 4%, with a 1% increase in the unemployment rates, of recent high school graduates and of all adults, respectively. Examining individual-level data, Duncan (1965) finds that for males who reached age 16 between 1951 and 1958, a one percentage-point increase in the unemployment rate is associated with a 0.7% higher retention rate of high school students, relative to a moving average. Gustman and Steinmeier (1981), testing for the effects of local (SMSA) youth labor market conditions on enrollment rates of 17- to 22-year-olds, find results suggesting that, for males, lower youth unemployment and higher youth wages are associated with higher enrollment rates for this age group in 1976. However, the effects were generally smaller for 17- and 18-year-olds. This smaller effect for people of high school age may be partly attributable to the estimates being affected by the endogeneity of youth wages and youth unemployment rates. That is, if a high proportion of some area's teenagers prefer working to attending school, then the supply of teenage labor would be high, which would be associated with lower teenage wages and perhaps higher teenage unemployment rates.

To be a valid instrument, the state unemployment rate during a person's teenage years must affect years of schooling and must not affect earnings 20 to 30 years later, except for its effect through educational attainment. One concern about the validity of the instrument is that it is possible that weak job opportunities when entering the labor market could cause depreciation in skills (Pissarides 1992) and psychologically affect young workers (Clark, Georgellis, and Sanfrey 2001). Most studies examining this issue estimate the effect of individual unemployment experiences rather than the effect of a weaker economy (e.g., Franz et al. 1997 and Gregg 2001). Oreopoulos, van Watcher, and Heisz (2006) find that Canadian college graduates entering the labor market in a recession have lower earnings in subsequent years, but the effect disappears after 8–10 years. On the other hand, Raaum and Roed (2006) find persistent employment effects among Norwegians from weak labor market conditions when people graduate from secondary school. Thus, there are plausible reasons and some supporting evidence (at least from Norway) for why the unemployment rate may be an invalid instrument. However, overidentification tests provided below offer no evidence of any independent effects of the unemployment rate (or labor market conditions) during teenage years on earnings 20 to 30 years later.

A second concern about this instrument is that the unemployment rate for teenagers could conceivably be correlated with the general willingness to work instead of attending school among a state's teenagers, which might also be correlated with those teenagers' later earnings. But the unemployment rate for workers covered by unemployment insurance should hardly be affected by teenage labor supply decisions. Furthermore, state fixed effects should control for differences across states in teenagers' general willingness to work. A third concern about the validity of the instrumental variable is that the unemployment rate could be correlated with the educational and skill levels of parents within a state, which is likely correlated with parental influences on children's development. However, the state fixed effects should also eliminate any partial correlation between the unemployment rate and parental influences.

One other potential problem with the instrumental variable is that teenagers intent on working instead of continuing with school may move to states with strong economic

Table 1. Descriptive Statistics for the Earnings Analyses (Number of Observations = 279,522)

	Mean	Standard Deviation
Weekly earnings	439.12	337.22
Annual earnings	21,507.67	11,601.85
Years of schooling completed	13.04	3.13
Age	41.44	2.94
Average unemployment rates for ages 15, 16, and 17	3.95	1.52

conditions, which would make the unemployment rate endogenous. This is the reason why I average the unemployment rate measured only up to age 17. Up to this age, it is unlikely for a teenager to move to a different state other than a move with their parents. Once a person turns 18 he or she may be more likely to move away from home, perhaps to a different state, so that the unemployment rate from a person's state of birth would be less indicative of the labor market conditions a person faced.[4] Furthermore, whereas with teenagers in their high school years the substitution effect dominates so that there is a significant relationship between the unemployment rate and educational attainment, the income effect would likely be greater for people in their college years, as families may expect children to help the family financially when they graduate from high school. In addition, a higher unemployment rate makes college less affordable for families. Indeed, I found no significant relationship between the unemployment rate at age 18–22 and educational attainment, as the larger income effect likely cancels out the substitution effect.

3. Data and Methods

Data

The data for this analysis come from the 1980 Census 5% Public Use Microsample. The sample is restricted to white males who were born between 1933 and 1942 in the United States (and subsequently were teenagers in the late 1940s and the 1950s and age 37 to 46 in 1980), were a salary or wage earner, had positive earnings and weeks worked in the previous year, and did not have any allocated values for any variable used in the analysis. Table 1 contains the descriptive statistics. I restrict the model to males because, with the relatively low employment rates of females, the selectivity issue would be extremely difficult to address while using instrumental variables models to estimate the returns to schooling. The oldest cohort is of individuals born in 1933 because this is the oldest group for which state unemployment rates at age 15 are available. The reason for the lower limit on age, as mentioned earlier, is to have the sample close to the flat part of the age-earnings profile. I include only whites in the sample because the unemployment rate performs poorly as a predictor of the educational attainment of other racial groups. This may be attributable to the income and substitution effects canceling each other out for the other racial groups or just being smaller than that for whites. With fewer labor market opportunities for minorities, when the economy was generally strong, there would be a smaller substitution effect than for whites. At the same time, due to these fewer job

[4] As evidence, of whites aged 19 to 22 in 1950 and in 1960, 77.1% and 69.2% lived in the same state in which they were born, respectively, compared with 84.3% and 77.5% of 15- to 17-year-olds.

opportunities, the ability of a minority teenager to contribute towards family income in a weak economic period may be limited, which would keep the income effect small.

The Census does not provide the state in which a person lived as a teenager, but it does indicate a respondent's state of birth. These states of birth are matched with state-level unemployment rates to give an indication of the economic conditions a person faced as a teenager. This method causes some imprecision in the measure of the actual labor demand conditions a person faced because of moves to other states during one's childhood. From the 1950 and 1960 Censuses—which is roughly the time when the people in the sample were teenagers—84.3% and 77.5% of the white 15-17-year-old teenagers lived in the same state in which they were born, respectively.

It is important to note that the 1980 Census provides a unique window of opportunity to examine this issue with these instruments. For earlier Censuses, the unemployment rate is not available for the appropriate age range. For later Censuses, there is no quarter of birth, and the educational attainment is measured differently. Furthermore, based on exploratory work I did, the unemployment rate loses power as an instrument for later cohorts, as the economy likely played a smaller role in educational attainment.

Methods

The 2SLS model used to estimate the returns to schooling consists of the following two equations:

$$S_{ist} = X_{ist}\gamma_1 + \lambda(UR_{st}) + u_{ist}, \tag{1}$$

$$Y_{ist} = X_{ist}\gamma_2 + \beta S_{ist} + \varepsilon_{ist}, \tag{2}$$

where S_{ist} is the years of school completed for person i from state s who was 15 to 17 years old in period t; Y_{ist} is the natural logarithm of either weekly or annual earnings measured in 1980; X_{ist} is a vector of exogenous determinants of earnings; UR_{st} is the average annual unemployment rate for a respondent's state of birth during the years in which he turned 15, 16, and 17 years old (which is particular to a state and birth year); and u_{ist} and ε_{ist} are error terms. We use the average unemployment rate for the three years rather than the three unemployment rates in order to reduce the possibility of finite sample bias, as described in Bound, Jaeger, and Baker (1995). It is assumed that $E[u \mid \mathbf{X}, UR] = 0$.

The primary empirical problem in the model comes from a correlation between years of education (S) and the error term (ε) in Equation 2, which would cause a bias in the estimate of β. To correct for this and other biases, the model is solved by substituting the fitted value for S from Equation 1 for the actual value for S in Equation 2, for which it is assumed that $E[\varepsilon \mid \mathbf{X}, UR] = 0$.

The vector $\mathbf{X}$ includes year-of-birth, state-of-birth, and current-state-of-residence (as of 1980) dummy variables. The year-of-birth dummy variables are meant to capture age differences in earnings and the rising trend in school attendance and educational attainment through the late 1940s and the 1950s. The state-of-birth dummy variables are included to control for state-level differences in the quality of education, parental influences, and the propensities to acquire more education. The purpose of controlling for the current state of residence is to increase the power of the model. I exclude marital status and urbanicity from the model because they are potentially endogenous to the factors determining education and earnings.

Table 2. First-Stage Results: The Effects of State Unemployment Rates (UR) on Educational Attainment (Dependent Variable = Years of Schooling; Number of Observations = 279,522)

	First-Stage
Average state-of-birth UR at ages 15, 16, and 17	0.038 (0.009)***
Born in the second quarter	0.051 (0.017)***
Born in the third quarter	0.093 (0.016)***
Born in the fourth quarter	0.146 (0.016)***
R^2	0.048

Also included in the regression are a constant and state- and year-of-birth dummy variables. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

As mentioned in the previous section, the key exclusion restriction assumption is that the within-state changes in the unemployment rate in a person's teenage years (the proxy for the demand for teenage labor) does not affect his adult earnings 20 to 30 years later, except for its effect through years of schooling, once one controls for the other factors represented in vector **X**.

Because I use the same data source as Angrist and Krueger (1991), I can estimate separate models using their instrumental variables: the quarters of birth. This allows a comparison of estimates with the same sample, but using two different instruments. This also presents the opportunity to examine the validity of Angrist and Krueger's quarter-of-birth instruments. While I used the same sample for both models for comparative purposes, the sample differs from Angrist and Krueger's original sample in that I use only white males born between 1933 and 1942, while they use all males born between 1930 and 1939. Regarding the quarter-of-birth instruments, whereas Angrist and Krueger use 30 instruments (10 year-of-birth dummy variables interacted with 3 quarter-of-birth dummy variables), I use just the three quarter-of-birth variables as the instruments to reduce the potential of finite-sample bias. And, whereas the covariates I use for the models, besides the instruments, are just a simple set of dummy variables for birth state, current state (in 1980), and year of birth, Angrist and Krueger, depending on the specification, also include whether the respondent lives in an SMSA and marital status, along with less-specific current geographical variables (Census region dummy variables) and age variables (age and age-squared).

4. Empirical Results

First-Stage Results

Table 2 shows the results of the first stage of the empirical model (Eqn. 1). A higher state unemployment rate in one's teenage years (indicating lower demand for teenage labor) leads to greater educational attainment. An increase of one percentage point in the average unemployment rate a person faced in the three years between ages 15 and 17 is estimated to increase the number of years of school he completed by 0.038, *ceteris paribus*.[5] The estimate is

[5] In an alternate specification in which the three unemployment rates—at ages 15, 16, and 17—are included separately, only the estimate on the unemployment rate at age 15 is statistically significant. The statistical insignificance of the other unemployment rates may be attributable to the high correlation of the three unemployment rates to each other. The correlations between the unemployment rates at ages 15 and 16, at ages 15 and 17, and at ages 16 and 17 are, respectively, 0.48, 0.34, and 0.53.

Table 3. The Effects of Educational Attainment on Weekly Earnings (Dependent Variable = Natural Logarithm of Weekly Earnings; Number of Observations = 279,522)

	OLS	2SLS		
	(1)	(2)	(3)	(4)
Years of schooling completed (predicted value for 2SLS)	0.0655 (0.0003)***	0.096 (0.044)**	0.098 (0.020)***	0.098 (0.018)***
Born in the second quarter	0.001 (0.003)	−0.001 (0.004)	Excluded	Excluded
Born in the third quarter	0.003 (0.003)	0.001 (0.005)	Excluded	Excluded
Born in the fourth quarter	0.004 (0.003)	−0.000 (0.007)	Excluded	Excluded
Average UR at ages 15, 16, and 17	0.001 (0.002)	Excluded	−0.0001 (0.0018)	Excluded
R^2	0.149			

Also included in the regression are a constant and state- and year-of-birth dummy variables. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

statistically significant at the 1% level. The estimates indicate that if a state unemployment rate were to increase by one percentage point relative to other states and if this change were to either induce people to acquire one more year of schooling or not alter a person's schooling, then about 4% of the white males in that state would acquire one more year of schooling.

Quarter-of-birth dummy variables are also included in the first stage. Each quarter-of-birth variable has a statistically significant ($p < 0.01$) coefficient estimate. The estimates imply that being born one quarter later in the year is associated with roughly one-twentieth of a year more schooling. This result is consistent with Angrist and Krueger's (1991) argument that those born earlier in the year have less schooling because they turn the school-leaving age earlier.

Second-Stage Results

Table 3 presents the results for the effects of schooling on the natural logarithm of weekly earnings. The first column shows the results for the OLS equations that assume random selection into educational levels, distinguished by whether the current-state dummy variables are included. Columns 2–4 display results from the 2SLS model that uses as instruments, respectively, the average state unemployment rate at ages 15 to 17, the quarter-of-birth variables, and both the unemployment rate and quarter-of-birth variables. All standard errors for the 2SLS models are corrected for the presence of a predicted variable.

In the OLS model, one year of schooling is estimated to increase weekly earnings by 6.6%. All of the 2SLS estimates are close to 0.100, suggesting that one year of schooling increases earnings by 10%. The estimates on predicted years of schooling using the unemployment rate as the instrument are statistically significant at the 5% level, while the estimates from the two models using the quarters of birth as the instruments are significant at the 1% level.

The results for annual earnings follow the same pattern as those for weekly earnings, as shown in Table 4. The OLS model produces a coefficient estimate on years of schooling of 0.074. The 2SLS estimates are all about 50% higher—ranging from 0.112 to 0.118. The levels of significance of the variables remain the same as for weekly earnings.

Table 4. The Effects of Educational Attainment on Annual Earnings (Dependent Variable = Natural Logarithm of Annual Earnings; Number of Observations = 279,522)

	OLS	2SLS		
	(1)	(2)	(3)	(4)
Years of schooling completed (predicted value for 2SLS)	0.0743 (0.0004)***	0.118 (0.056)**	0.113 (0.023)***	0.112 (0.021)***
Born in the second quarter	−0.001 (0.003)	−0.003 (0.004)	Excluded	Excluded
Born in the third quarter	0.004 (0.003)	0.000 (0.006)	Excluded	Excluded
Born in the fourth quarter	0.004 (0.003)	−0.002 (0.009)	Excluded	Excluded
Average UR at ages 15, 16, and 17	0.001 (0.002)	Excluded	0.0002 (0.0020)	Excluded
R^2	0.143			

Also included in the regression are a constant and state- and year-of-birth dummy variables. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Thus, the results from the 2SLS model using the unemployment rate during one's teenage years as the instrumental variable are very close to the results from the model using the quarter of birth as the instruments. Furthermore, these results are consistent with the general findings of the articles Card (1999) reviews—that the 2SLS estimates exceed the corresponding OLS estimates.

Tests on the Validity of the Instruments

With four instrumental variables—the unemployment rate and the three quarter-of-birth variables—I can perform Sargan overidentification tests (for the models corresponding to column 4 of Tables 3 and 4) to test the validity of the instruments. The tests produce a statistic of 0.243 ($p = 0.97$) for weekly earnings in Table 3 and 1.417 ($p = 0.70$) for annual earnings. These provide no evidence indicating that the instruments are invalid. In addition, in columns 2 and 3 of Tables 3 and 4, I include in both stages the set of instruments not used as the excluded variables to determine whether they have an independent effect on earnings. That is, I included quarter-of-birth dummy variables in both stages of the 2SLS model using the unemployment rates as the instruments and vice versa. The estimates on the quarter-of-birth and unemployment-rate variables have very low magnitudes, and in no case are the estimates statistically significant, individually or jointly, in the earnings equations. While such a test is valid only if the exclusion restriction is valid, these results support the argument that these are valid instruments.

5. Discussion and Conclusions

In this article I have introduced a new instrumental variable—the state unemployment rate during teenagehood—that can be used for estimating the returns to schooling. Individuals who were teenagers at a time of relatively high unemployment rates in their states had higher educational attainment than others who were teenagers at a time of relatively low unemployment rates, *ceteris paribus*. In addition, I have offered evidence supporting the validity of this new instrument, as well as Angrist and Krueger's (1991) concept of using the quarter of birth as the instrument. Using data from the same source as in Angrist and Krueger's analysis—although using a different set of explanatory variables and a slightly different sample—I find that the estimated returns to schooling using the state unemployment rate as the instrument are very close to those from a model using the quarter of birth as the instrument. The estimated returns to schooling are about 10% for weekly earnings and about 11% for annual earnings. These are about 50% higher than the OLS-estimated return to schooling. The differences between the OLS and 2SLS models are statistically significant at the 10% level just for the models using both the unemployment rate and quarter-of-birth variables as the instruments and the models using just the quarter-of-birth variables as instruments. These results support the conclusion of earlier studies that ability bias is not the complete story on OLS estimates in the returns to schooling.

Card (1999) offers four explanations for why 2SLS estimates have consistently exceeded OLS estimates in the literature:

(1) Measurement error causes downward bias in OLS estimates.
(2) The instruments have been invalid in that they have been correlated with earnings beyond their effects through educational attainment.
(3) Publication bias is positive for 2SLS estimates.
(4) There are heterogeneous returns to schooling, and the returns to schooling are higher for the people or years of schooling affected by the instruments; this is based on the argument that the 2SLS estimate represents the marginal return to schooling for the people affected by the instrument and for the years of schooling affected by the instrument.

While Card (1999) argues that measurement error produces at most a 10% bias, Ashenfelter and Krueger (1994) find, based on an analysis of twins, that downward measurement error in self-reported years of schooling causes substantial downward bias in the estimated returns to schooling. However, this was based on a sample of just 149 sets of twins. Regarding the second explanation, the results of this article offer support for the argument that both the unemployment rate during one's teenage years and the quarter of birth are valid instruments.

Under the assumption that publication bias is not relevant in this case, I can make an argument for the heterogeneous returns-to-schooling explanation being the primary source of differences between the 2SLS and OLS estimates. This argument relies on the reasonable theory that, rather than being a population parameter, the return to schooling is a random variable that varies across people and across years of schooling. While OLS models estimate a return to schooling more weighted on college years because that is where most of the variation lies, perhaps the 2SLS estimate measures a return to schooling more weighted on high school years because, as Card (1999) notes, factors like compulsory schooling would affect the educational attainment of those who would otherwise have low levels of schooling.

One would expect that the unemployment rate during teenage years would also affect mostly the high school years, with perhaps a small effect for the college years. To determine whether this is the case, I estimate simple OLS regressions for the number of high school years completed (beyond eighth grade, taking the values 0 to 4) and the number of college years (or post–high school years, to be specific) with the unemployment rate and the quarter-of-birth and year-of-birth dummy variables. Table 5 shows the results of these regressions. As would be expected, the unemployment rate primarily affects the number of high school years, having a small and insignificant effect on the number of college years completed. The story is different for the quarter-of-birth variables, for which significant effects persist into college. This suggests that the people who are spurred to obtain more education due to a poor economy are not likely

Table 5. The Effects of the Instruments on High School and College Years of Schooling (Number of Observations = 279,522)

	Dependent Variable	
	High School Years Completed	College Years Completed
Average state-of-birth UR at ages 15, 16, and 17	0.027 (0.003)***	0.004 (0.005)
Born in the second quarter	0.016 (0.006)**	0.029 (0.009)***
Born in the third quarter	0.045 (0.006)***	0.032 (0.009)***
Born in the fourth quarter	0.0069 (0.006)***	0.056 (0.009)***
R^2	0.047	0.027

Also included in the regression are a constant and state- and year-of-birth dummy variables. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

to go on to college, while those who are spurred from having to stay in school due to mandatory attendance laws do sometimes end up obtaining some college education. The argument that the 2SLS estimates would be higher than the OLS estimates because the return to the high school years is higher than that for college years would only work for the models using the unemployment rate as the instrument.

Another quite plausible argument is that the return to schooling may be higher for the type of person whose schooling would depend on the quarter of birth (via compulsory schooling laws) or the availability of jobs during teenagehood. Based on a simple opportunity-cost argument, those who find schooling more difficult will have a higher opportunity cost to attending school and thus will be more responsive to reasons to quit school (such as a good job opportunity or turning the school-leaving age). Under the plausible assumption that people do not really know what their returns to schooling are, it is possible that these people who struggle get more value-added from schooling than the people who breeze through school. Thus, with this argument, it could be that those who are marginally attached to schooling would actually be attaining greater returns to schooling than those with greater attachment to school. Providing some supporting evidence, Kling (2001) finds that the people who are affected by the Card (1995) instrument of college proximity are people who are from more disadvantaged backgrounds and whose parents have low educational levels. Furthermore, Kling finds that the instrumental variables-estimated returns to schooling—which are 10–14%, compared to 8% for OLS—are weighted 53% by the "lowest family background quartile" and another 20% by the second lowest quartile. This also suggests that the returns to schooling for this group are higher than normal. What my results, along with Kling's, suggest is that the returns to schooling for these people who are marginally attached to school are quite high.

While most efforts today seem to be for promoting a college education, the same concepts may apply since most people obtain a high school education today, and most of the variation lies in the amount of a college education a person gets. If so, the results would suggest that the returns to schooling for those marginally attached to schooling (for example, perhaps those whose college attendance would depend on available credit or scholarships) may be quite high, so that policies of making college affordable to all people may be justified.

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