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Many Types of Human Capital and Many Roles in U.S. Growth: Evidence from County-Level Educational Attainment Data*

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Abstract

We utilize county-level data to explore the roles of different types of human capital accumulation in U.S. growth determination. The data includes over 3,000 cross-sectional observations and 39 demographic control variables. The large number of observations provides enough degrees of freedom to obtain estimates for the U.S. as a whole and for 32 states in and of themselves. This data contains measures of educational attainment for four distinct categories: (a) 9 to 11 years, (b) high school diploma, (c) some college and (d) bachelor degree or more. These variables represent directly measured human capital stocks for each and every county. This is a departure from much of the economic growth literature which has (at least in part) relied on extrapolation of stocks from flows, e.g. school enrollment data. We use a consistent three stage least squares estimation procedure. We find that (i) the percentage of a county's population with less than a high-school education is negatively correlated with economic growth, (ii) the percentage obtaining a high school diploma is positively correlated with growth, and (iii) the percentage obtaining some college education has no clear relationship with economic growth but (iv) the percentage that obtains a bachelor degree or more is positively correlated with growth. Further, we find that (v) there is significant qualitative heterogeneity in estimated coefficients across states for the 9 to 11 years and high school diploma categories but (vi) no qualitative heterogeneity for the college level categories. The most consistent conclusion across samples is that the percent of a county's population obtaining a bachelor degree or higher level of college education has a positive relationship with economic growth. Oddly enough, despite findings (ii), (iv) and (vi) above, we find that the percentage of a county's population employed in educational services is negatively correlated with economic growth.

1. Introduction

Does human capital accumulation contribute to economic growth and in what way(s)? This has been a fundamental query for policy-makers, educators and scholars.

One perspective from which to view this query is a macroeconomic one, and the view from this perspective is not straightforward. Human capital accumulation may allow a populace to better obtain and use the technologies already existing world-wide (raising an economy's balanced growth *path*), or instead it may allow a populace to better produce new, previously-nonexistent technologies (raising an economy's balanced growth *rate*). The perspective is further clouded by data limitations. For many economies, human capital *stock* data is unavailable and must be extrapolated (imperfectly) from human capital flows, creating measurement error. Furthermore, there are many types of human capital with potentially many different contributions to economic growth. Macroeconomic data often aggregates away this heterogeneity.

This paper analyses the role of human capital accumulation in U.S. income growth determination. County-level data allows us to explore different roles of different types of human capital accumulation. The data includes over 3,000 cross-sectional observations and 39 demographic control variables. The large number of observations provides enough degrees of freedom to provide estimates for the U.S. as a whole and for 32 states in and of themselves. Our human capital measures are of educational attainment and cover four distinct categories: percent of a population with (a) 9 to 11 years of school and no more, (b) with a high school diploma and no more, (c) with some college, and (d) with a bachelor degree or more. We find significant heterogeneity in estimated effects across the four types of human capital, as well as across individual U.S. states. Only one estimated effect of human capital in growth determination is (i) clearly detected in the full U.S. sample and (ii) robust in that no state-wide sub-sample provides statistically significant evidence to the contrary: *the percent of a county's population having attained a bachelor degree or higher level of education is positive associated with economic growth.*

The variables we use as a measure of human capital offer two important advantages. First, they represent human capital stocks for each and every county. This is a departure from much of the economic growth literature that has (at least in part) relied on extrapolation of stocks from flows.[1] For example, Mankiw et al (1992) use school-enrollment rates as an investment proxy for human capital stocks in cross-county growth regressions.[2,3] As well, Kyriacou (1991) and Barro and Lee (1993) combine limited educational attainment observations with school-enrollment data to estimate human capital stocks for international samples. Second, our four-level categorization provides a distinction between different levels/types of human capital accumulation. Mankiw et al (1992) use data on high-school enrollment in their regressions. Klenow and Rodriguez-Claire (1997) add elementary and college enrollment variables to their analysis. Our data adds a distinction between more or less college-level education that we find to be economically important in our results.[4] It should also be noted that none of the above studies use county-level data.

Besides the fine categorizations of human capital levels/types and the exceptionally large number of cross-sectional observations, the data offers numerous other advantages over the international data often used in identifying growth determination processes. A single institution collects the data, ensuring considerable uniformity of variable definitions. There is no exchange rate variation between the counties and the price variation across counties is smaller than across countries. Also, U.S. counties are characterized by exceptional mobility of technology, resources and factors of production. Of course, many of these advantages are embodied in U.S. state-level data used by, e.g., Barro and Sala-i-Martin (1991) and Evans (1997a). However, the state level data sacrifices the large number of observations that the county-level data offers. This large number of observations, and accompanying degrees of freedom, allows

[1] See Wößmann (2003) for a recent survey of human capital stock measures used in empirical studies of economic growth.

[2] Mankiw et al (1992), working from the neoclassical growth model, actually derive a regression specification in terms of physical and human capital flows instead of stocks. However, doing so requires an assumption that countries in their sample are currently on their balanced growth paths.

[3] We also have variables representing school enrollment at public elementary level, private elementary level, public nursery level, and private nursery level. These variables are included in the full U.S. sample regressions. However, the related coefficient values are estimated to be 0 to four decimal places. Therefore, to preserve degrees of freedom, they are not included in the within-state regressions.

[4] Barro and Lee (1993) develop a 7-level categorization, adding finer distinction within the elementary and high-school level years of attainment. However, their measures are extrapolated from flows for over 50 percent of their observations and are for a sample of 129 countries. Our measures are stocks and focus entirely on one country: the U.S. Further, Barro and Lee do not pursue growth regressions in their own study.

for a more detailed analysis of the roles of human capital in the U.S. by addressing inter-state heterogeneity for 32 states.[5]

The primary contribution of this paper to the empirics of human capital and growth lies in the extensive dataset that we have constructed. However, we also utilize a cross-sectional variant of a three stage least squares (3SLS) approach suggested recently by Evans (1997b) for estimating growth equations consistently.[6]

Both Evans (1997b) and Caselli et al (1996) demonstrate that data must satisfy highly implausible conditions for OLS estimators to be consistent for growth regressions. Caselli et al advocate a panel data generalized methods of moments (GMM) estimator that differences out omitted variable bias and instruments to alleviate endogeneity concerns. This is an attractive method when enough time period observations are available after differencing. With our data set, however, the conditioning variables are collected only at 10 year intervals so, for example, we would only have 2 time series observations in applying Caselli et al's method. Our data's appeal lies in the exceptionally large cross-sectional dimension and, therefore, a cross-sectional estimation technique makes more sense.[7] We employ the Evans (1997b) 3SLS technique to identify the structure of growth processes and, for comparison, report OLS estimates as well.

The neoclassical specification employed in this paper plays a role in interpreting human capital's role(s) in growth determination. As mentioned above, human capital can be hypothesized to have a role in technology *adoption* (balanced growth path effect) and/or technology *development* (balanced growth rate effect). These effects are, of course, not mutually exclusive, but understanding which, if either, is predominant seems to be important. Benhabib and Spiegel (1994) explore this question for a cross-country sample and find that the adoption effect is more important. Our neoclassical specification allows us to consider the same question for the U.S. in and of itself. As detailed in Section 2 below, with the average growth of income as the dependent variable, the estimated value of the coefficient on the initial income level provides a discriminating

[5] A full 29 of our states each have counties numbering more than 50 (the number of U.S. states).
[6] We have previously employed this 3SLS estimation with the county-level data to study convergence within the U.S. (Higgins et al, 2003) and heterogeneity in convergence rates across U.S. states (Young et al, 2003).
[7] Evans (1997a) also develops a panel data variant of his method and applies it to U.S. state and international data.

test. A negative value suggests that the neoclassical growth model (i.e. human capital aids adoption) is the better approximation to reality than an endogenous growth model (i.e. human capital aids development). As outlined below, and documented fully in Young et al (2003), this coefficient is negative and significant for the full U.S. sample and negative for every individual state sample where the value was statistically significant. This allows us to not only ascertain the correlation of human capital types with economic growth, but also to offer an interpretation supported by the data. The criterion we use for reporting 32 states is that their regressions yield significant initial income coefficient estimates. In all these 32 cases, the estimate is negative thus supporting the neoclassical specification.

Using county-level data on human capital stocks and the consistent 3SLS estimation, we find that (i) the percentage of a county's population with less than a high-school education is negatively correlated with economic growth, (ii) the percentage obtaining a high school diploma is positively correlated with growth, (iii) the percentage obtaining some college education has no clear relationship with economic growth but (iv) the percentage that obtains a bachelor degree or more is positively correlated with growth. Further, (v) there is significant qualitative heterogeneity in estimated coefficients across states for the 9 to 11 years and high school diploma categories but (vi) no qualitative heterogeneity for the college level categories. A broad conclusion is that high levels of human capital accumulation in a population are conducive to the adoption and application of available technologies, and that this represents a positive contribution to economic growth via a higher balanced growth path.

An additional, and surprising, finding that we briefly discuss in this paper concerns another demographic variable included in our regressions: percentage of a county's population employed in the education services industry. Based on the above findings we might expect the coefficient on this variable to be positive. In fact, we report that it is negative and significant for the full sample *and* for every individual state sample where the estimate is significant at the 10 percent level or better! This is consistent with the view that the variation in resources devoted to schooling does not have clear positive relationship with benefits to be gained from education (Hanushek and Rivkin (2004), Hanushek (1996), Hanushek et al (1996) and Wößmann (2001)).

The paper is organized as follows. Section 2 discusses the econometric specification of the neoclassical growth regression and the 3SLS technique we employ. Section 3 describes the county-level data. Section 4 briefly describes the convergence rate parameter estimates on which our interpretation of human capital coefficient estimates rests. The estimates of contributions from different types/levels of human capital for full and individual U.S. state samples are presented and discussed in section 5. Also in this section, we examine the average state-level SAT scores to see if the observed heterogeneity in human capital effects correlates with school quality/student achievement. Section 6 briefly touches upon the finding that the prevalence of educational service employment is negatively correlated with economic growth. Section 7 concludes.

2. Econometric Model and 3SLS Estimation Procedure

The basic specification used here and in other cross-sectional growth regressions arises from the neoclassical growth model of Ramsey (1928), Solow (1956), Swan (1956), Cass (1965) and Koopmans (1965).[8] The growth model implies that,

$$\hat{y}(t) = \hat{y}(0)e^{-Bt} + \hat{y}^{*}(1 - e^{-Bt}) \tag{2.1}$$

where $\hat{y}$ is log of income per effective unit of labor (technology assumed to be labor augmenting), t is the time period (0 being the initial time period), and B is a nonlinear function of the economy's discount (average, subjective), population growth, and technological growth rates, as well as preference parameters. B governs the speed of adjustment to the steady state. The $\hat{y}^{*}$ is the economy's steady-state log level of income per effective unit of labor. From (2.1) it follows that the average growth rate of income per unit of labor between dates 0 and T is,

$$\frac{1}{T}\left(y(T) - y(0)\right) = z + \left(\frac{1 - e^{-BT}}{T}\right)\left(\hat{y}^{*} - \hat{y}(0)\right) \tag{2.2}$$

[8] A derivation of the baseline specification from the growth model is provided by Barro and Sala-i-Martin (1992).

where z is the exogenous rate of technical progress and B represents the responsiveness of the average growth rate to the gap between the steady state of log income per effective unit of labor and the initial value. Since effective units of labor (L) are assumed to equal Le^{zt}, we have $\hat{y}(0) = y(0)$.

From this model, growth regressions are usually obtained by using OLS to fit cross-sectional data on economies 1,…, N to the equation,

$$g_n = \alpha + \beta y_{n0} + \gamma' x_n + \nu_n . \tag{2.3}$$

In (2.3), g_n is the average growth rate of per capita income for economy n between years 0 and T [i.e., $\frac{1}{T}\left(y(T) - y(0)\right)$]; α is a constant that is a function of z, $\beta = \left(\frac{1-e^{-BT}}{T}\right)$; x_n is a vector of variables that control for cross-economy heterogeneity in determinants of the steady-state, $\hat{y}^*$; γ is a vector of coefficients on those variables; and ν_n is the error term assumed to have zero mean and finite variance.

An estimate of β provides a discriminating test between the neoclassical growth model, where technological change is exogenous and, therefore, human capital affects growth through better adoption of existing technologies, and endogenous growth theories where human capital accumulation may affect technological change directly. If $\beta < 0$, then γ describes how the x_n affect the height of economy n's balanced growth *path*. Otherwise, γ describes how the x_n affect n's balanced growth *rate* (Evans, 1997b, pp.3-4).

However, Evans (1997b) shows that OLS estimates of β and γ will be consistent only when the data satisfy highly implausible conditions. Plausible departures from these conditions can produce large biases. Specifically, Evans demonstrates that OLS estimators are inconsistent unless (i) the dynamical structures of the economies examined have identical, first-order autoregressive representations, (ii) every economy affects every other economy symmetrically, and (iii) the set of conditioning variables controls for all permanent cross-economy differences.[9]

[9] For some technical details concerning the claim of OLS inconsistency, please see the econometric appendix below. We also thank Paul Evans for answering questions concerning these details and those of his 3SLS procedure.

Evans (1997b) proposes a 3SLS instrumental variables approach that consistently estimates the speed of convergence and the effects of conditioning variables. We use a cross-sectional variant of his method. The method consists of three stages. In the first and second stages we use instrumental variables to estimate the equation,

(2.4) $$\Delta g_n = \omega + \beta \Delta y_{n0} + \eta_n ,$$

where

$$\Delta g_n = \frac{(y_{n,T} - y_{n,0})}{T} - \frac{(y_{n,T-1} - y_{n,-1})}{T} ,$$

$\Delta y_{n0} = y_{n0} - y_{n,-1}$, y_n is the logarithm of per capita income for county n, ω and β are parameters, and η_n is the error term. We use the lagged (1969) values of all the independent variables as instruments, with the exception of Metro Area, Water Area, and Land Area.[10] Given the sample period we use here, we define,

$$\Delta g_n = \frac{(y_{n,1998} - y_{n,1970})}{T} - \frac{(y_{n,1997} - y_{n,1969})}{T} .$$

Next, define β^* as the estimator obtained from equation (2.4). In the third stage, we take the estimate for β^*, multiply it by y_{n0} and then subtract the product from g_n. This yields a variable,

(2.5) $$\pi_n = g_n - \beta^* y_{n0} ,$$

which is then regressed (using OLS) on an intercept and the vector of variables, x_n, that are potential influences on balanced growth path levels. This third-stage regression is of the form,

(2.6) $$\pi_n = \tau + \gamma x_n + \varepsilon_n ,$$

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[10] See the data appendix for details.

where τ and γ are parameters and ε_n is an error term. This regression yields a consistent estimator, γ^*.

What this two stage procedure essentially does is, in the first and second stages, differences out any uncontrolled form of heterogeneity from the specification so that an omitted variable bias does not occur[11] and then, in the third stage, uses the resulting estimate of β to recreate the component of a standard growth regression that is related to the set of conditioning variables. This component can then be regressed on a constant and the conditioning variables, in "un-differenced" form, to estimate the effects of conditioning variables on balanced growth paths. This procedure ensures that none of the information contained in the levels of the conditioning variables is lost.[12]

Besides reporting OLS results below, as well as 3SLS results, for comparison, we also use a Hausman test as an additional aid in the determination of the appropriateness of the instrumental variable approach for the full U.S. sample. Two separate tests were performed. The first test was run on the β values and yielded an m value of 134.6. The second test was run on the entire model and yielded an m value of 1236.6. Indeed, both tests reject the null hypothesis at the 1% level, thereby suggesting that the OLS estimates are inconsistent.

3. U.S. County-Level Data

The data for this study were drawn from several different sources. The majority of the data, however, came from the Bureau of Economic Analysis Regional Economic

[11] The derivation of this equation (see Evans (1997b)) depends on the assumption that the conditioning variables are (approximately) constant during the time frame considered, allowing them to be differenced out. We are indebted to Nazrul Islam for pointing out that, while this is a reasonable assumption for many conditioning variables in the literature (e.g., an index of democracy for an international sample over 15 years), many of our county-level conditioning variables potentially vary significantly (e.g., the percent of the population employed in the communications industry over 28 years). To make sure that this did not introduce significant omitted variable bias into our estimations we ran the IV regressions with differenced values of all conditioning variables included as regressors. All point estimates of β from the modified first and second stages fell within the 95 percent confidence intervals of the Evans method estimates. As well, if the β estimates are not significantly affected then neither are the second stage results (see below).

[12] This is a point on which Barro (1997, p.37) criticizes panel data methods. As they rely on time series information, the conditioning variables are differenced. However, the conditioning variables often vary slowly over time such that the most important information is in the levels.

Information System (BEA-REIS) and U.S. Census data sets.[13] The BEA-REIS data are largely based on the 1970, 1980 and 1990 decennial Census summary tape files, the 1972, 1977, 1982 and 1987 Census of Governments, the Census Bureau's City and County Book from various years. All dollar variables are expressed in constant 1992 prices. Natural logs were used throughout the project. We exclude military personnel from the measurements of both personal income and population.

Our entire data set includes 3,058 county-level observations.[14] (See **Map 1** for the outlines of county boundaries across the Continental U.S..) We examine the full sample, as well as U.S. states as economic units in and of themselves. We report estimation results for 32 of the 50 states. The standard we used for inclusion was whether or not, in the IV regressions, the estimate for β was statistically different from zero.

The measure we use for personal income is that of the U.S. Bureau of Economic Analysis (BEA).[15] The definitions that are used for the components of personal income for the county estimates are essentially the same as those used for U.S. national estimates. For example, the BEA defines "personal income" as the sum of wage and salary disbursements, other labor income, proprietors' income (with inventory valuation and capital consumption adjustments), rental income (with capital consumption adjustment), personal dividend income and personal interest income. (BEA, 1994) "Wage and salary disbursements" are measurements of pre-tax income paid to employees. "Other labor income" consists of payments by employers to employee benefit plans. "Proprietors' income" is divided into two separate components—farm and non-farm. Per capita income for a county is defined as the ratio of this personal income measure for the county to the population of the county. We adjust the personal income measure to be net of

[13] We thank Jordan Rappaport for kindly sharing with us some of the data used in this study.
[14] The original data set contained 3,066 observations. Eight counties, however, were excluded from the data set for various reasons. Primarily, counties were excluded for lack of data. Examples of counties that fell into this category include counties in northern Alaska and some counties in Hawaii. Some data for these counties were simply not recorded as far back as 1970. Furthermore, in Virginia, some cities are themselves independent counties. If the data for these independent cities were available we let them stand as their own county. However, if the data were not available, then we tried to incorporate the independent city into the surrounding county. If that was not feasible, it was then dropped from the data set.
[15] The data and their measurement methods are described in detail in "Local Area Personal Income, 1969–1992" published by the BEA under the Regional Accounts Data, February 2, 2001.

government transfers and express the value in per capita 1992 dollars using the U.S. GDP deflator. Natural logs of the real per capita income measures are used in the analysis.[16]

In addition to the per capita income variable we also utilize 39 demographic conditioning variables. In **Table 1** we provide the complete list of the variables we use in this study along with their definitions. In the table we also provide the source of each series as well as the period it covers. All 39 of these variables were used for estimation using the full sample. However, only 33 of these were used for the with-in state estimations to preserve degrees of freedom. Our standard for exclusion was that a conditioning variable, in the third stage regression using the full sample, resulted in a coefficient estimate with zeros to at least the fourth decimal place (0.0000). The variables excluded from the within-state regressions were "land area," "water area," "education: public elementary," "education: public nursery," "education: private elementary," and "education: private nursery."

The variables that we focus on in this paper are four educational attainment variables: "Education: 9-11 years," "Education: H.S. diploma," "Education: Some college," and "Education: Bachelor +."[17] Each variable represents the percentage of a county's population that has obtained its named level of education and no higher. The delineations and cut-offs for educational attainment are intuitive and also those used by the U.S. census. Initial values for each of the four variables were collected from the 1970 U.S. Census tapes. The data is based on self-reported values from the census surveys.

4. Motivating a Neoclassical Interpretation of Human Capital

The OLS and 3SLS estimates of β, the coefficient on the log of 1970 per capita income, are presented in **Table 2** for the full U.S. sample and for 32 individual U.S. states. The speed of conditional convergence can be inferred from β. Associated with

[16] See the Data Appendix at the end of this paper for more detailed descriptions of the personal income measure.
[17] In addition, we analyze the effect of a variable measuring the percent of a county's population employed in education services on growth (see section 6).

these estimates of β, **Table 3** reports the asymptotic (conditional) convergence rates and corresponding 95 percent confidence intervals.[18]

For the full sample, the significant and negative β estimate provides support for the neoclassical growth model and facilitates our interpretation of human capital types as aiding in the adoption and use of available technologies. The same applies to the 32 individual U.S. states. This interpretation holds that human capital accumulation, if contributing positively to economic growth, does not do so by increasing the long-run, balanced growth rate. Rather, human capital allows for a given county to more effectively obtain, install and apply existing technologies. Despite not affecting the balanced growth rate, such affects are still important because they speak to how quickly poor counties can catch up to their wealthier counterparts.

The convergence rate estimates in **Table 3** provide a point of reference demonstrating that quantitative differences that arise from using the consistent 3SLS technique rather than OLS. For the full sample of 3,058 counties the 3SLS point estimate of the conditional convergence rate is 6.82 percent and is significant at the 1 percent level. This is compared to 2.37 percent using the inconsistent OLS method (also significant at the 1 percent level). The OLS 2.37 percent is similar to results that Barro and Sala-i-Martin (1992), Mankiw et al (1992), and Sala-i-Martin (1996) report. Sala-i-Martin (1996) has noted that a roughly 2 percent convergence rate is so commonly found in international, inter-state and inter-regional growth regressions that it qualifies as a "mnemonic rule." The difference between the OLS and 3SLS estimate is nearly 300 percent. This suggests that OLS introduces substantial bias.

The difference is economically large. A 2.37 percent convergence rate implies the gap between the present per capita income level and the balanced growth path halves in 31 to 32 years, while a 6.82 percent rate implies the same in 12 to 13 years.

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[18] Following Evans (1997b, footnote 17, p.16), we use $c = 1 - (1 + T\beta)^{\frac{1}{T}}$ to compute the asymptotic rate of convergence. The confidence intervals (in parentheses) are obtained in two steps. First, we obtain end points of the β confidence intervals by computing $\beta \pm (1.96 \times s.e.)$, where $s.e.$ is the standard error associated with the β estimate. Next, these endpoints are plugged into $c = 1 - (1 + T\beta)^{\frac{1}{T}}$. If the low value of the confidence interval is less than $-T^{-1}$, the higher value is set equal to 1. It is clear from the above that the confidence intervals computed this way may be asymmetric around the point estimates. As **Figure 1** indicates, this is indeed the case in our data.

The basic finding that conditional convergence rates are higher than the 2 percent "mnemonic rule" of Sala-i-Martin (1996) holds when examining 32 states as economies in and of themselves. **Figure 1** presents confidence intervals as vertical bars (that include the point estimates). The 2 percent rule is represented by a horizontal line. Every point estimate is above 2 percent, and the average point estimate is 8.1 percent. For one fourth (8) of the states the point estimate is above 10 percent.[19] Considering the 95 percent confidence intervals, we find that only 3 states have a lower bound of the confidence interval not greater than 2 percent (California, Iowa, and South Dakota all bottom out at 1.8 percent). These results are encouraging for laggard counties in the limited sense that, *given proper policies/conditions to induce and support balanced growth paths similar to leader counties*, the laggard counties can approach their balanced growth paths relatively quickly. Human capital levels represent part of those conditions, and studying their contributions provides policy-makers insights into what constitutes improvements in those conditions.

5. Analysis of Human Capital Effects

We focus on four different variables measuring educational attainment within U.S. counties: the percent of the population with (a) 9-11 years of education and no more, (b) a high school diploma and no more, (c) some college education but less than a bachelor degree, and (d) a bachelor degree and/or higher degrees.[20]

Despite the fact that these are *direct* measures of educational attainment that are compiled by a single institution for at the same time for all U.S. states, Wößmann (2003) notes that problems still remain to be addressed in using such variables as proxies for human capital stocks. First, they represent the percent of the population with certain educational attainment rather than the percent of the labor force (p. 248). However, four conditioning variables (see **Table 1**) are age demographics that should control for

[19] It is worth noting that a 10 percent convergence rate implies that the distance from the balanced growth path is halved within 10 years.

[20] For the remaining four variables (for public and private elementary schools and nurseries) we get mixed results in the full sample in terms of statistical significance. Although two of them have statistically significant estimated coefficient values, none of the coefficients represent economically significant effects. The point estimates of all coefficients for the full sample are zero at four-digit precision.

variation in a population's educational attainment not related to the education of the labor force.

Second, mismeasurement can occur, in any educational attainment category, it is not known whether counted individuals started into the next level (e.g. someone with 1 year of college education would be placed in the (b) category above) (p. 248). While this is also true, our four-category stratification should minimize the importance of such mismeasurement.

Third, the returns to education may not be constant, meaning that entering unweighted educational variables linearly in a regression may a misspecification (p.249).[21] This means that caution must be exercised in the interpretation of the coefficient estimates. A reasonable interpretation is that they correspond to the *average* (rather than marginal) effect on growth of the percent of a county's population having achieved a certain level of education. Given this interpretation, the four-level stratification is again for inferring marginal effects. For example, a 0 coefficient estimate on variable (c) and a positive estimate on variable (b) may indicate that the average effect on growth of more people attaining 2 years of college is zero and that marginal effect is negative.[22]

Fourth, the returns to even similar years of schooling may be different in different economies because of quality differences (p.249). We address this issue somewhat below in discussing the average SAT scores of different states, and in discussing coefficient estimates on the percent of populations employed in providing educational services (which may proxy for quality). Also, while the county-level data does not provide us with direct measures of quality, our results imply the importance of quality. We report both quantitative and qualitative differences in educational attainment effects across U.S. states. Given the reliability of the data and the homogeneity (in institution and time) of its collection, a strong case can be made that detected heterogeneity is resulting from quality differences in educational systems.

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[21] Wößmann (2003) suggests constructing a human capital measure using educational attainment measures along with micro labor study estimates of rates of return to different years of schooling as weights. While preferable, such micro labor studies do not exist for the U.S. county by county.

[22] This simple example would be assuming certainty in the coefficient estimates, so the interpretation of actual results is, of course, not nearly as clean cut.

Table 4 reports the 3SLS coefficient estimates for these four educational attainment variables for the full sample and within-state samples. We first consider the percent of the population with at least 9 years of education, but less than a high school (or its equivalent) degree. For the full sample the coefficient is –0.0221 and is significant at the 1 percent level. This seems sensible and suggests that the greater the percentage of an economy's population without the remedial mathematics, writing and communications skills – as well as the minimum personal discipline and social behavior – necessary to obtain a high school diploma, the lower the economy's balanced growth path.

The significant negative estimated effect again shows the danger of interpreting years of schooling as a monotonic, linear measure of homogenous human capital accumulation. In fact, some years of schooling may not even be indicative of human capital at all. Mankiw (1997, p.106) notes that "[S]econdary-school [high-school] enrollment represents a decision between work and education. By contrast, a 7-year-old not at school might be home with a parent. This time may at least represent a form of home schooling [and in general] primary-school enrollment might contain little information about human capital accumulation." Mankiw is referring to enrollment, rather than attainment measures, but his point remains: opportunity cost is the natural measure of human capital and the opportunity cost to the first 9-11 years of education is often nil.

Passing that threshold, the coefficient for the population achieving, but not surpassing, a high school diploma has a point estimate of 0.0097, also significant at the 1 percent level, for the full sample. Again, one can easily make the argument that the completion of the high-school degree (the last two years of which are non-compulsory in most cases and apply to individuals old enough to join the labor force) has a positive opportunity cost and therefore represents a positive investment in human capital.

More surprisingly, for the full sample the coefficient point estimate is –0.0025 for the percent of the population with some college education but not enough for a bachelor degree. However, this estimate is not statistically different from zero. Compare this to the coefficient on the percent of the population with a bachelor degree or more: 0.0732 and significant at the 1 percent level. This point estimate, as well, dwarfs that of the high-school variable coefficient. A possible interpretation of this result again concerns opportunity cost. College education ostensibly involves a benefit in the form of increased skills/productivity for the individual, but it also involves a cost in the form of foregone wages. The results may imply that college education of at least 4 years represents (on average) a positive net return to individuals, while the net return on a 2-year degree is questionable.

Of course, the immediate response to the above is to ask: But then why do individuals go to college and not pursue bachelor degrees to begin with? Straightforward explanations include that the net return is positive but too small to be statistically identified and that individuals consistently overestimate the return. The first of these is a dead-end for this analysis, and the second is not appealing if we wish to maintain an assumption of some basic rationality on the part of agents. This does not rule out either of these explanations, but another explanation exists that is plausible and some evidence exists for: agents do not bear the full opportunity cost. Kane and Rouse (1995) and Surette (1997) both report that the estimated return to 2-year degrees is positive and equals about 4-6 percent and 7-10 percent respectively. However, these studies measure private return and not social return. They examine individuals' costs (tuition paid, wages forgone, experience forgone, etc.) and benefits (wage premiums). On the other hand, Kane and Rouse (p.600n) note that "Twenty percent of Federal Pell Grants, 10 percent of Guaranteed Student Loans, and over 20 percent of state expenditures for postsecondary education, go to community colleges." Our findings may be detecting that when the full opportunity cost is accounted for, the social return is nil.

Besides heterogeneity in the estimated effects of different types/levels of educational attainment, we also find significant heterogeneity – sometimes qualitative, sometimes only quantitative, and in some cases both qualitative and quantitative – within attainment categories across different U.S. states. In the case of the less than high school degree attainment, there are only 6 statistically significant (10 percent level or better) within-state coefficient estimates. These are evenly split as far as sign is concerned. They range from –0.0904 (South Dakota, 1 percent level) to 0.1171 (Colorado, 5 percent level). (These point estimates and their 95 percent confidence intervals are presented in **Figure 2**.) This heterogeneity is interesting. If the above interpretation we offered for the negative full sample coefficient (significant at the 1 percent level) is convincing, why would there be some individual state economies where having a larger percent of the population not obtaining a high school diploma be conducive to economic growth?

One potential explanation concerns compulsory education laws. The story could run two ways. Stronger (in terms of years required) laws might be associated with positive coefficients. The variable may include many people who would have had no high school education at all, given their druthers, and benefited by being forced to pick up some remedial math and verbal skills. On the other hand, stronger laws might be negatively associated with low coefficient estimates because of the opportunity cost forced on rather-be-truant individuals and the direct cost being incurred by the school system to deal with them. If these individuals are students being pushed through the school system (which is costly) while never actually receiving/accepting the benefits of education, they also forego the productive opportunities available in the meanwhile. However, neither of these stories is suggested by the data. Each of the 6 states included in **Figure 2** has roughly similar age spans of compulsion: 7 to 16 (Alabama, Colorado, Illinois and North Carolina), 6 to 16 (South Dakota), or 6 to 18 (Texas) years old.[23] There is no apparent correlation between these small differences and the coefficient estimates.

Qualitative heterogeneity also exists across the significant high school diploma variable estimates (**Figure 3**). Of the 8 coefficient estimates significant at the 10 percent level or better, 2 of them are negative (Mississippi and Ohio) and 1 has a 95 percent confidence interval entirely in the negative range (Mississippi). However, among the 6 coefficients with positive point estimates there is no statistically significant difference between them.

One straightforward potential explanation for the existing heterogeneity is simply that school systems are simply better in some states than others.[24] This hypothesis can be informally tested by comparing average scholastic aptitude test (SAT) scores from the states with negative coefficient estimates to those with positive coefficient estimates. **Figures 4a**, **4b** and **4c** plot total, math and verbal average SAT scores for the 1998-1999

[23] These laws are from a report by the U.S. Department of Education (2001a), except for the Colorado law, which comes from the Colorado Department of Education.
[24] Over the 1999-2000 school year private schools accounted for only 12 percent of elementary and secondary students in the U.S. (U.S. Department of Education (1999-2000)).

school year, respectively, for the 8 states included in **Figure 3**. Indeed, Ohio has both the lowest math and verbal average score (and, of course, total as well). However, Mississippi – with the only coefficient negative and significant at the 5 percent level – has SAT average scores neither exceptionally high nor low relative to the other seven states. So if the heterogeneity arises from school quality, this relationship does not clearly manifest itself in test scores.[25] This is surprising given the evidence, overviewed in Hanushek (2002), that standardized test results correlate with higher earnings and economic growth.

No statistically significant heterogeneity can be detected among coefficients for the some college variable. This is shown in **Figure 5**. In the first place, for only 5 states is the coefficient statistically different from zero at the 10 percent level. Furthermore, the confidence intervals are all overlapping. Still, it should be noted that every point estimate shown is positive.

We find the same for the bachelor degree or more variable (**Figure 6**). Although we cannot distinguish the coefficient estimates for the 10 state coefficients significant at the 10 percent level, a full 9 of these 10 are significantly positive at the 5 percent level. In the case of the college education variable coefficients, in general, we cannot detect any statistically significant heterogeneity across U.S. states. The only statement we can make concerns homogeneity: the effect on a growth path of the percent of the population attaining four years of college education is positive uniformly across U.S. states.

6. Education Fosters Growth. Providing It Does *Not*?

Our data set also includes county-level measures of the percent of a county's population employed in education services. While the results of educational attainment above are different across individual states and types/levels of attainment, the overall picture still would suggest human capital has a positive effect on growth. However, the percent of the population providing education services is *negatively* correlated with growth in the full sample (point estimate -0.0334 and significant at the 1 percent level). Furthermore, no statistically significant qualitative heterogeneity is detected across states.

[25] The SAT score test of the hypothesis, however, relies on an assumption that the predominant portion of the population with high school degrees obtained their high school degrees in the considered state. For individual states, this seems *a priori* plausible but not certain.

For the 6 states with coefficient estimates significant at the 10 percent level or better, each and every point estimate is negative.

Why does education appear to foster growth while the ostensible provision of it does not? Perhaps a given county itself does not internalize the benefits of education provided in that county. For example, we find a positive partial correlation between four-year college or higher educational attainment and economic growth, but the correlation is silent as to where the education was attained. Individuals may attend college or university where human capital is relatively easy to accumulate, and then move to other counties as they join the workforce. In Higgins et al (2003) we find that the negative correlation between education service provision and growth is particularly strong in metro counties. This is consistent with an externality argument insofar as a large proportion of colleges and universities are in metro areas, and many students leave the metro areas upon graduation. Indeed the estimated partial correlation is insignificant when the sample only includes non-metro counties.[26]

Another explanation is bureaucratic overexpansion of the public school systems. This hypothesis is frequently entertained in the popular media and is explored by Marlow (2001) in the California primary and secondary school districts. However, Marlow finds that an increase in the number of teachers has no statistically significant effect on SAT scores or dropout rates while an increase in the size of administrative staff *increases* the SAT scores and *decreases* the dropout rates.

Yet another explanation is that, since we account for the output of the human capital production processes (with the educational attainment variables), and since it is the output that matters for growth, it is reasonable that the inputs to the human capital production processes (labor in educational services) have a negative contribution *independently*. But would not one still expect, especially given the detected heterogeneity in human capital effects, that the percentage of the population providing educational services proxies for the *quality* of the human capital stock (*quantity*)? One answer to this query is that more providers of educational services do not lead to a greater

[26] Considerable evidence exists suggesting that the external benefits of education are negligible (Heckman and Klenow (1997), Acemoglu and Angrist (2001), and Ciccone and Peri (2000)). However, in the case of individual counties (especially when considering a single U.S. state in and of itself) labor mobility may be so great that an external benefits story becomes relevant.

quality provision of human capital. This view has found considerable support in the literature. The variation in conventional measures of resources devoted to education (e.g. per student spending, teacher to student ratios and teacher experience/education) generally does a poor job of accounting for variation in student achievement (Hanushek and Rivkin (2004), Hanushek (1996), Hanushek et al (1996) and Wößmann (2001)). We view the negative correlation of educational services provision and growth, along with the heterogeneity in estimated attainment effects, as supporting this view and pointing to the importance of policymakers identifying and pursuing determinants of educational quality (Hanushek (2004)).

7. Conclusions

We use county-level data to study the role of different types of human capital accumulation in U.S. growth determination. The data includes over 3,000 cross-sectional observations and 39 demographic control variables. The large number of observations provides enough degrees of freedom to obtain estimates for the U.S. as a whole and for 32 states in and of themselves. The data contains measures of educational attainment for four distinct categories: (a) 9 to 11 years, (b) high school diploma, (c) some college and (d) bachelor degree or more. These variables represent human capital stocks for each and every county.

Using a consistent three stage least squares estimation procedure, we find that (i) the percentage of a county's population with less than a high school education is negatively correlated with economic growth, (ii) the percentage obtaining a high school diploma but no more is positively correlated with economic growth, (iii) the percentage obtaining only some college education has no clear relationship with economic growth, but (iv) the percentage that obtains a bachelor degree or more is positively correlated with economic growth.. Further, we find that (v) there is significant qualitative heterogeneity in estimated coefficients across states for the 9 to 11 years and high school diploma categories but (vi) no qualitative heterogeneity for the college level categories.

The most consistent and significant conclusion across samples is that the percent of a county's population obtaining a bachelor degree or higher level of college education has a positive relationship with economic growth. Oddly enough, despite findings (ii),

(iv) and (vi) above, we find that the percentage of a county's population employed in educational services is negatively correlated with economic growth.

For econometric estimation of growth equations we employ the neoclassical specification, which enables us to use the sign of the estimated coefficient on the initial income level as a discriminating test between the validity of the neoclassical growth model against the alternative of the endogenous growth model. We find that this coefficient is negative and significant for the full U.S. sample. In addition, we find that it is negative also for every individual state sample where the value is statistically significant. Thus, the data support the neoclassical growth model, which implies that high levels of human capital accumulation are conducive to the adoption and application of available technologies, and that this represents a positive contribution to economic growth via a higher balanced growth path.

Data Appendix

The variables for this study were drawn from several different sources; however, the majority of the data came from the Bureau of Economic Analysis Regional Economic Information System (BEA REIS) data sets. We thank Jordan Rappaport for allowing us access to his very extensive data set. His data sets, more fully described in Rappoport (1999), are largely based on the 1970, 1980 and 1990 decennial census summary tape files, the 1972, 1977, 1982 and 1987 Census of Governments, various years of the Census Bureau's City and County Book along with data from BEA REIS.

Our data set includes variables for 3,062 U.S. counties. Several U.S. counties were excluded from the data set for various reasons. Primarily, counties were excluded for lack of data. Examples of counties that fell into this category include counties in northern Alaska and some counties in Hawaii. Some data for these counties was simply not recorded as far back as 1970. Furthermore, in Virginia, some cities are themselves independent counties. If the data for these independent cities was available we let them stand as their own county. However, if the data was not available we tried to incorporate the independent city into the surrounding county. If that was not feasible, it was then dropped from the data set.

In the construction of the data set, since we were interested in the time period 1970 – 1998, it was necessary to use an interpolation procedure. We chose the procedure outlined in Dezhbaksh & Levy (1994). In order to employ the econometric techniques outlined by Evans (1997), the data set needed to have available data values for 1969 and 1997. Therefore, the interpolative procedure was used to generate the needed variables based on the available observations in the data set. Data relating to income and population were not generated by this method as they were available from BEA REIS on a yearly basis. Additionally, Census data variables which were available in 1970, 1980 and 1990 were interpolated for their respective 1969, 1997 and 1998 values. The interpolated values for example, for 1969, were generated in the following manner:

We start with

(i) $y_t = a + b \times time$

Our time period is from 1970-1990, so we have 20 sub-periods.

(ii) $y_{70} = a + b \times 1970$

(iii) $y_{90} = a + b \times 1990$

Rearrange (ii) to solve for a and then substitute into (iii).

(iv) $a = y_{70} + b \times 1970$

(v) $y_{90} = y_{70} + b(1990\text{-}1970)$

Rearranging (v) yields:

(vi) $b = (y_{90} - y_{70})/20$

Substituting (vi) into (iv) and then into (i) yields:

(vii) $y_t = y_{70} - [(y_{90} - y_{70})/20] \times 1970 + [(y_{90} - y_{70})/20] \times t$

(viii) $y_t = y_{70} + [(y_{90} - y_{70})/20] \times (t - 1970)$

Now, solving for our period of concern, 1969, yields:

(ix) $y_{1969} = y_{1970} + [(y_{1990} - y_{1970})/20] \times (1969 - 1970)$

(x) $y_{1969} = (20/20)y_{70} - (1/20)y_{90} + (1/20)y_{70}$

(xi) $y_{1969} = (21/20)y_{70} - (1/20)y_{90}$

Per capita values were converted to real 1992 constant dollars utilizing the equation:

(i) real per capita income = [nominal per capita income/GDP deflator] x 100

Natural logs of these values were used throughout the project.

Because of the critical importance of the income variable for the study of growth and convergence, we want to address its measurement in some detail. Two options were available to us for the construction of the county-level per capita income variable: (1) Census Bureau database, and (2) BEA-REIS database.

Income information collected by the Census Bureau for states and counties is prepared decennially from the "long-form" sample conducted as part of the overall population census (BEA, 1994). This money income information is based on the self-reported values by Census Survey respondents. An advantage of the Census Bureau's data is that they are reported and recorded by place of residence. These data, however, are available only for the "benchmark" years, i.e., the years in which the decennial Census survey is conducted.

The second source for this data, and the one chosen for this project, is personal income as measured by the Bureau of Economic Analysis (BEA).[27] The definitions that

[27] The data and their measurement methods are described in detail in "Local Area Personal Income, 1969–1992" published by the BEA under the Regional Accounts Data, February 2, 2001.

are used for the components of personal income for the county estimates are essentially the same as those used for the national estimates. For example, the BEA defines "personal income" as the sum of wage and salary disbursements, other labor income, proprietors' income (with inventory valuation and capital consumption adjustments), rental income (with capital consumption adjustment), personal dividend income and personal interest income. (BEA, 1994) "Wage and salary disbursements' are measurements of pre-tax income paid to employees. "Other labor income" consists of payments by employers to employee benefit plans. "Proprietors' income" is divided into two separate components—farm and non-farm. Per capita income is defined as the ratio of this personal income measure to the population of an area.

The BEA's estimates of personal income reflect the revised national estimates of personal income that resulted from the 1991 comprehensive revision and the 1992 and 1993 annual revisions of the national income and product accounts. The revised national estimates were incorporated into the local area estimates of personal income as part of a comprehensive revision in May 1993. In addition, the estimates incorporate source data that were not available in time to be used in the comprehensive revisions.[28]

The BEA compiles data from several different sources in order to derive this personal income measure. Some of the data used to prepare the components of personal income are reported and recorded by place of work rather than place of residence. Therefore, the initial estimates of these components are on a place-of-work basis. Consequently, these initial place-of-work estimates are adjusted so that they will be on a place-of-residence basis and so that the income of the recipients whose place of residence differs from their place of work will be correctly assigned to their county of residence.

As a result, a place of residence adjustment is made to the data. This adjustment is made for inter-county commuters and border workers utilizing journey-to-work (JTW) data collected by Census. For the county estimates, the income of individuals who commute between counties is important in every multi-county metropolitan area and in many non-metropolitan areas. The residence adjustment estimate for a county is calculated as the total inflows of the income subject to adjustment to county i from

[28] For details of these revisions, see "Local Area Personal Income: Estimates for 1990–92 and Revisions to the Estimates for 1981–91," *Survey of Current Business* 74 (April 1994), 127–129.

county *j* minus the total outflows of the income subject to adjustment from county *i* to county *j*. The estimates of the inflow and outflow data are prepared at the Standard Industrial Classification (SIC) level and are calculated from the JTW data on the number of wage and salary workers and on their average wages by county of work for each county of residence from the Population Census.

Econometric Appendix

The method of ordinary least squares (OLS) could be used to infer the values of β and γ in equation (2.3). However, Evans (1997b) states that the OLS estimates obtained from (2.3) are unlikely to be consistent.[29] In order to demonstrate this inconsistency, Evans first specifies a general autoregressive moving average (ARMA) data-generating process for y_{nt}:

(1A) $$y_{nt} - a_t = \delta_n + \lambda_n(y_{n,t-1} - a_{t-1}) + \sum_{i=1}^{q} \theta_{ni}\varepsilon_{n,t-i}$$

with

(2A) $$\delta_n = \kappa + \xi_n' x_n + \omega_n$$

where ε_{nt} is a zero-mean, covariance stationary error process independently distributed over time and across economies. The error term, ε_{nt}, is uncorrelated with x_n, λ_n is an autoregressive parameter which lies on $(0,1]$, and $\theta_{n0}...\theta_{nq}$ satisfy the restriction $\theta_{n0} = 1$. As such, $y_{nt} - a_t$ will also have an autoregressive representation and will be covariance stationary if λ_n<1 or difference stationary if $\lambda_n = 1$. The common time-specific effect experienced by every economy is represented by the term a_t. Evans assumes that Δa_t is covariance stationary and independent of ε_{nt}.

The common trend a_t for all the y variables will be the sole catalyst of economic growth in all economies if λ_n<1. In this case, growth is exogenous and economies would follow a balanced-growth path. If λ_n=1, on the other hand, then economy n will grow endogenously since y_{nt} diverges from a_t and the y variables of all remaining economies. The parameter δ_n controls for the relative height of economy n's balanced growth path if all the λ s are less than one. If $\lambda_n = 1$, then δ_n controls for economy n's relative growth rate. The error term ω_n measures the portion of δ_n that is not explained

[29] This section borrows heavily from Evans (1997b), which can be consulted for further details.

by x_n. This error term is assumed to be uncorrelated with x_n. The inequality λ_n<1 will hold for an economy described by the neoclassical growth model.

Solving equation (1A) backward from year *T* to year 0, substituting from equation (2A), and rearranging produces

(3A)
$$g_n = \alpha_n + \beta_n y_{n0} + \gamma'_n x_n - \frac{\beta_n \omega_n}{1-\gamma_n} + \frac{1}{T}\sum_{i=0}^{T-1} \lambda_n^i \left(\sum_{j=0}^{\min[i,q]} \lambda_n^{-j} \theta_{nj} \right) \varepsilon_{n,T-i}$$
$$+ \left(\frac{\lambda_n^T}{T} \right) \sum_{i=0}^{q-1} \lambda_n^i \left(\sum_{j=i+1}^{q} \lambda_n^{-j} \theta_{nj} \right) \varepsilon_{n,-i}$$

where $\beta_n = \dfrac{\lambda_n^T - 1}{T}$, $\gamma_n = \dfrac{-\beta_n \xi_n}{1-\lambda_n}$, and $\alpha_n = \dfrac{a_T - a_0}{T - \beta_n \left(\dfrac{a_0 + \kappa}{1-\lambda_n} \right)}$. If $\beta_n < 0$, then economy n grows exogenously $(\lambda_n < 1)$. On the other hand, if $\beta_n = 0$, then economy n grows endogenously $(\lambda_n = 1)$.

Now consider a special case in which every intercept δ_n is completely explained by the county characteristics included in x_n $(\omega_n = 0, \forall n)$ and every series $y_{nt} - a_t$ is a first-order auto-regression $(q = 0)$. Under these restrictions equation (3A) reduces to:

(4A)
$$g_n = \alpha_n + \beta_n y_{n0} + \gamma'_n x_n + \frac{1}{T}\sum_{i=0}^{T-1} \lambda_n^i \varepsilon_{n,T-i}$$

The estimator for $\hat{\beta}$ can then be obtained in two steps. First, regress y_{n0} on an intercept and x_n to obtain the residual r_n and then regress g_n on r_n. (This is simply the OLS estimator of β.) Each term in $\frac{1}{T}\sum_{i=0}^{T-1} \lambda_n^i \varepsilon_{n,T-i}$ is uncorrelated with the intercept, y_n, x_n and the residual r_n. As a result, one has

$$\text{(5A)} \quad \underset{N\to\infty}{p\lim}\hat{\beta} = \frac{\underset{N\to\infty}{p\lim}\frac{1}{N}\sum_{n=1}^{N}\alpha_n r_n + \underset{N\to\infty}{p\lim}\frac{1}{N}\sum_{n=1}^{N}\beta_n r_n y_n + \underset{N\to\infty}{p\lim}\frac{1}{N}\sum_{n=1}^{N}\gamma_n' r_n x_n}{\underset{N\to\infty}{p\lim}\frac{1}{N}\sum_{n=1}^{N}r_n^2}$$

Making further assumptions that α_n is uncorrelated with r_n, β_n is uncorrelated with $r_n y_n$, and γ_n is uncorrelated with $r_n x_n$, equation (5A) leads to

$$\text{(6A)} \quad \underset{N\to\infty}{p\lim}\hat{\beta} = \frac{\underset{N\to\infty}{p\lim}\frac{1}{N}\sum_{n=1}^{N}\beta_n r_n^2}{\underset{N\to\infty}{p\lim}\frac{1}{N}\sum_{n=1}^{N}r_n^2}$$

The probability limit of the OLS estimator is then a weighted average of the economy specific β_ns. It is a consistent estimator of that weighted average.[30]

But what if the assumption that every intercept δ_n is completely explained by x_n and also the assumption that every series $y_{nt} - a_t$ is a first-order auto-regression, are relaxed? Relaxing these assumptions, and imposing the additional restriction that the λs and ξs and, as a result, the βs and γs are identical across all economies (for the simplicity of the exposition), (3A) can be re-written as

$$\text{(7A)} \quad \begin{aligned} g_n &= \alpha + \beta y_{n0} + \gamma' x_n - \frac{\beta\omega_n}{1-\gamma} + \frac{1}{T}\sum_{i=0}^{T-1}\lambda^i\left(\sum_{j=0}^{\min[i,q]}\lambda^{-j}\theta_{nj}\right)\varepsilon_{n,T-i} \\ &+ \left(\frac{\lambda^T}{T}\right)\sum_{i=0}^{q-1}\lambda^i\left(\sum_{j=i+1}^{q}\lambda^{-j}\theta_{nj}\right)\varepsilon_{n,-i} \end{aligned}$$

[30] Strictly speaking, even for this restrictive case, an OLS estimate less than unity does not mean that all the economies in the sample conform to the neoclassical growth model. Rather, it would mean that enough economies conform, so that the weighted average is less than unity. It would mean, therefore, that exogenous growth is the predominant case across the sample.

where $\beta = \dfrac{\lambda^T - 1}{T}$, $\gamma = \dfrac{-\beta\xi}{1-\lambda}$, and $\alpha = \dfrac{a_T - a_0}{T - \beta\left(\dfrac{a_0 + \kappa}{1-\lambda}\right)}$. Applying the same steps to equation (6A) yields

$$\text{(8A)} \qquad \underset{N\to\infty}{p\lim}\,\hat{\beta} = \beta + \frac{(\Phi + \Psi)}{\underset{N\to\infty}{p\lim}\dfrac{1}{N}\sum_{n=1}^{N} r_n^2}$$

where $\Phi = \dfrac{\lambda^T}{T}\underset{N\to\infty}{p\lim}\dfrac{I}{N}\sum_{n=1}^{N}\left[\sum_{i=0}^{q-1}\lambda^i\left(\sum\lambda^{-j}\theta_{n,j+i+1}\right)r_n\varepsilon_{n,-i}\right]$ and $\Psi = -\dfrac{\beta}{1-\lambda}\underset{N\to\infty}{p\lim}\dfrac{1}{N}\sum r_n\omega_n$.

As a result, equation (8A) implies that $\underset{N\to\infty}{p\lim}\,\hat{\beta}$ differs from β if *either* $q > 0$ ($y_{nt} - a_t$ is not a first-order AR process) or the cross-sectional variance of ω_n is positive (not all cross-sectional heterogeneity is accounted for). In other words, the OLS estimator is inconsistent unless (a) the log of income per capita has an identical first-order AR representation across economies, and (b) all cross-section heterogeneity is controlled for.

Evans shows that the resulting bias from $q > 0$ is likely to be negligible in practice but the bias resulting from a positive cross-sectional variance for ω_n can be substantial. This is essentially an omitted variable bias. Evans demonstrates that

$$\text{(9A)} \qquad \underset{N\to\infty}{p\lim}\hat{\beta} = \left[\frac{\text{var}(y\,|\,x,\omega)}{\text{var}(y\,|\,x)}\right]\beta$$

and

$$\text{(10A)} \qquad \underset{N\to\infty}{p\lim}\hat{\gamma} = \left[\frac{\text{var}(y\,|\,x,\omega)}{\text{var}(y\,|\,x)}\right]\gamma.$$

The bracketed portions in equations (9A) and (10A) are the ratio of the cross-sectional variance of y_{n0} conditional on both x_n and ω_n to the cross-sectional variance of y_{n0} on

x_n. As such, $\hat{\beta}$ and $\hat{\gamma}$ will be biased towards zero unless the xs are able to control for a large portion of the cross-economy variation in the ys.

The intuition here is that if a large portion of the growth of per capita income is explained by variables left out of the OLS regression, then the estimate of the convergence effect will be biased. In general, omitted variable bias can be either positive or negative. However, in this case, theoretically, the bias is negative. Evans (1997b, Tables on p. 11 and p. 15) estimates β for Mankiw, et al.'s (1992) international data using both the OLS, which yields inconsistent estimates, and the 3SLS approach (as outlined in section 2), which yields consistent estimates of both β and γ. He finds that the 3SLS estimate implies a conditional convergence rate between 4 to 5 times as large as the OLS estimate. The bias produced by the OLS in this case, therefore, is substantial.

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Table 1: Variable Definitions and their Source

Variable	Definition	Period	Source
Income	Per Capita Personal Income (excluding transfer payments)	1969–1998	BEA[31]
Land Area	Land Area in km^2	1970-1990	Census[32]
Water Area	Water Area in km^2	1970-1990	Census
Age: 5-13 years	Percent of 5–13 year olds in the population	1970-1990	Census
Age: 14-17 years	Percent of 14–17 year olds in the population	1970-1990	Census
Age: 18-64 years	Percent of 18–64 year olds in the population	1970-1990	Census
Age: 65+	Percent of 65+ olds	1970-1990	Census
Blacks	Percent of Blacks	1970-1990	Census
Hispanic	Percent of Hispanics	1970-1990	Census
Education: 9-11 years	Percent of population with 11 years education or less	1970-1990	Census
Education: H.S. diploma	Percent of population with high school diploma	1970-1990	Census
Education: Some college	Percent of population with some college education	1970-1990	Census
Education: Bachelor +	Percent of population with bachelor degree or above	1970-1990	Census
Education: Public elementary	Number of students enrolled in public elementary schools	1970-1990	Census
Education: Public nursery	Number of students enrolled in public nurseries	1970-1990	Census
Education: Private elementary	Number of students enrolled in private elementary schools	1970-1990	Census
Education: Private nursery	Number of students enrolled in private nurseries	1970-1990	Census
Housing	Median house value	1970-1990	Census
Federal government employment	Percent of population employed by the federal government in the county	1969-1998	BEA
State government employment	Percent of population employed by the state government in the county	1969-1998	BEA
Local government employment	Percent of population employed by the local government in the county	1969-1998	BEA
Self-employment	Percent of population self-employed	1970-1990	Census
Agriculture	Percent of population employed in agriculture	1970-1990	Census
Communications	Percent of population employed in communications	1970-1990	Census
Construction	Percent of population employed in construction	1970-1990	Census
Finance, insurance & real estate	Percent of population employed in finance, insurance, and real estate	1970-1990	Census
Manufacturing: durables	Percent of population employed in Manufacturing of durables	1970-1990	Census
Manufacturing: non-durables	Percent of population employed in manufacturing of non-durables	1970-1990	Census
Mining	Percent of population employed in mining	1970-1990	Census
Retail	Percent of population employed in retail trade	1970-1990	Census
Business & repair services	Percent of population employed in business and repair services	1970-1990	Census
Educational services	Percent of population employed in education services	1970-1990	Census
Professional related services	Percent of population employed in professional services	1970-1990	Census
Health services	Percent of population employed in health services	1970-1990	Census

[31] All BEA variables are available for each year from 1969-1998.
[32] Note, all Census variables are gathered from the 1970, 1980 & 1990 Census tapes. Values for 1969 were obtained via the interpolation method as discussed in the data section.

Table 1: Variable Definitions and their Sources (Cont.)

Personal services	Percent of population employed in personal services	1970-1990	Census
Entertainment & recreational services	Percent of population employed in entertainment and recreational services	1970-1990	Census
Transportation	Percent of population employed in transportation	1970-1990	Census
Wholesale trade	Percent of population employed in wholesale trade	1970-1990	Census
Poverty	Percent of the population living at or below the poverty level	1970-1990	Census
Metro area, 1970	Dummy Variable: 1 if the county was in a metro area in 1970, and 0 otherwise	1970	Census

Table 2: Coefficients on Initial Income (β) from Growth Regressions

State	Number of Counties	OLS	3SLS
United States[33]	3,058	-0.0174 (22.15)*	-0.0345 (24.19)*
Alabama	67	-0.0251 (2.38)**	-0.0334 (20.49)*
Arkansas	74	-0.0267 (4.48)*	-0.0384 (22.08)*
California	58	-0.0261 (2.50)**	-0.0235 (4.87)*
Colorado	63	-0.0134 (2.53)**	-0.0318 (13.41)*
Florida	67	-0.0190 (2.06)**	-0.0319 (14.98)*
Georgia	159	-0.0171 (4.33)*	-0.0367 (36.46)*
Idaho	44	-0.0403 (2.23)**	-0.0406 (10.03)*
Illinois	102	-0.0255 (5.46)*	-0.0281 (9.07)*
Indiana	92	-0.0061 (1.02)	-0.0299 (9.25)*
Iowa	99	-0.0288 (5.65)*	-0.0289 (4.75)*
Kansas	106	-0.0286 (9.76)*	-0.0301 (12.18)*
Kentucky	120	-0.0253 (6.11)*	-0.0354 (19.74)*
Louisiana	64	-0.0222 (3.83)*	-0.0413 (13.83)*
Michigan	83	-0.0104 (1.36)	-0.0387 (16.52)*
Minnesota	87	-0.0156 (2.85)*	-0.0260 (9.34)*
Mississippi	82	-0.0182 (2.05)**	-0.0448 (13.43)*
Missouri	115	-0.0171 (3.78)*	-0.0455 (10.74)*
Montana	56	-0.0229 (3.31)*	-0.0328 (9.14)*
New York	62	0.0129 (1.24)	-0.0264 (7.78)*
North Carolina	100	-0.0171 (3.32)*	-0.0467 (7.11)*
North Dakota	53	-0.0279 (3.29)*	-0.0594 (4.79)*
Ohio	88	-0.0136 (1.87)***	-0.0274 (7.68)*
Oklahoma	77	-0.0248 (3.95)*	-0.0387 (22.11)*
Pennsylvania	67	-0.0176 (2.53)**	-0.0312 (9.01)*
South Carolina	46	-0.0118 (0.62)	-0.0336 (5.97)*
South Dakota	66	-0.0193 (2.39)**	-0.0265 (4.77)*
Tennessee	97	-0.0199 (3.55)*	-0.0392 (15.21)*
Texas	254	-0.0211 (8.10)*	-0.0356 (15.18)*
Virginia	84	-0.0045 (0.69)	-0.0348 (15.81)*
Washington	39	-0.0349 (1.09)	-0.0327 (9.29)*
West Virginia	55	0.0043 (0.43)	-0.0336 (15.49)*
Wisconsin	70	-0.0191 (3.08)*	-0.0240 (6.83)*

t-statistics are reported in parentheses

*   significant at 1% level
** significant at 5% level
*** significant at 10% level

[33] See Higgins, Levy and Young (2002).

Table 3 Asymptotic Convergence Rates – Point Estimates & 95% Confidence Intervals

State	Number of Counties	OLS Estimates & 95% C.I. [34]	3SLS Estimates & C.I.
United States[35]	3,058	0.0237 (0.0208, 0.0267)	0.0682 (0.0544, 0.0911)
Alabama	67	0.0424 (0.0036, 0.1080)	0.0931 (0.0492, 0.1466)
Arkansas	74	0.0479 (0.0166, 0.1098)	0.0738 (0.0570, 0.1363)
California	58	0.0457 (0.0046, 0.1249)	0.0375 (0.0178, 0.0868)
Colorado	63	0.0166 (0.0031, 0.0384)	0.0759 (0.0426, 0.1009)
Florida	67	0.0268 (0.0010, 0.1109)	0.0767 (0.0480, 0.1174)
Georgia	159	0.0230 (0.0109, 0.0413)	0.1043 (0.0699, 0.1142)
Idaho	44	0.0892 (0.0021, 0.1566)	0.0913 (0.0471, 0.1145)
Illinois	102	0.0434 (0.0213, 0.1168)	0.0537 (0.0337, 0.1062)
Indiana	92	0.0067 (-0.0054, 0.0245)	0.0622 (0.0354, 0.1221)
Iowa	99	0.0570 (0.0224, 0.1176)	0.0574 (0.0175, 0.0954)
Kansas	106	0.0560 (0.0360, 0.1086)	0.0639 (0.0434, 0.1228)
Kentucky	120	0.0431 (0.0233, 0.0922)	0.1054 (0.0561, 0.1160)
Louisiana	64	0.0341 (0.0128, 0.0955)	0.1555 (0.0989, 0.1940)
Michigan	83	0.0121 (-0.0043, 0.0427)	0.1152 (0.0536, 0.1659)
Minnesota	87	0.0202 (0.0053, 0.0459)	0.0454 (0.0305, 0.0719)
Mississippi	82	0.0249 (0.0009, 0.1509)	0.1405 (0.0455, 0.1923)
Missouri	115	0.0230 (0.0094, 0.0452)	0.0817 (0.0387, 0.1132)
Montana	56	0.0359 (0.0099, 0.0996)	0.0865 (0.0367, 0.1566)
New York	62	0.0111 (-0.0238, 0.0284)	0.0465 (0.0285, 0.0853)
North Carolina	100	0.0228 (0.0078, 0.0491)	0.1302 (0.0966, 0.1574)
North Dakota	53	0.0528 (0.0103, 0.1247)	0.0761 (0.0353, 0.1102)
Ohio	88	0.0170 (-0.0005, 0.0520)	0.0503 (0.0299, 0.1059)
Oklahoma	77	0.0415 (0.0139, 0.1136)	0.1152 (0.0574, 0.1437)
Pennsylvania	67	0.0240 (0.0043, 0.0707)	0.0705 (0.0291, 0.1099)
South Carolina	46	0.0142 (-0.0147, 0.1259)	0.0960 (0.0243, 0.1315)
South Dakota	66	0.0274 (0.0036, 0.1391)	0.0406 (0.0184, 0.1144)
Tennessee	97	0.0287 (0.0102, 0.0689)	0.0681 (0.0488, 0.1168)
Texas	254	0.0312 (0.0208, 0.0458)	0.1170 (0.0675, 0.1564)
Virginia	84	0.0047 (-0.0074, 0.0227)	0.0703 (0.0500, 0.1271)
Washington	39	0.0518 (-.0119, 0.0971)	0.0845 (0.0448, 0.1449)
West Virginia	55	0.0040 (-0.0184, 0.0199)	0.0634 (0.0466, 0.0972)
Wisconsin	70	0.0270 (0.0077, 0.0716)	0.0390 (0.0231, 0.0688)

[34] Asymptotic convergence rates and 95% confidence intervals reported are for those estimates statistically different than zero in the 3SLS regressions.
[35] See Higgins, Levy and Young (2002) for full set of results for the United States.

Table 4: Analysis of Education Variables

Region	9-11 Years and No More		High School Diploma		Some College Education		Bachelor Degree or Higher	
	3SLS	95% C.I.	3SLS	95% C.I.	3SLS	95% C.I.	3SLS	95% C.I.
United States	-0.0221 (6.21)*	(-0.0292, -0.0152)	0.0097 (3.26)*	(0.0038, 0.0156)	-0.0025 (0.41)	(-0.0143, 0.0094)	0.0732 (12.01)*	(0.0613, 0.0852)
Alabama	0.0832 (2.07)**	(0.0015, 0.1649)	0.0832 (2.07)**	(0.0014, 0.1649)	0.1229 (1.41)	(-0.0538, 0.2997)	0.0448 (0.77)	(-0.0741, 0.1639)
Arkansas	-0.0223 (0.73)	(-0.0844, 0.0397)	0.1539 (0.49)	(-0.0478, 0.0786)	0.0492 (0.75)	(-0.0832, 0.1818)	0.1188 (1.56)	(-0.0346, 0.2723)
California	-0.0673 (1.02)	(-0.2041, 0.0694)	-0.0212 (0.60)	(-0.0941, 0.0515)	0.0513 (0.77)	(-0.0857, 0.1884)	0.1003 (2.36)*	(0.0126, 0.1880)
Colorado	0.1171 (2.44)**	(0.0191, 0.2151)	0.0654 (2.23)**	(0.0053, 0.1255)	0.0600 (0.90)	(-0.0769, 0.1969)	0.1178 (3.03)*	(0.0382, 0.1974)
Florida	-0.0045 (0.07)	(-0.1282, 0.1193)	0.0649 (0.95)	(-0.0750, 0.2048)	0.1813 (1.69)***	(-0.0372, 0.3998)	0.1094 (1.28)	(-0.0647, 0.2837)
Georgia	0.0087 (0.58)	(-0.0210, 0.0384)	0.0103 (0.60)	(-0.0240, 0.0447)	0.0715 (1.77)***	(-0.0084, 0.1515)	0.0279 (0.90)	(-0.0335, 0.0894)
Idaho	0.0612 (0.93)	(-0.0859, 0.2085)	0.0893 (2.31)**	(0.0031, 0.1755)	-0.0052 (0.11)	(-0.1135, 0.1030)	0.0656 (0.65)	(-0.1579, 0.2891)
Illinois	-0.0587 (3.22)*	(-0.0952, -0.0223)	-0.0149 (1.38)	(-0.0364, 0.0066)	0.0280 (0.89)	(-0.0345, 0.0907)	0.0495 (1.61)	(-0.0117, 0.1108)
Indiana	-0.0333 (1.31)	(-0.0842, 0.0175)	-0.0220 (1.37)	(-0.0542, 0.0102)	0.1129 (2.09)**	(0.0045, 0.2214)	0.0406 (0.82)	(-0.0584, 0.1396)
Iowa	-0.0314 (1.28)	(-0.0803, 0.0174)	-0.0003 (0.03)	(-0.0245, 0.0238)	0.0157 (0.58)	(-0.0383, 0.0698)	-0.0369 (1.20)	(-0.0983, 0.0244)
Kansas	-0.0281 (1.34)	(-0.0697, 0.0135)	0.0556 (4.49)*	(0.0309, 0.0804)	-0.0031 (0.16)	(-0.0414, 0.0351)	0.0403 (1.54)	(-0.0118, 0.0925)
Kentucky	0.0140 (0.51)	(-0.0410, 0.0692)	0.0562 (3.07)*	(0.0198, 0.0925)	0.0769 (1.77)***	(-0.0096, 0.1635)	0.0711 (1.55)	(-0.0202, 0.1625)
Louisiana	0.0188 (0.73)	(-0.0340, 0.0717)	-0.0178 (0.86)	(-0.0600, 0.0243)	0.0717 (1.19)	(-0.0509, 0.1943)	0.0707 (1.19)	(-0.0508, 0.1922)
Michigan	-0.0428 (1.34)	(-0.1067, 0.0211)	-0.0187 (0.77)	(-0.0677, 0.0302)	-0.0079 (0.15)	(-0.1131, 0.0972)	0.0873 (2.44)**	(0.0154, 0.1592)
Minnesota	-0.0142 (0.43)	(-0.0803, 0.0520)	0.0053 (0.31)	(-0.0290, 0.0396)	0.0441 (1.23)	(-0.0281, 0.1165)	0.0273 (0.64)	(-0.0578, 0.1126)
Mississippi	0.0095 (0.30)	(-0.0551, 0.0743)	-0.0950 (2.24)**	(-0.1804, -0.0095)	-0.0352 (0.57)	(-0.1595, 0.0891)	0.0182 (0.25)	(-0.1294, 0.1659)
Missouri	-0.0226 (0.83)	(-0.0773, 0.0319)	0.0187 (1.11)	(-0.0149, 0.0523)	-0.0271 (0.71)	(-0.1028, 0.0484)	0.1255 (3.50)*	(0.0542, 0.1969)
Montana	-0.1081 (1.54)	(-0.2529, 0.0368)	-0.0028 (0.07)	(-0.0856, 0.0799)	0.0100 (0.24)	(-0.0755, 0.0956)	0.0085 (0.17)	(-0.0983, 0.1154)
New York	0.0306 (0.68)	(-0.0616, 0.1229)	-0.0719 (1.51)	(-0.1697, 0.0258)	0.0193 (0.26)	(-0.1324, 0.1712)	0.1734 (3.31)*	(0.0661, 0.2807)
North Carolina	0.0395 (1.85)***	(-0.0031, 0.0821)	0.0223 (1.07)	(-0.0193, 0.0641)	-0.0162 (0.34)	(-0.1125, 0.0800)	0.1134 (3.02)*	(0.0384, 0.1883)
Ohio	0.0171 (0.60)	(-0.0400, 0.0742)	-0.0317 (1.87)***	(-0.0656, 0.0022)	0.1305 (2.62)*	(0.0305, 0.2306)	0.0691 (1.58)	(-0.0187, 0.1571)
Oklahoma	-0.0018 (0.05)	(-0.0769, 0.0732)	0.0556 (2.43)**	(0.0095, 0.1017)	0.0728 (1.54)	(-0.0226, 0.1683)	0.0230 (0.59)	(-0.0553, 0.1014)
Pennsylvania	-0.0021 (0.07)	(-0.0626, 0.0584)	-0.0331 (1.59)	(-0.0754, 0.0091)	0.0121 (0.20)	(-0.1105, 0.1348)	0.2049 (4.32)*	(0.1085, 0.3014)
South Carolina	-0.0098 (0.20)	(-0.1160, 0.0962)	-0.1084 (1.48)	(-0.2676, 0.0508)	0.1236 (0.66)	(-0.2876, 0.5355)	0.0301 (0.17)	(-0.3684, 0.4249)
South Dakota	-0.0904 (2.58)*	(-0.1621, -0.0186)	0.0181 (0.80)	(-0.0279, 0.0642)	0.0352 (0.72)	(-0.0647, 0.1353)	-0.0811 (1.39)	(-0.1999, 0.0376)
Tennessee	0.0093 (0.33)	(-0.0477, 0.0665)	0.0077 (0.26)	(-0.0511, 0.0666)	-0.0673 (1.10)	(-0.1897, 0.0549)	0.1732 (3.04)*	(0.0592, 0.2872)
Texas	-0.0594 (4.85)*	(-0.0836, -0.0353)	0.0061 (0.45)	(-0.0208, 0.0330)	0.0282 (1.28)	(-0.0153, 0.0717)	-0.0175 (0.70)	(-0.0667, 0.0315)
Virginia	0.0128 (0.48)	(-0.0412, 0.0668)	0.0157 (0.54)	(-0.0429, 0.0744)	-0.0211 (0.28)	(-0.1756, 0.1333)	0.0961 (1.68)***	(-0.0191, 0.2114)
Washington	0.0307 (0.21)	(-0.3521, 0.4135)	0.0146 (0.12)	(-0.2885, 0.3178)	0.0199 (0.28)	(-0.1656, 0.2055)	0.1531 (0.95)	(-0.2609, 0.5673)
West Virginia	0.0470 (0.81)	(-0.0733, 0.1674)	-0.0223 (0.50)	(-0.1146, 0.0700)	-0.0134 (0.15)	(-0.2037, 0.1768)	0.1089 (0.98)	(-0.1206, 0.3385)
Wisconsin	-0.0276 (0.95)	(-0.0855, 0.0308)	-0.0268 (1.62)	(-0.0605, 0.0067)	0.0229 (0.71)	(-0.0424, 0.0884)	0.0923 (2.56)**	(0.0195, 0.1652)

t-statistics are reported in parentheses

* significant at 1% level
** significant at 5% level
*** significant at 10% level

Map 1: Continental U.S. County Map

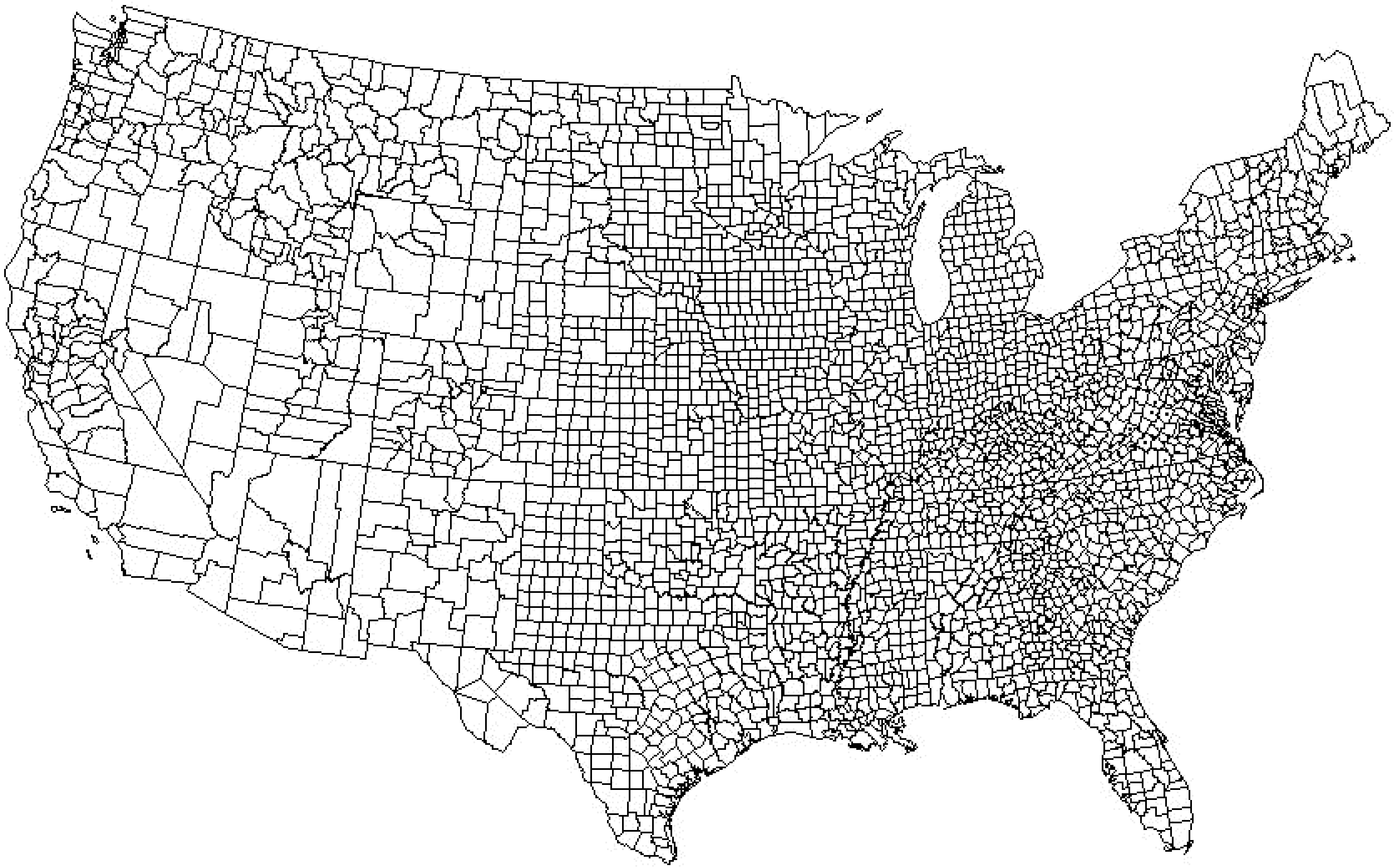

Figure 1:
95% Confidence Intervals and Point Estimates of Within-State Asymptotic Convergence Rates

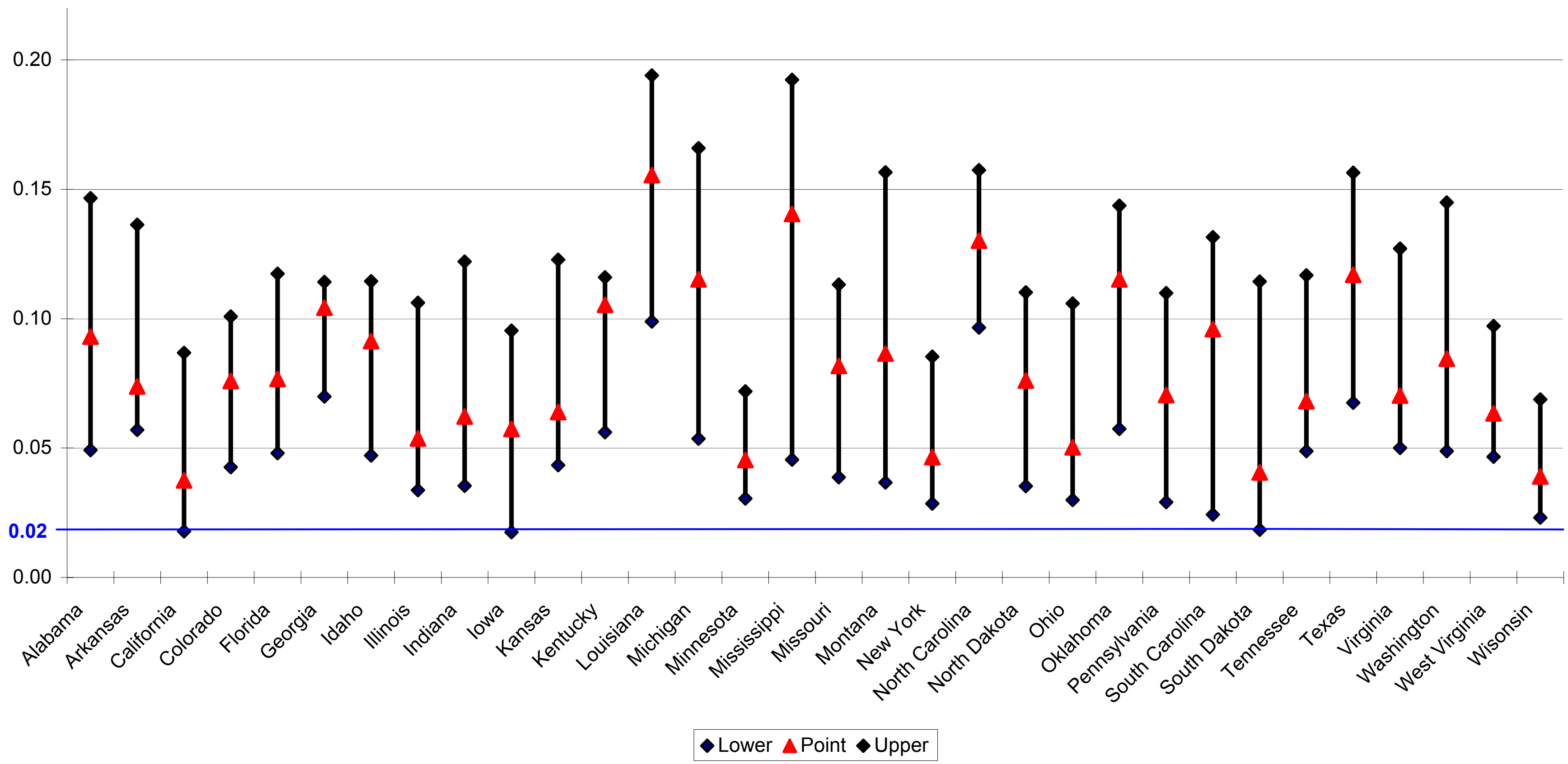

Figure 2:
95% Confidence Intervals and Point Estimates of Within-State Education Regression Coefficients: 9-11 Years and No More

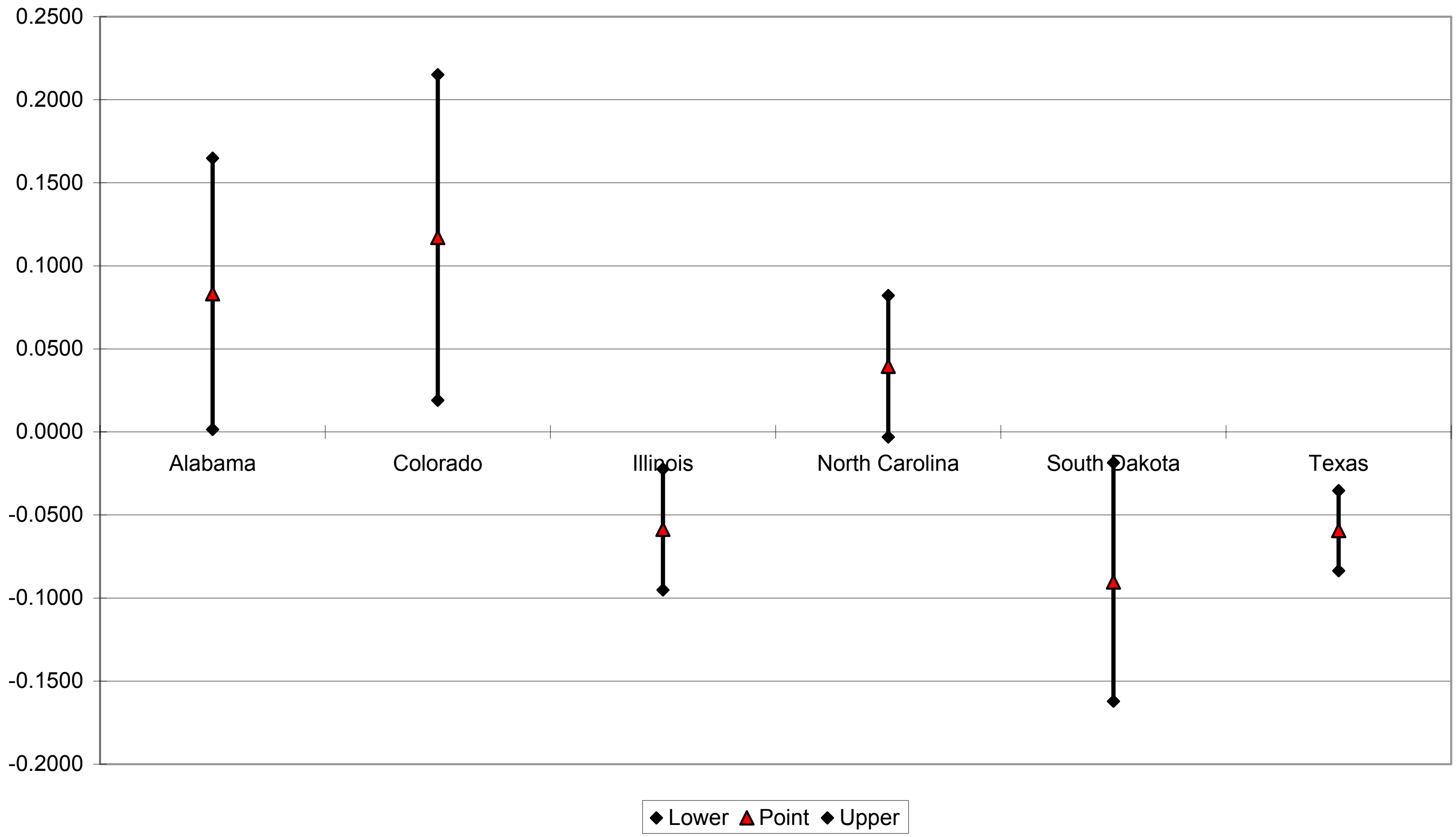

Figure 3:
95% Confidence Intervals and Point Estimates of Within-State Education Regression Coefficents: High School Diploma

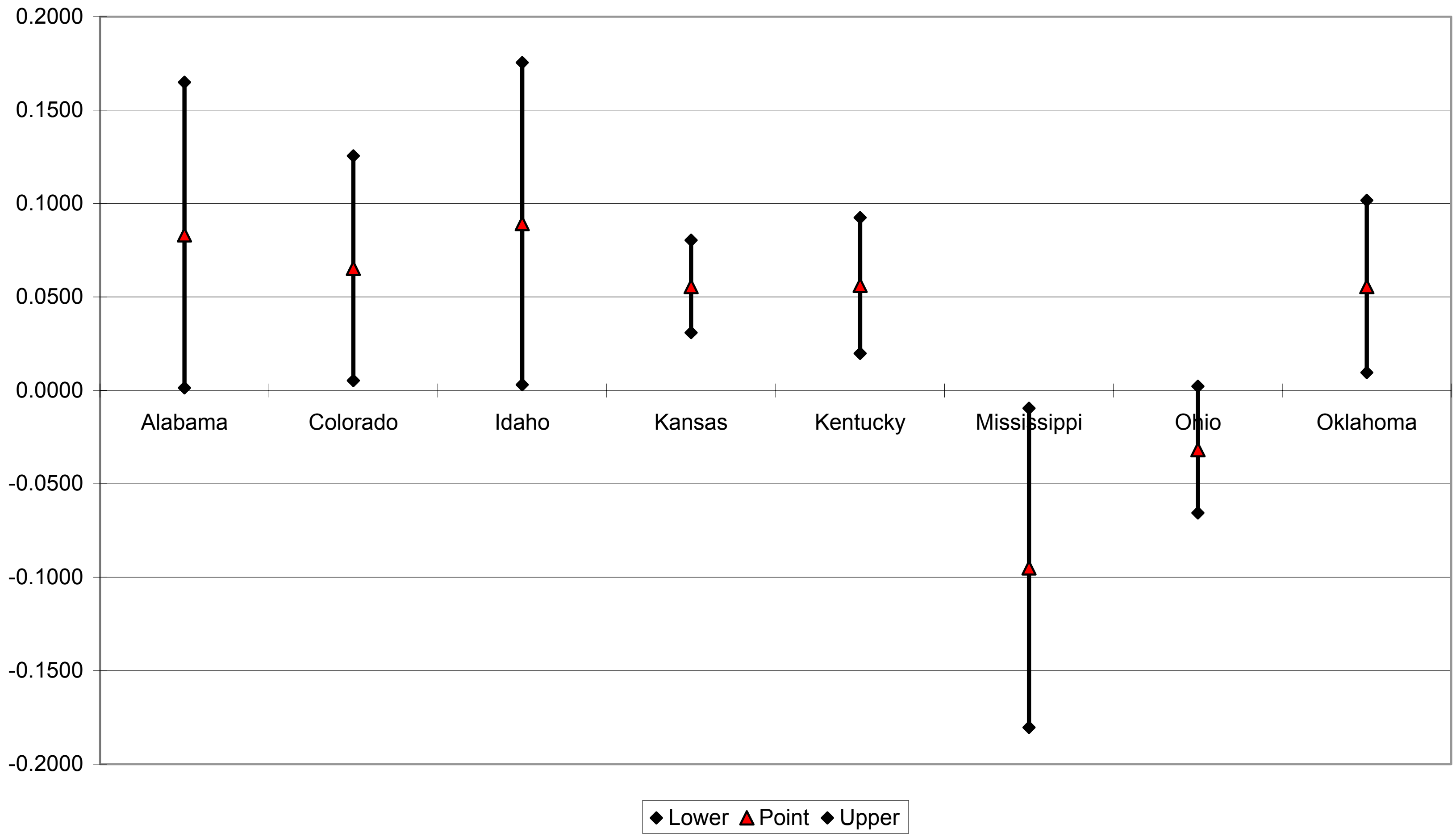

Figure 4a:
1998-1999 Average Total SAT Scores Versus High School Diploma Coefficient Point Estimates

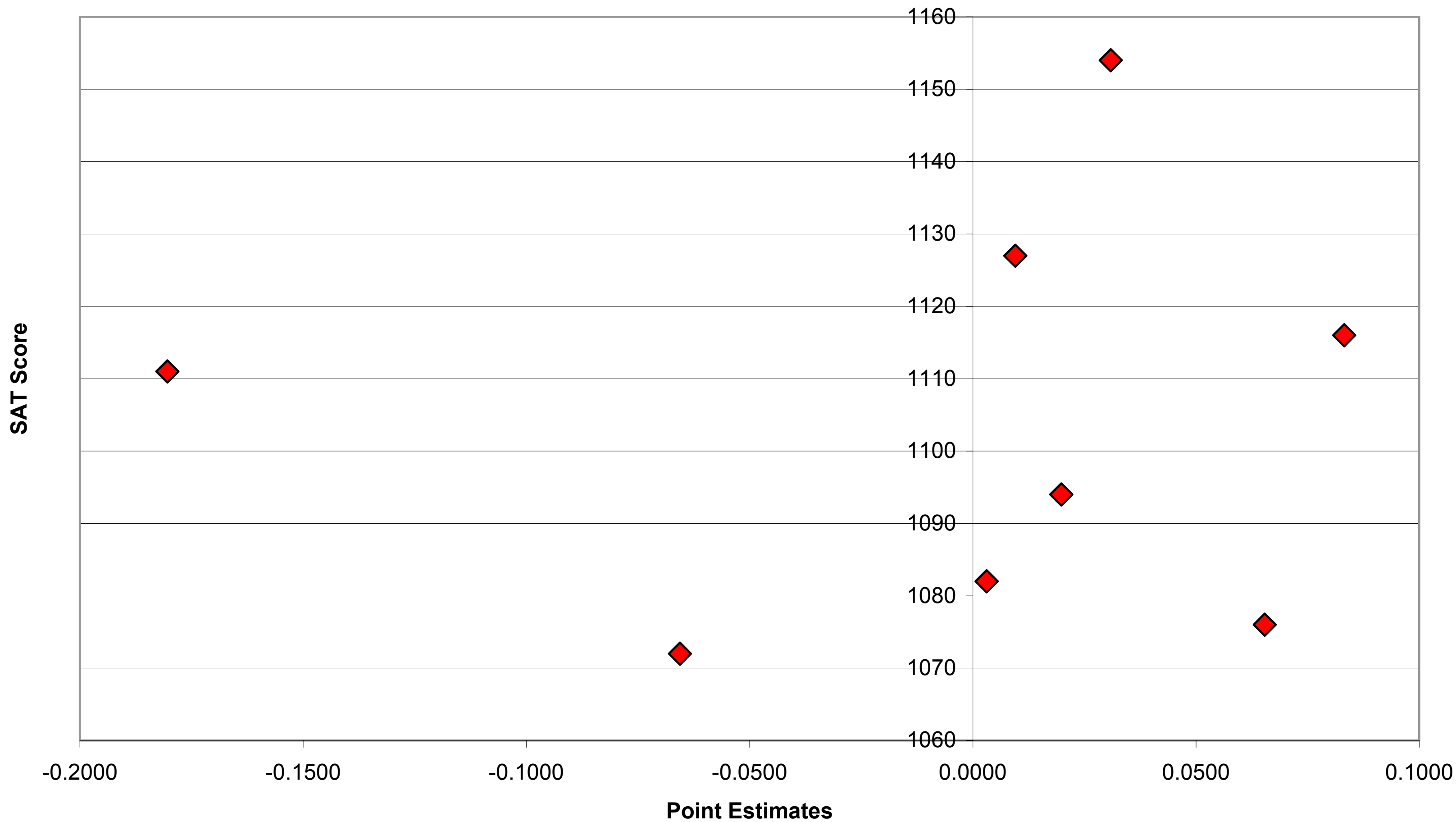

Figure 4b:
1998-1999 Average Math SAT Scores Versus High School Diploma Coefficient Point Estimates

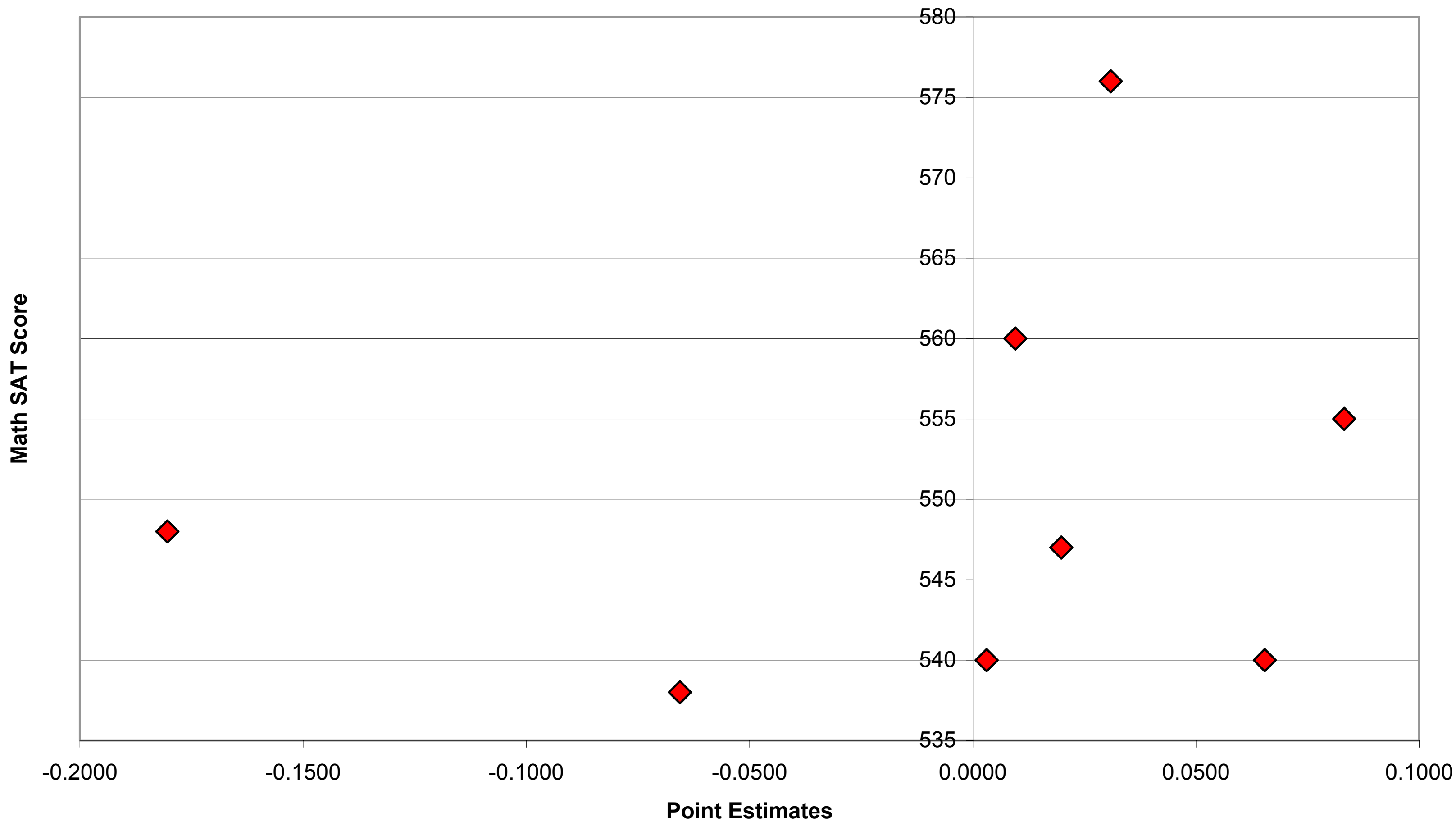

Figure 4c:
1998-1999 Average Verbal SAT Scores Versus High School Diploma Coefficient Point Estimates

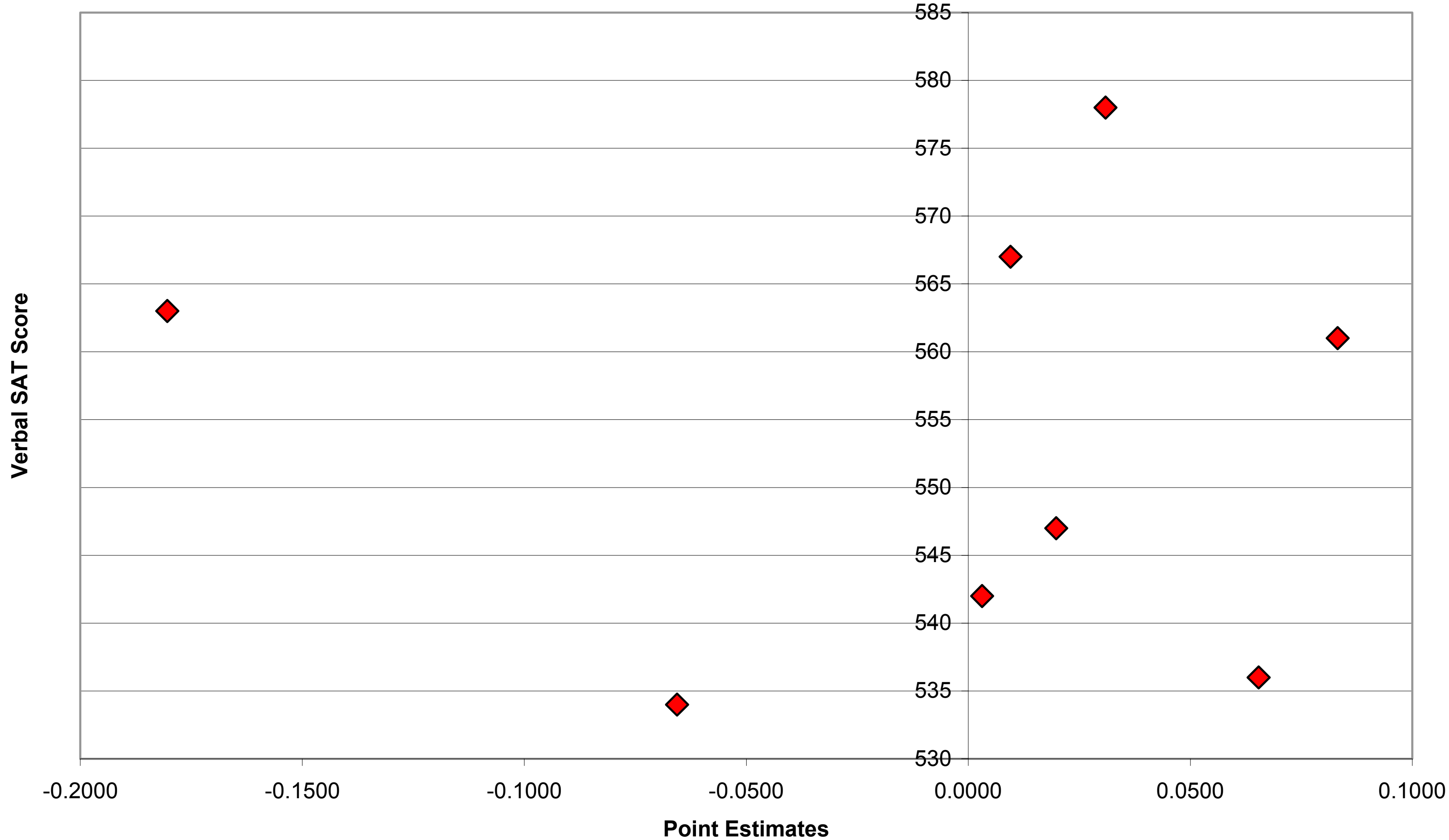

Figure 5:
95% Confidence Intervals and Point Estimates of Within-State Education Regression Coefficents: Some College

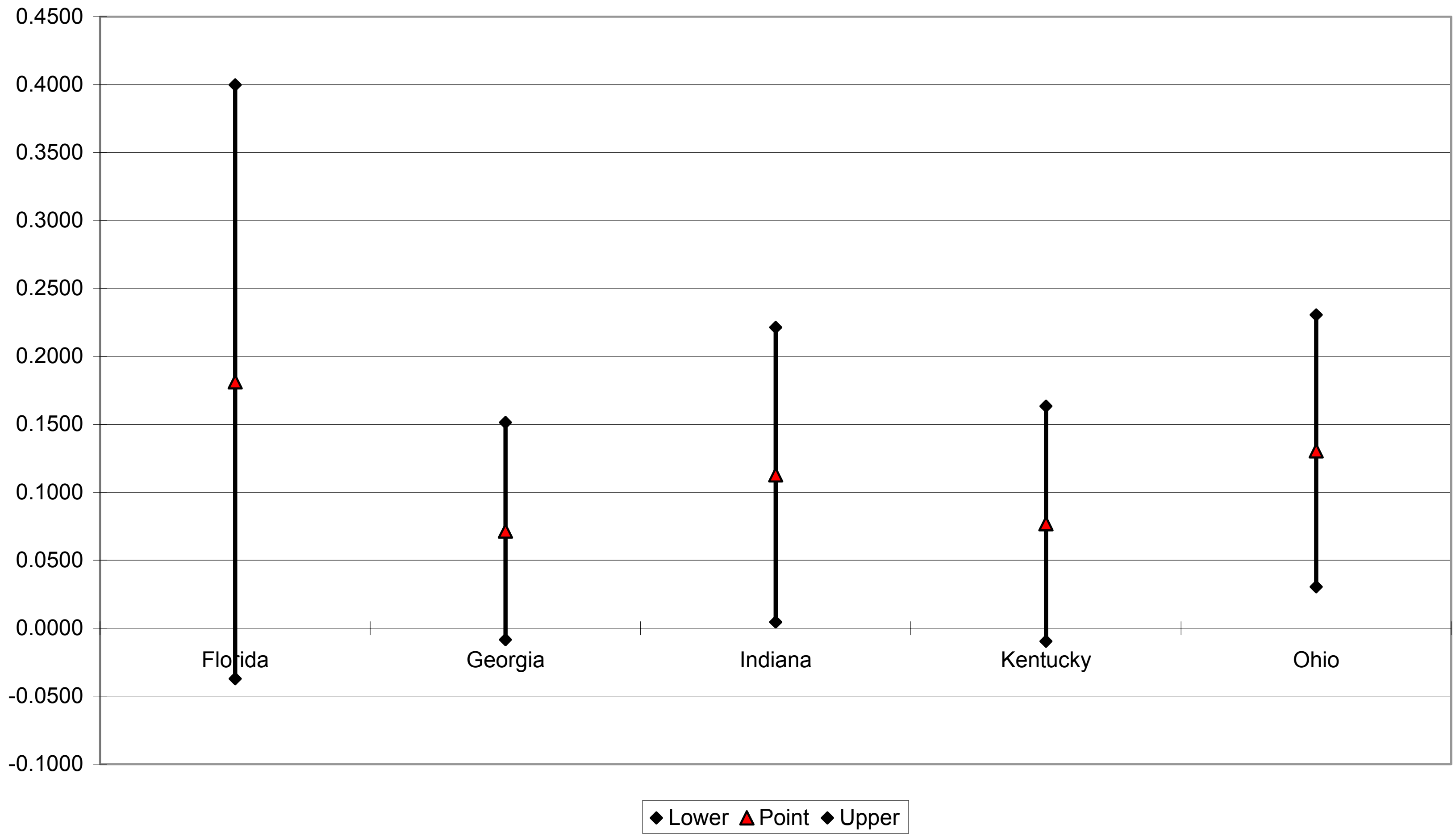

Figure 6:
95% Confidence Intervals and Point Estimates of Within-State Education Regression Coefficents: Bachelor Degree or Higher