Ability Bias and the Return to Schooling: A Cohort Based Analysis *

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Abstract

The traditional estimates of the return to schooling obtained by regressing wages on education are known to potentially suffer from ability bias. This paper attempts to eliminate ability bias and measure the causal effect of schooling on wages by instrumenting educational attainment with year of birth in a wage regression. Birth year captures the cost and benefit conditions at the time a worker makes his schooling decision. For instance, a rise in the cost of education decreases the schooling choices by younger workers relative to other cohorts. This generates a decrease in earnings of that cohort, relative to others, only if education is productivity enhancing. Identification requires that the average ability of a cohort does not systematically vary with the average education of that cohort. Possible longer term trends in average ability are captured by time trends, and potential cohort size effects on wages are controlled for by the relative size of the labor force in different age and education groups. Using data from the CPS March Supplement for the years 1964-2003 and the Decennial Census Surveys 1960 - 2000, wage regressions are estimated where education is instrumented by birth cohorts and state - birth cohorts. The results suggest that the a year spent at school increases worker productivity by 5%, which is approximately 3% less than the LS return. This contrasts sharply with instruments used in the literature, such as proximity to college, tuition costs, or the timing structure of the compulsory schooling laws, which have generally yielded larger returns than OLS.

Keywords Ability Bias, Returns to Schooling, Education, Earnings

JEL Codes J24, J31, I20

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One of the most robust findings in the empirical labor literature is that the workers with higher educational attainment have higher earnings. The rate of increase in labor productivity due to education is more controversial. If the decision to educate oneself is a matter of pure income maximization, then the rate of return to a year spent at school should simply be equal to the interest rate. The standard estimates obtained by the least squares (LS) method are usually on the order of 8-10%. But these estimates may be erroneous, because the differences in the wage rates of workers with different levels of education may partly reflect the inherent differences in unobservable characteristics (Willis and Rosen (1979), Griliches (1977). If workers with higher educational attainments also have more productive qualities, then the observed link between education and wages overstates the return to education. The purpose of this paper is to purge the estimates of the ability bias and measure the true return to education[1].

A common remedy adopted in the literature is to estimate the relationship between wage and education level by instrumenting schooling outcomes with variables that are orthogonal to ability[2] (Angrist and Krueger (1991), Angrist (1990), Card (1995), Harmon and Walker (1995)). The estimates of the return to education in this literature using the Instrumental Variables (IV) method are actually higher than the estimates obtained by the Least Squares (LS) method[3] casting doubt on the validity of the instruments[4]. Although these studies have contributed significantly to the understanding of the IV method, their results can not be reconciled with other studies in the literature. Using IQ test scores as a proxy for ability, Blackburn and Neumark (1993) conclude that the LS estimate is biased upward. Belzil and Hansen (2002) reach the same conclusion with more structural restrictions. Under the assumption of a non-negative causality between education and wages (monotonic treatment response) and between ability and education (monotonic treatment selection), Manski and Pepper (2000) derive an upper bound for the average annual return to college of about 9.9 percent. This return is lower than most of the existing IV estimates in the literature. The method adopted here is also an IV approach, but the results indicate that the true return to education is significantly lower than the LS estimate and that the ability bias can be up to 3 percentage points depending on the specification.

This paper argues that one's year of birth can be used as an instrument for his educational attainment. People usually complete their education early in their life before entering the labor market and hardly modify their decisions later. This implies that one's year of birth describes his cost and benefit conditions for the schooling decision relative to the workers of other birth cohorts. If ability is pre-determined, then a cohort's ability could not be affected by a change in these conditions. For instance, a fall in the cost of education would increase

[1]It is understood that the true return to education refers to the rate of productivity augmentation at school. If the ability bias emerges as a result of sorting in a human capital model, this is also the worker's private return. If the ability sorting is driven by a signaling motive as in Spence (1973), then the true return would be less than the private return.

[2]See Card (1999) and Heckman et al. (2005) for a survey of the empirical literature on the relationship between education and earnings.

[3]Although the IV point estimates are almost always higher, the difference between the two estimates is not always statistically significant. See Table II in Card (1999).

[4]An alternative explanation provided in the literature is that the instruments appeal to a narrowly defined set of agents who are not representative of the overall population, and the IV estimates are capturing local effects for these workers (Angrist and Imbens (1994)).

the educational outcomes of the more recent cohorts, but their ability would be unchanged. Earnings of these cohorts would rise only if the additional education enhances their market productivity. Therefore the rate of return to education could be measured by projecting the aggregate productivity of cohorts on the average level of education.

In general, the productivity may rise over time for other reasons, such as developments in early childhood education and care, or improvements in the quality of education. These long term trends may also be accompanied by rising educational attainments of cohorts, but if these slower movements can be captured by a deterministic trend in the year of birth, then the true return to education can still be consistently estimated.

In addition to the general changes in the costs and benefits of education, regional changes in the determinants of schooling choices also affect the educational achievements for a subgroup of people in a birth cohort. Changes in the state-level policies on education finance or introduction of compulsory education laws are typical factors that would generate variation in education levels across cohorts by state. As far as one's state of birth is informative of his decision environment and thereby of his educational outcome, one could also measure the true return to education by using the interaction of the year of birth with the state of birth as instruments for educational choices. Section A. estimates the rate of return to education using state-cohorts and confirms that the IV estimate is lower than the LS estimate. Additional variation in educational attainment captured by state-cohorts considerably improves the precision of our estimates.

Section B. relaxes the assumption of perfect substitutability of labor across education and age groups, which is implicit in the standard Mincer regressions. The potential impact of equilibrium effects, triggered by changes in the relative supply of an education group in a cohort, on our estimates is bound to be of second order because each cohort constitutes only a fraction of the work force. However, since the identification of the return to education relies on the cohort-based comparison of earnings and education, an association between the size of a cohort and its educational attainment could lead to a first order bias if the cohort size is omitted from the regressions. Controlling for the changes in the size of the work force across cohorts and education groups slightly increases the IV estimate but the main result of the paper remains intact as it is still less than the LS estimate.

I. Relevance of Birth Cohorts as Instruments

The objective here is not to provide a justification for a specific event that separates workers into treatment and control groups with respect to their educational achievement. Provided that the decision environment for education is subject to variation, classifying workers by their year of birth naturally separates workers with regard to the determinants of their schooling choices. Nevertheless it may be useful to mention some examples of cohort effects in educational attainment. Data suggest variations in both costs and benefits of education that are relevant to the workers in the sample[5]. Figure 1 depicts the average tuition cost of education for years 1919 - 1995. Average tuition cost per student increased from $777

[5]The data used here contains cohorts that were born between the years 1910 and 1968, and the average educational attainment of these cohorts varied from 10 to 14 years in the sample (See Table 1).

in 1919 to over $4000 in early 1990s in the institutions of post-secondary education[6]. The amount of time that an average worker had to work in order to meet the average tuition payment varied from 3 to 6 weeks between 1919 and 1995. Figure 2 depicts the revenues of educational institutions obtained from local sources in the form of tuition payments, gifts and donations. Local revenue per student increased throughout the period, and total share of local funding for secondary education decreased from around 80% to 45% between 1919 and 1969 indicating a rise in state and federal funding for education.

The Federal Family Education Program (FFEP) was initiated in 1966, making it available to roughly a quarter of the cohorts in the sample. Since these loans were not awarded on a merit basis, movements in the cost of education are not directly related to ability. Figure 4 displays the total number and the value of loans provided under the FFEP. Total subsidies to college increased considerably since early stages of the program, and were made widely accessible.

Importance of institutional factors in educational attainment has been mentioned in the literature. Goldin (1999) emphasizes the role of the compulsory education laws in the "high school movement" of early twentieth century.[7] Bound and Turner (2002) argue that the G. I. Bills for the veterans of the World War II raised the college attendance rates in mid-20th century.

Similarly a change in the return to education over time would also generate fluctuations in educational attainment. Katz and Murphy (1992) and Card (2001) argue that a transformation in labor demand is necessary to explain the rising wage premium between college and high school graduates. An improvement in technology that is biased toward the skilled would make higher education more attractive. In addition, increases in wealth or life expectancy over time, changes in federal education policy, or events like the Vietnam War and the World War II are other factors that might lead to changes in educational achievement.

Figure 3 depicts the average educational attainment of birth cohorts. The average level of education rose steadily earlier in the last century and has been stagnant for cohorts born after 1950s. The lower line shows the fluctuations in education level relative to a quadratic trend in the year of birth. Educational attainment has been fluctuating throughout the century, suggesting potential variation for the estimation of the return to education.

Validity of birth year cohorts as instruments in a wage regression requires, perhaps more importantly, absence of cohort effects in wages. However, these effects have been discussed in the literature as early as Welch (1979). If workers of different age and education groups are imperfect substitutes in the market, then a change in the relative supply of a group depresses the wages. Welch (1979) and Berger (1985) argue that the baby boom generation suffered from reduced earnings growth early in their career because of their larger size. However, the quantitative impact of cohort size is bound to be of second order, because workers born in a particular year constitute only a small fraction of the labor force at any given time. The impact on our estimates of the return to education can be sizable only if the cohort size has

[6]The tuition cost per student is approximated by dividing the total tuition revenues of post-secondary education institutions by enrollment. Statistics are obtained from the U.S. Department of Education and all values are converted to 2003 dollars.

[7]In fact, variation in educational attainment due to changes in the compulsory schooling laws has been used in the literature to estimate the return to education. See, for instance, Lang and Kropp (1986), Angrist and Krueger (1991) or Acemoglu and Angrist (2000).

a direct effect on educational attainment. I account for the cohort effects in two different ways. First, these effects are directly controlled for by inclusion of a flexible function of the size of the cohort-education cells. Second, I extend the instruments to include state-cohorts. Since the cohort size are likely to affect the wage growth at a national level, the variation in wages with respect to birth cohorts across states is not subject tot he cohort effects discussed in the literature. This is discussed further in section B..

II. Benchmark Specification

A standard empirical formulation that captures the basic relationship between education and earnings is

$$\ln w_{it} = \alpha + \beta s_i + \mathbf{X}'_{it}\psi + a_i + \nu_{it} \tag{1}$$

where w_{it} is a measure of worker i's earnings at time t. Log-wages are a function of educational attainment, s_i, and ability, a_i. $\mathbf{X}_{it}$ is a vector of other wage determinants. ν_{it} is a random error component orthogonal to education. If workers with higher ability also have higher educational attainment, then education and unobserved ability are positively related: $cov(s_i, a_i) > 0$. A least squares estimation of equation (1) captures this covariance and overestimates β.

$$E[\hat{\beta}_{LS}] = \beta + \frac{cov(s_i, a_i)}{var(s_i)}$$

To see how instrumenting works, let $\bar{x}_c$ denote the mean level of x for cohort c. Conditioning log-wages on a set of cohort dummies, Z, we get,

$$E[\ln w_{it}|\mathbf{Z}] = \alpha + \beta\bar{s}_c + \bar{\mathbf{X}}'_{ct}\psi + \bar{a}_c + \varepsilon_c$$

where $\varepsilon_c = E[\nu_{it}|\mathbf{Z}]$. If one groups the sample into birth cohorts by taking the averages of all variables, and estimates the equation above, then the estimate of β obtained in this way is consistent, provided that the average ability of a cohort, $\bar{a}_c$, is orthogonal to the average schooling, $\bar{s}_c$. Estimation with grouped data is asymptotically equivalent to an IV approach where education is instrumented by group dummies[8]. This paper uses the IV method since it provides a more efficient estimator.

The average ability effectively contains all predetermined and unobservable components of earnings. Improved standards of living and higher literacy rates in growing economies may create a better learning environment at home. Likewise, improvements in the quality of pre-school education may also contribute to a potential cognitive development over time and induce a slow moving trend in the average ability. If there is a causal link between ability and educational attainment, then the identifying assumption of the model would be violated. In order to avoid a potential bias and capture these long term changes, a quadratic trend in the year of birth is included in the set of regressors.

[8]See Angrist (1991).

III. Data and Estimation Results

The data are taken from the Annual March Supplements to the Census Population Survey (CPS) for the years 1964 - 2003 and from the Decennial Census Surveys (DCS) for the years 1960 - 2000. The sample is restricted to the males of ages 25 - 60. A birth cohort is defined by the group of workers born in the same year. For each birth cohort, at least 10 years of observations for the CPS and two observations for the DCS are required. Hence the data include cohorts born as early as 1914 and as late as 1968 in the March CPS and cohorts that were born in 1910 to 1965 in the DCS.

Educational attainment is measured by the number of years of schooling. Wages are measured by weekly earnings, calculated by dividing the annual wage and salary earnings by the total number of weeks worked during the year. Observations that yield less than half the weekly earnings, based on a 40 hour week priced at the minimum wage in 2003, are dropped. Workers who spent less than 13 weeks at work during the year before the survey are also dropped.

Table 1 shows the descriptive statistics. The first column displays the frequency of cohorts in the sample. Due to the revolving nature of cohorts, the earliest and the latest cohorts constitute only a fraction of the sample. The average age of the newest cohort in the sample is 30, whereas the average age of the earliest cohort is 57. The average level of education increases quite steadily until the latest cohorts in the sample. A preliminary comparison of the average weekly earnings with the average educational attainment in the third column suggests a positive relation, which implies education may raise productivity [9]. However this indication is not conclusive since presence of a possible trend in average ability would make it harder to separate marginal effects of education by examining the pattern in the table.

Table 2 displays the estimation results for the benchmark model. The control variables are a quartic function of age, to capture the return to experience, a quadratic trend in the year of birth, to capture longer term trends in educational attainment and predetermined component of wages, and dummies for race and the survey year. The LS estimate of the return to a year of schooling is 8.2 percent, comparable to the estimates found in the literature. The second column displays the IV results. The true return to education is estimated to be 4.7 percent with a standard error of 1.32. The model is estimated under three different structural specifications of the covariance matrix. The observations are clustered by cohorts, survey year, and cohort–year interactions. The standard errors reported in Table 2 are obtained under the specification that allows arbitrary correlations within cohorts, and are the highest

[9] Since the age composition of cohorts are different, the earnings of early cohorts are bound to include the return to experience. Instead of reporting plain sample averages, wages are projected on a complete set of age, time, and race dummies.

$$w_{it} = \mathbf{D}'_T\eta_0 + \mathbf{D}'_A\eta_1 + \mathbf{D}'_R\eta_2 + \varepsilon_{it}$$

Once estimates are obtained, a wage level, $\tilde{w}$, is predicted, assuming that all observations are white males of age 40 and that they are all subject to an average time effect.

$$\tilde{w}_{it} = \sum_{j=1964}^{2003} \frac{\hat{\eta}_{0j}}{40} + \hat{\eta}_1 \times D_{40} + \hat{\eta}_2 \times D_{White} + e_{it}$$

of the three clustering methods[10]. Wu-Hausman test statistic using these standard errors for the difference between the two estimators is 7.05, suggesting that the IV estimate is significantly lower than the LS estimate. This is contrary to the estimates obtained in earlier studies.

Including a quadratic term in the years of education does not change the results. Third column of Table 2 shows that the LS estimate of the return to schooling is 9.1 percent compared to the IV estimate of 4.5 percent. The LS estimate of the quadratic term in education is positive indicating a convex relation between earnings and schooling. Card (2001) illustrates how heterogeneity in the return to education might generate a convex earnings–education profile, even though the marginal return to education is diminishing for each individual. If the first two moments of the individual specific returns to education are conditionally independent of the year of birth and the residuals from the first step estimation are linear in the individual specific heterogeneity component, then the IV approach consistently estimates the mean return to education. Furthermore, the coefficient of the quadratic term would estimate the curvature of the return to education[11]. The IV estimate presented in Table 2 is negative, indicating diminishing marginal returns to education.

Hausman's J statistics for overidentification are 192.4 and 188.2 in the benchmark formulation, in other words, different (sub)sets of instruments yield different estimates of the return to education. This is possible, for instance, if the skill premium is changing over time. Since most cohorts are observed in only a part of the sample, the estimates using these cohorts as instruments will give different results if return to education is not constant over time. Katz and Murphy (1992), among others, documents the rise in wage inequality among education groups in the second half of the century. In order to investigate this, the benchmark specification is estimated for ten-year intervals between 1964 and 2003 in the March CPS sample.

Table 4 shows the least squares estimation results . The LS return to schooling increased from 6.1 percent to 11.5 percent. The IV estimate of the return to education, displayed in the second row of Table 4, increased by 6.4 percentage points from 4.9 percent to 11.3 percent in four decades. IV estimates of the return to education increase over the sample period and the rise is more pronounced after 1980s. The IV regression applied to the whole sample then estimates a weighted average of the return to schooling over time, where the weights are increasing in the number of years a cohort is observed in the sample.

The second part of the table gives the results from the Decennial Census Survey. These results are reported to check the robustness of our results across different datasets, to improve the efficiency that is brought about by the higher sample size in the DCS, and to provide a basis of comparison between the benchmark specification and the more general specification discussed in the next section. The estimates of the rate of return are in general lower than the March CPS. The results from the DCS confirm that the IV estimate is significantly lower than the LS estimate.

The Wald test statistics from the first step regression are reported in the last rows of the table. The F-statistic for the cohort dummies is 46.07 in the March CPS and 326.8 in the

[10]The standard errors obtained with survey year clusters and cohort - survey year interaction clusters are 0.77 and 0.56 respectively.

[11]See Wooldridge (1997).

DCS, implying that the instruments perform well in explaining the variation in educational choices. Critical values provided in Stock and Yogo (2005) also confirm that any potential TSLS bias is far less than 5% of the OLS bias.

A. Instrumenting with the State Cohorts

Educational achievement may also depend on regional factors such as availability of nearby schools, local policies on education finance, state policies on compulsory education or even cultural attitude towards education. Changes in these local factors would affect schooling choices of a subgroup of workers in a birth cohort, and creates additional variation in educational achievement across cohorts by state or region. If one's state of birth captures these local conditions, then the return to education can be identified using birth cohorts by states. With a similar motivation, Angrist and Krueger (1991) exploit differences in age–related schooling requirements across states to improve the precision of their estimates.

Note that the state of birth need not capture the cost and benefit conditions of a cohort in *that* state. In a setting where individuals are mobile, state of birth may affect the set of educational choices for a cohort. Differences in costs of mobility or taste would generate an association between one's place of birth and his educational outcome. In addition, the state of birth should not affect earnings because the regional labor market conditions are captured by the *current* state of residence. Dummy variables for the state of birth are also included in the wage regression, therefore the estimate of the return to education is still identified by differences across cohorts.

Data used in this section is taken from the DCS. State cohorts are generated by interacting the state of birth with the year of birth[12]. Dummy variables for state of birth are also included as controls therefore the identification comes solely from variation across cohorts. Quadratic trend in the year of birth is extended to include a varying linear term across states[13]. This specification allows for regional time trends in the predetermined component of wages. Table 3 displays the estimation results. The IV estimate of the return to schooling in this case is %4.60, close to the benchmark specification and less than the LS estimate. Standard errors of the IV estimates decrease substantially, thanks to the higher sample size and the additional variation captured by state cohorts. The Wu-Hansen test rejects the hypothesis that IV and LS estimates are similar at 1% significance level.

B. Cohort Size and Congestion

The benchmark specification implicitly assumes that workers in different age and education groups are perfect substitutes for each other. If substitution of labor is imperfect across these workers, then a change in the labor supply of an age or education group has an equilibrium effect on the distribution of wages. For instance, a rise in the supply of higher education groups may decrease their earnings, and bias our estimates downward. However, quantitative impacts of these changes should be insignificant, because each birth cohort represents only a fraction of the labor force at any given time. Therefore, a larger fraction of an education group in a cohort will only have a second order effect on wages.

[12]There are 2754 (=51 States $\times$55 Cohorts-51 States) dummy variables to be used as instruments.

[13]Allowing for varying curvature does not alter the results.

Omission of changes in the labor supply from the wage regression could have a stronger effect on our estimates if they are related to educational attainment in a systematic way. It has been argued that the baby boom generation, at the outset of their career, had to accept lower wages since the market was overwhelmed with younger workers. Falaris and Peters (1992) argue that the congestion in the labor market induced some workers in larger cohorts to defer their entry to the market, and stay at school instead. If this is true, neglecting cohort size would bias the IV estimate of the return to education downward. On the other hand, one could also argue that a larger cohort may congest the education market, and raise the effective cost of education. Bound and Turner (2007) argue that larger cohorts receive lower public subsidies for education and hence have lower college attainment. Stapleton and Young (1988) point out that if substitutability between young and old workers diminish with education, workers with higher educational attainment in a large cohort will experience suppressed wages for a longer period, which in turn will reduce the incentive to acquire education. Both of these arguments would lead to lower average educational attainment by larger cohorts, and cause the IV estimate in the previous section to overstate the true return to education. Alternatively, larger cohorts could crowd classes, and reduce the effective quality of education captured by a year of schooling. This too would lead to overestimation of the return to education even if educational attainment, as measured by years of schooling, is not responsive to cohort size.

In order to account for the equilibrium effects of differential changes in labor supply across workers and the potential effects of cohort size, the control set is broadened to include measures of labor supply by age and education. At any time t, let N_{ast} denote the total labor supply of all workers in age group[14] a and education group s. A worker's earnings are the amount of efficiency units he governs , $h_{ic} = \exp(\beta s_{ic} + a_{ic})$, priced at the wage per efficiency unit, $\omega_{ast} = \frac{\partial Q_t}{\partial N_{ast}}$, where Q_t is the output function. Equation (1) becomes

$$\ln w_{it} = \alpha + \ln \omega_{ast} + \beta s_i + \mathbf{X}'_{it}\psi + a_i + \epsilon_{it} \tag{2}$$

where changes in labor supply will be reflected in ω_{ast}. Following the literature, a trans-log function is included in the regression to capture movements in $\omega_{ast.}$ Trans-log form is preferred for its flexibility to arbitrarily approximate different production functions.

$$\ln \omega_{ast} = \gamma_0 \ln N_{at} + \gamma_1 \ln N_{st} + \gamma_2 \ln N_{at} \times \ln N_{st} + \gamma_3 \ln^2 N_{at} + \gamma_4 \ln^2 N_{st}$$

Table 5 displays the estimation results. In all specifications the IV estimate of the return to education increases compared to the benchmark estimation, getting closer to (but still less than) the LS estimate.The return to education is 4.88% in the DCS sample and 6.38% in the CPS. The data support a positive covariance between education and cohort size and a negative covariance between wages and cohort size, supporting the entry deferral argument provided by Falaris and Peters (1992). Change in the IV estimate is particularly due to the omission of cohort size effects in the benchmark model, because identification of the return to education essentially depends on cohort-based comparisons of education and earnings. The LS estimate only slightly decreases when the general equilibrium effects are accounted

[14] Since age, time and birth year are linearly dependent one can equivalently use cohort size keeping time constant, $N_{cst} = N_{ast}$.

for.

Table 6 reports the estimation results using the state–cohort dummies as instruments for education. Here, the IV estimate of the return to education is virtually the same, %4.85, as the benchmark specification. This is not surprising since the cohort size effects on wages are general equilibrium effects and they operate at a national level. Since the identification with state cohorts comes from relative changes in educational attainment across cohorts at the state level, they are less prone to these effects. The estimate here also has a standard error that is four times less. This enables us to provide improved confidence in the Wu-Hansen test statistic which now rejects the similarity to LS estimate at 1% significance.

Estimation results in Tables 5 and 6 indicate an ability bias of 1-3 percentage points, depending on the dataset and specification. Since the LS return is still around 8.0 percent, the increase in actual marginal product of a worker induced by a year of education constitutes 60 - 90% of the observed wage differences.

IV. Conclusion

Standard estimates of the return to education overstate the true causal relation from education to earnings if unobserved components of productivity are positively related to educational attainment. A strand of the literature tries to eliminate ability bias by using instruments that explain schooling choices but are orthogonal to unobserved components. Estimates in this literature are oddly higher than the LS estimate creating skepticism toward the validity of the instruments used in these studies and/or instrumental variables approach in general. Results presented in this paper suggest that using birth cohorts to instrument educational attainment generates estimates that are significantly lower than the LS estimate. In particular, an additional year of schooling increases a worker's market productivity by approximately 5 percent, which is 3 percent lower than the LS return. Including a quadratic trend in the year of birth helps identify the true return to education independent of a potential trend in the pre-determined component of wages. Controlling for long term trends in educational attainment and wages is essential to generating lower IV estimates compared to the LS estimates.

Instrumenting education with birth cohorts by state reassures the validity of birth cohorts as instruments and improves the efficiency of our estimates. In an attempt to further check the robustness of the estimates, we allow for imperfect substitution of labor across different age and education groups. Accounting for the equilibrium effects only slightly increases the estimate of the true return to education and the estimates remain lower than the LS estimate.

In the presence of heterogeneity in the rate of return to schooling, the IV approach would effectively estimate the rate of return of those workers that are more sensitive to the instrument. The identification strategy here is more mechanical and does not depend on a specific determinant of education. Therefore, the IV estimate presented here can be considered as a weighted average of different rates of return, where weights depend on the underlying cause of changing educational attainment and on the relative sensitivity of workers to the changing conditions. Card (2001) demonstrates how the IV estimator may be misleading if the instruments induce a single affected subgroup of individuals. Especially in cohort-based studies, where the treatment group is usually a single cohort of workers, the IV estimator is vulnerable to a potential bias if there is a strong cohort effect on the treatment group relative to the comparison cohorts. The IV estimates presented here are more reliable, because they are based on the comparison of many cohorts that are classified by year of birth and/or place of birth.

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APPENDIX

Both Decennial Census Surveys 1960 - 2000 and Annual March supplements to Current Population Survey (March - CPS) for years 1964 - 2003 are extracted from IPUMS - USA database(Steven Ruggles and Ronnander, 2004). Education is measured by years of completed schooling. March - CPS collected years of schooling data until 1992, after which the coding was changed to a categorical variable including information on the degree obtained. All observations after 1991 were recoded to maintain consistency across years[15]. Total number of weeks worked were categorized before 1976. Each category is replaced by the average number of weeks worked in surveys after 1976. Sample is limited to workers who worked 14 weeks or more during a year. Total wage and salary income is topcoded for anonymity reasons in the data and topcodes changed almost every year. Beginning in 1996 Census started reporting the average income of workers above the topcode instead of the topcode itself. Topcoded earnings are replaced by an estimated mean under the assumption of log-normality[16]. Average wage inflation is used to calculate real income in 2003 prices. Using CPI to calculate real values does not change the results.

[15]The completed number of years of schooling is equated to the grade completed. If completed grade is 1^{st} to 4^{th}, number of years of schooling completed is set to 3 and for $5^{th}-8^{th}$ grades it is set to 7. These are the median number of years completed for the survey years prior to 1992. All degrees beyond college are set to 16 years of education.

[16]Standard cure for this right censoring is to multiply the topcoded income values by a constant number (1.4, for instance, in Katz and Murphy (1992)). Estimates obtained here suggest that average ratio is 1.36, but it varies from 1.2 to 1.7.

Table 1: Descriptive Statistics

CPS March Supplement 1964 - 2003

Birth Cohort	Percent Frequency	Average Age in Sample	Average Education	Weekly Earnings
1959 – 1968	18	33	13.3	6.91
1949 – 1958	26	37	13.4	6.96
1939 – 1948	24	41	13.0	7.01
1929 – 1938	17	44	12.1	6.97
1919 – 1928	12	49	11.4	6.92
1914 – 1918	3	53	10.7	6.87
Total	100	40	12.7	7.01

Decennial Census 1960 - 2000

Birth Cohort	Percent Frequency	Average Age in Sample	Average Education	Weekly Earnings
1960 – 1965	11	33	13.4	6.89
1950 – 1959	23	37	13.5	6.93
1940 – 1949	23	41	13.2	7.00
1930 – 1939	18	42	12.1	6.98
1920 – 1929	16	45	11.3	6.94
1910 – 1919	9	50	10.3	6.88
Total	100	40	12.5	6.95

Table 2: Estimation Results – Benchmark Specification

	March CPS				Decennial Census			
Explanatory Variables	LS	IV	LS	IV	LS	IV	LS	IV
$100 \times educ$	**8.18**	**4.70**	**9.14**	**4.47**	**7.49**	**2.40**	**8.98**	**2.41**
	0.16	*1.32*	*0.15*	*1.43*	*0.17*	*1.02*	*0.17*	*1.04*
$100 \times educ^2$	..	..	**0.36**	**-0.47**	..	..	**0.44**	**0.03**
	..	..	*0.02*	*0.53*	..	..	*0.02*	*0.29*
First Step Wald Test								
educ	46.07		46.07		326.80		326.80	
$educ^2$	..		6.15		..		16.92	
Wu-Hansen Test	7.05*		10.78*		25.61*		41.00*	
N	943,760		943,760		5,227,984		5,227,984	

Notes: *Significant at 1 percent level. Standard errors are clustered by cohort and reported in italics. Control variables are a quartic function in age, a quadratic trend in the year of birth (by state for the DCS data) and dummy variables for race and survey year.

Table 3: Estimation Results – Instrumenting with State Cohorts

Explanatory Variable	LS	IV	LS	IV
$100 \times educ$	**7.10**	**4.60**	**8.32**	**4.85**
	0.17	*0.34*	*0.15*	*0.66*
$100 \times educ^2$			**0.47**	**0.06**
			0.02	*0.09*
First Step Wald Test				
$educ$	72.26		72.26	
$educ^2$	..		57.42	
Wu-Hansen Test	76.91*		29.15*	
N	5,227,984		5,227,984	

Notes: *Significant at 1 percent level. Standard errors are clustered by cohort and are reported in italics. Data is taken from the Decennial Census Surveys 1960 - 2000. Control variables are a quartic function in age, a quadratic trend in the year of birth by state, and dummy variables for race, survey year, state of residence and state of birth.

Table 4: Estimation Results – Rising Skill Premium

	1964 – 1973	1974 – 1983	1984 – 1993	1994 – 2003
Least	6.12	6.57	8.84	11.52
Squares	*0.07*	*0.17*	*0.12*	*0.14*
Instrumental	4.87	-0.29	6.37	11.31
Variables	*1.98*	*1.54*	*2.02*	*2.05*

Notes: Standard errors in italics.Data comes from the March supplements to Census Population Survey, 1964 - 2003.

Table 5: Estimation Results: General Specification

Explanatory Variables	March CPS				Decennial Census			
	LS	IV	LS	IV	LS	IV	LS	IV
$100 \times educ$	**8.29**	**6.38**	**8.63**	**7.94**	**7.86**	**4.88**	**8.79**	**5.20**
	0.19	*2.50*	*0.12*	*2.48*	*0.21*	*1.72*	*0.15*	*1.69*
$100 \times educ^2$	..	..	**0.45**	**-0.91**	..	..	**0.50**	**-0.16**
	..	..	*0.02*	*0.54*	..	..	*0.03*	*0.26*
lnN_{at}	-0.68	-0.30	-0.97	-0.18	2.22	2.56	2.52	2.38
	0.64	*0.61*	*0.67*	*0.61*	*1.10*	*1.03*	*1.16*	*1.08*
lnN_{st}	-1.27	-0.80	-0.43	-3.07	-1.17	0.24	-0.15	0.87
	0.12	*0.62*	*0.06*	*1.37*	*0.13*	*0.51*	*0.06*	*1.25*
$lnN_{at} \times lnN_{st}$	0.06	0.06	0.04	0.10	0.05	0.04	0.01	0.05
	0.01	*0.01*	*0.00*	*0.02*	*0.01*	*0.01*	*0.00*	*0.03*
$10 \times ln^2 N_{at}$	-0.09	-0.24	0.12	-0.51	-1.13	1.18	-1.02	-1.20
	0.26	*0.24*	*0.28*	*0.28*	*0.40*	*0.38*	*0.43*	*0.38*
$10 \times ln^2 N_{st}$	0.22	0.07	-0.02	0.73	-0.16	0.08	-0.09	0.05
	0.04	*0.21*	*0.02*	*0.41*	*0.02*	*0.13*	*0.02*	*0.27*
First Step Wald Test								
educ	58.24		58.24		171.9		171.9	
$educ^2$	..		28.59		..		71.69	
Wu-Hansen Test	0.59		0.08		3.05***		4.55**	
N	943,760		943,760		5,227,984		5,227,984	

Notes: **, *** Significant at 5 and 10 percent level, respectively. Standard errors are clustered by cohort and reported in italics. Additional controls are a quartic function in age, a quadratic trend in the year of birth (by state for the DCS data) and dummy variables for race and survey year.

Table 6: Estimation Results: General Specification with State Cohorts

Explanatory Variable	LS	IV	LS	IV
$100 \times educ$	**7.50**	**4.85**	**8.05**	**5.27**
	0.21	*0.43*	*0.13*	*0.57*
$100 \times educ^2$			**0.55**	**0.09**
			0.02	*0.08*
lnN_{at}	1.63	6.70	1.97	2.70
	0.81	*6.34*	*0.83*	*0.94*
lnN_{st}	-1.21	-0.48	0.23	-0.34
	0.09	*0.14*	*0.04*	*0.13*
$lnN_{at} \times lnN_{st}$	0.05	0.05	0.01	0.04
	0.01	*0.01*	*0.00*	*0.00*
$10 \times ln^2 N_{at}$	-0.92	-2.73	-0.08	-1.24
	0.03	*0.23*	*0.03*	*0.34*
$10 \times ln^2 N_{st}$	0.02	0.00	-0.01	-0.06
First Step Wald Test				
educ	254.41		254.41	
$educ^2$	..		391.05	
Wu-Hansen Test	49.88*		25.56*	
N	5,227,984		5,227,984	

Notes: *Significant at 1 percent level. Standard errors are clustered by cohort and are reported in italics. Data is taken from the Decennial Census Surveys 1960 - 2000. Additional controls are a quartic function in age, a quadratic trend in the year of birth by state, and dummy variables for race, survey year, state of residence and state of birth.

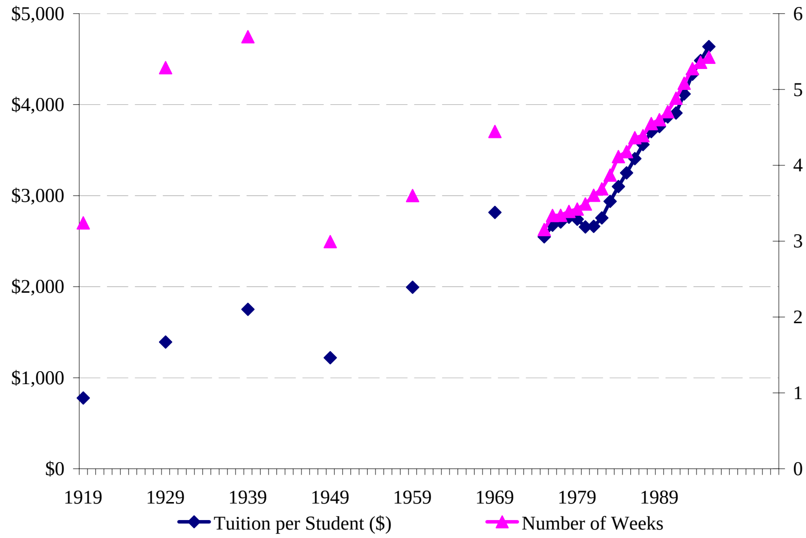

Figure 1: Average Tuition Cost

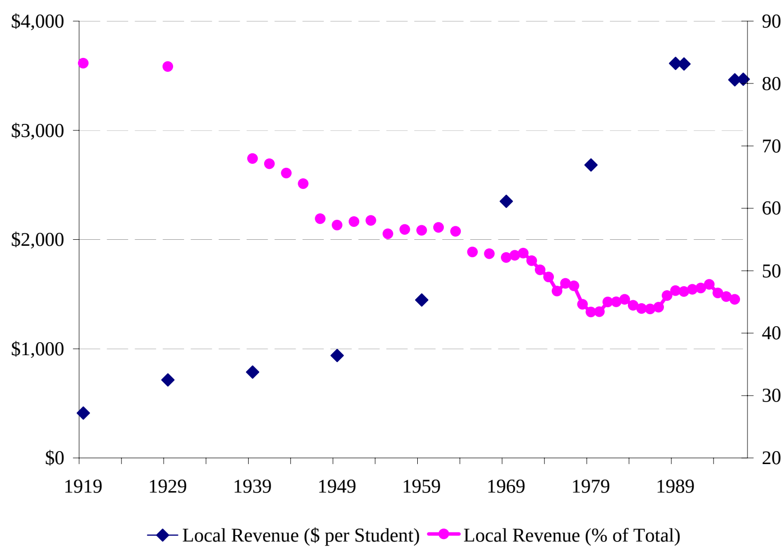

Figure 2: Local Revenues of High Schools

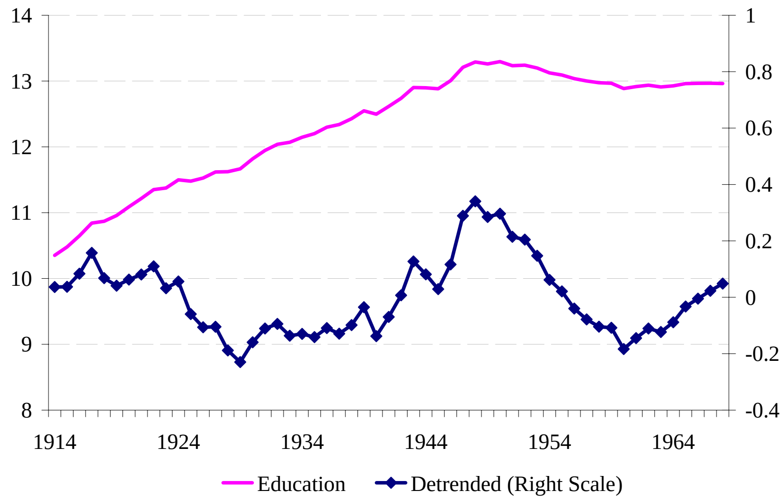

Figure 3: Average Education Across Birth Cohorts (March CPS)

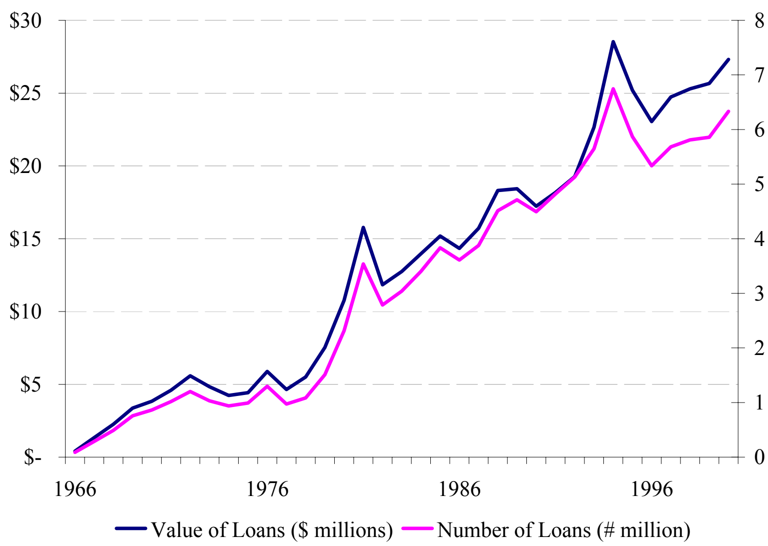

Figure 4: Federal Family Education Loans: Volume and Accessibility