A Non-Technical Introduction to Regression

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Abstract: This paper provides a non-technical introduction to regression analysis, a statistical technique commonly used by economists and others to summarize empirical relationships among variables. It is intended to make the basic idea accessible quickly to people with no prior training in statistics or econometrics. The paper covers topics such as regression with a single explanatory variable, regression with multiple explanatory variables, omitted variable bias, “bad control,” reverse causality, sampling error, standard errors, confidence intervals, statistical significance, and how to read and interpret a table reporting regression coefficients. This is all done in an intuitive and non-technical way relying on easy-to-interpret two-dimensional graphs, based on what Angrist and Pischke (2009) call the “regression anatomy” approach. The concepts are illustrated by investigating the empirical relationship between education and economic growth in cross-country data.

Introduction

A *regression* is a statistical technique for summarizing the empirical relationship between a variable and one or more other variables. In economics, regression analysis is, by far, the most commonly used statistical tool for discovering and communicating empirical evidence. This paper provides a non-technical introduction to regression analysis, illustrating the basic principles through an example using real-world data to address the following question: how does education affect the rate of economic growth? The goals of this paper are to help the reader understand the basic idea of what a regression means, learn how to read and interpret a table that presents estimates from a regression, and begin to appreciate some of the reasons why a regression may or may not provide credible evidence on any particular question. Introductory textbooks and courses in statistics and econometrics can provide you with a deeper, more mathematically sophisticated, and more precise understanding of regression analysis.[1] The purpose of this paper is just to give you a sense of the forest, before you delve into examining the individual trees.

The Question and the Data

The question we will investigate in this paper is: how does education affect the rate of economic growth? To address this, we will use data on 84 countries from around the world, assembled by Bosworth and Collins (2003). Table 1 describes the variables used in the analysis, and reports descriptive statistics for each one.

The "outcome" or "dependent" variable that we will seek to explain is the average annual percentage growth rate in real gross domestic product (GDP) per worker between 1960 and 2000. We'll refer to this variable as *growth,* and will use the symbol G_i to represent the value of *growth* for country i. We'll also use the terms "GDP" and "income" interchangeably, since GDP is a measure of the aggregate income of a country.

[1] Any introductory econometrics textbook, such as Stock and Watson (2007), would be a good resource for learning the technical mathematical details and understanding the issues in greater depth. Angrist and Pischke (2009) is a particularly good resource for more advanced students. Wheelan (2013) provides a highly accessible book-length introduction to statistics and regression analysis for those who prefer a very verbal, non-technical, non-mathematical exposition.

As Table 1 indicates, the mean value of *growth* in this sample of 84 countries is 1.60, meaning that on average, real GDP per worker grew by 1.6 percent per year. The value of *growth* ranges from a minimum of -1.34 to a maximum of 5.6.

Table 1 -- Variables and descriptive statistics

Variable name	Symbol	Description	Mean	Min-imum	Max-imum	Number of obser-vations
growth	*G*	Average annual growth rate of real GDP per worker, 1960 - 2000, in percentage points.	1.60	-1.34	5.60	84
initial education	*E*	Average years of education in 1960, working age population.	3.85	0.12	9.64	84
initial income	*Y*	GDP per capita in 1960, as a share of U.S. GDP per capita in 1960.	0.29	0.03	1.13	84
landlocked	*L*	Dummy variable equal to 1 if landlocked, 0 if not landlocked.	0.13	0.00	1.00	84
frost area	*F*	Share of land area that gets at least 5 days of frost per month in the winter.	0.39	0.00	1.00	84
ethnolinguistic diversity	*D*	Index of ethnolinguistic fractionalization (probability that two randomly selected people from the same country do not belong to the same ethnolinguistic group), 1960.	0.39	0.00	0.93	81

Source: Author's calculations based on data from Bosworth and Collins (2003).

The main "independent" or "explanatory" variable of interest is the average number of years of educational attainment among working-age people in the country in 1960. We'll refer to this as *initial education,* and will use E_i to symbolize the value of *initial education* for country *i*. The goal of our regression analysis will be to learn something about whether, and to what extent, countries with a higher initial level of educational attainment experienced better subsequent economic growth, eventually holding constant the effects of some other variables. There are numerous sensible reasons why, in theory, education might have a positive effect on economic growth. For example, education may teach useful skills, making workers more productive, thus increasing GDP. In addition, a better-educated nation might be better able to invent new technologies, or to adapt and implement existing productive technologies borrowed from other countries. Education could also improve the quality of country's governance if it helps the citizens of a country become better-informed voters and improves their ability to think critically. Better governance might in turn improve economic growth, for example because it reduces corruption, which can act like a tax, harming incentives to be productive, or which can divert revenues that otherwise would have been used to finance government expenditures that make the economy more productive.

Table 1 also includes information on four "control variables," which are additional explanatory variables that we might want to account for in our regression analysis.[2] The control variables are other variables which might have independent causal effects on *growth*, and which might be correlated with *initial education*. In that case, if we omitted[3] these other variables from our regression analysis, our regression would give us a misleading (or "biased") estimate of the causal effect of *initial education* on *growth*. When we "control" for these other variables in our regression analysis, we will be able to

[2] Every explanatory variable in a regression analysis can be considered a "control variable," but sometimes economists tend to call the particular explanatory variable that we are most interested in, and that we are focusing on at the time, the "explanatory variable of interest," and to call the *other* explanatory variables the control variables.

[3] "To omit" means to "leave out." So an omitted variable is a variable that is not included in our regression analysis. It might be omitted, for example, because it is impossible to measure or because we simply do not have any good data on it, or it might be omitted because it did not occur to us to include it, among other reasons.

say something about how a change in *initial education* would affect *growth* holding these other factors constant.

The first control variable is *initial income*, symbolized by Y_i. It represents the ratio of per capita GDP in 1960 in country *i* to per capita GDP in the U.S. in 1960. Other things equal, we would probably expect a country's initial level of income to have an independent *negative* effect on its subsequent economic growth rate – that is, countries that start out poorer, other things equal, might be expected to experience higher rates of economic growth, leading to "convergence" in levels of income across countries over time. This idea is most closely associated with the work of Robert Solow (1957). The idea is that in order to achieve sustained high rates of economic growth, countries that start out at a high level of income per person need to do difficult things, such as developing new technologies. Such countries tend to also already have a lot of physical capital (e.g., factories, productive machinery) per worker, and diminishing marginal returns to physical capital make it difficult for such countries to achieve high economic growth rates solely through additional saving and investment. It might, in principle, be easier for countries starting out with a lower level of income per person to achieve rapid and sustained economic growth, because the technology they need to grow already exists in other countries and they just need to copy it. In addition, countries that start out with lower incomes also start out with low levels of physical capital per worker, so the marginal benefit in terms of productivity from adding additional physical capital through saving and investment can be very large. On the other hand, countries starting with low levels of income per person tend to have all sorts of other problems, such as poor governance, which make it more difficult for them to grow. So it is not obvious, without examining the data, whether initial income should have a positive or negative effect on subsequent economic growth.

For most of this paper, we'll just focus on the relationship among *growth*, *initial education*, and *initial income*, but we'll also eventually consider some additional control variables, which I'll describe immediately below, to help illustrate some points that come up much later in the paper.

The next two control variables are both indicators of geographic characteristics of a country that might influence economic growth.[4] The variable *landlocked*, symbolized by L_i, equals 1 if the country is landlocked (meaning it does not have any direct access to an ocean) and zero if it is not landlocked.[5] *Landlocked* is an example of a “dummy variable” (also known as an “indicator variable”), meaning that the variable can take on just two values, zero and one. Being landlocked may have a negative effect on economic growth, for example because it makes it more difficult to engage in international trade, which hinders the country’s ability to specialize and achieve gains from trade. Being landlocked makes it harder for the country to interact with the outside world more generally, reducing the country’s ability to learn about and adapt new technologies or to benefit from inflows of capital investment from savers in the rest of the world. The variable *frost area*, symbolized by F_i, represents the share of the land area in the country that gets at least 5 days of frost (below-freezing ground temperatures) per month in the winter. This variable is originally from Masters and MacMillan (2001), who suggest it as a good summary measure of climactic conditions that might influence growth. When you look around the globe, you’ll notice that most of the very poor countries are located nearer to the equator, and most of the rich countries are located in temperate or cold climates. Some economists have argued that this is not entirely an accident. Very warm climates tend to be hospitable to disease-carrying or crop-destroying pests (e.g., malaria-infected mosquitoes), and it can be difficult for some such pests to survive in frosty conditions. Poor health and destroyed crops hinder economic productivity, and could conceivably make it more difficult to take advantage of productivity-enhancing technological advances happening in the rest of the world. For this reason, it makes sense that *frost area* might have a positive influence on economic growth.

The final control variable we will consider is *ethnolinguistic diversity*, symbolized by D_i. An “ethnolinguistic group” is a group of people who historically spoke the same native language and are of the same ethnicity. This variable represents the probability, as of 1960, that two randomly selected people from country *i* do not belong to the same

[4] For an interesting discussion of the role that geography might play in influencing economic growth, see Gallup, Sachs, and Mellinger (1999).

[5] The original source of the *landlocked* variable is Rodrik *et al*. (2002)

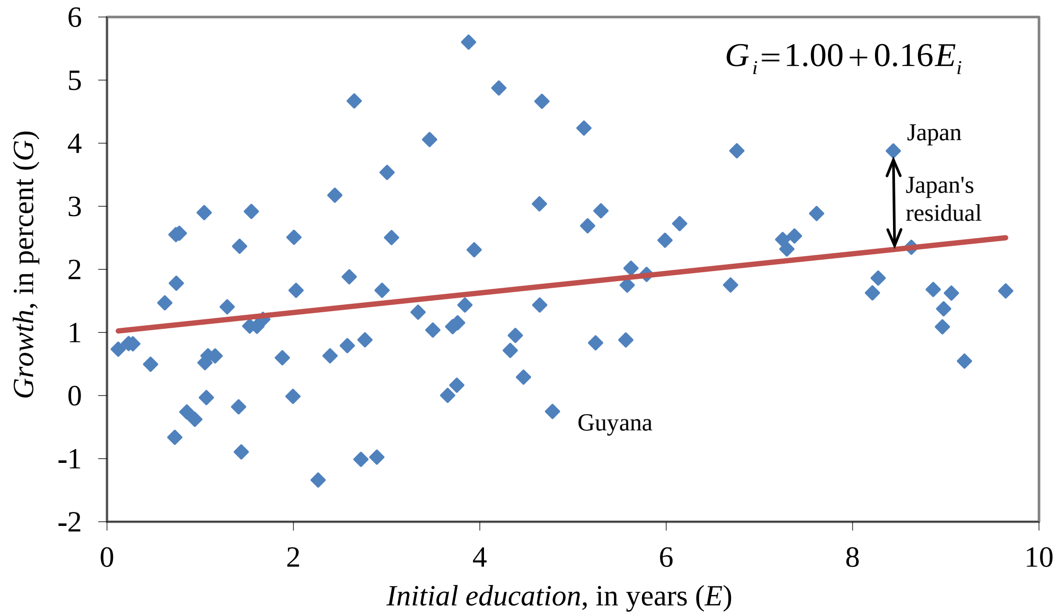
ehtnolinguistic group. Since it is a probability, it is measured on a scale from zero to one, and unlike a dummy variable, it can take on any value in between. If the value of the variable is zero, it means that everyone in the country is in the same ethnolinguistic group. If the value of the variable is one, it means that every single person in the country is in a different ethnolinguistic group from every other person in the country. In the sample of 84 countries, the average value of this variable is 0.39, the minimum value (for South Korea) rounds to 0.000, and the maximum value (for Tanzania) is 0.93. This variable was originally developed by the Department of Geodesy and Cartography of the State Geological Committee of the USSR (1964). Economists Paul Mauro (1995) and William Easterly and Ross Levine (2001) have argued that higher values for this variable have an important negative impact on economic growth. For example, they argue, when a single country has many different ethnolinguistic groups, it is possible that people in the country will be less willing to cooperate with each other (making it difficult to solve certain market failure problems, such as public goods problems, that require cooperation to solve), might be more prone to try to steal from each other through government (leading to higher corruption which hurts growth), or they might be prone to civil wars that also harm economic growth. This variable is only available for 81 of the 84 countries in our sample.

There are of course many other variables we can think of that might affect economic growth that we are not including here. In addition, there are many other questions we might want to ask about the best way to investigate the effects of various explanatory variables on economic growth that we are glossing over. For example, maybe should we be looking at how *changes over time* in education, as opposed to the *initial level* of education, affect economic growth? Or perhaps we are throwing away valuable information by collapsing data on economic growth into a single 40-year average? Those are indeed good questions, and there is a rich empirical literature on the determinants of economic growth that takes questions such as these very seriously and involves all sorts of clever strategies. We will leave all that aside for now, as our present purpose is just to illustrate how a regression works and what it means, not to provide thoroughly convincing evidence on what factors cause economic growth.[6]

Regression with a Single Explanatory Variable

Figure 1 illustrates the basic idea of a regression with a single explanatory variable. It shows, for each of 84 countries, the relationship between *initial education*, measured on the horizontal axis, and *growth*, measured on the vertical axis. Each dot represents a country. The dot's left-right location indicates the country's average years of educational attainment in 1960, and the dot's up-down location indicates the country's economic growth rate from 1960 to 2000. Looking at the cloud of dots suggests that there is a loose *positive correlation* between education and growth. That means that higher values of education tend to go together with higher values of growth, on average.

Figure 1 -- Annual growth rate in real GDP per worker 1960-2000 (*G*) versus average years of education in 1960 (*E*)



Residual ($\ddot{G}$) = actual *growth* minus *growth* predicted by *initial education* = height of dot minus height of regression line.

[6] Bosworth and Collins (2003) provide an example of a more thorough econometric analysis of the determinants of economic growth. Eberhardt and Teal (2011) discuss some of the major challenges that make it difficult to credibly estimate the causal effects of particular variables on long-run economic growth, and survey some newer, more sophisticated econometric methods meant to address some of these challenges.

The straight line drawn through the dots in figure 1 is the “ordinary least squares” (OLS) regression line. A regression line is a straight line that summarizes the relationship between two variables. The OLS regression line is meant to "fit" the cloud of dots as closely as possible, in the sense of summarizing the *average* relationship between *initial education* and *growth*. We will explain more precisely how it is computed and what it means a bit later. The equation for the OLS regression line shown in figure 1 is:

$$G_i = 1.00 + 0.16E_i \tag{1}$$

The height (or vertical axis value) of the regression line at a given level of *initial education* (E_i) is 1.00 + 0.16(E_i). This represents the growth rate that the OLS regression line "predicts" a country with that amount of education will have. The vertical axis intercept of the regression line, 1.00, tells us the predicted value of *growth* for a country with a value of zero for *initial education*. The slope of the regression line, 0.16, means that each one year increase in *initial education* is associated, on average, with an increase in the average annual growth rate in real GDP per worker of 0.16 percentage points. So for example, if a country had a growth rate of 1 percent per year, and added a year of education, its growth rate would be predicted to increase to 1.16 percent per year. Similarly, the OLS regression line predicts that a country with 5 years of *initial education* would have an average annual growth rate of $1.00 + 0.16 \times 5 = 1.8$ percent per year. The estimated slope of the regression line, 0.16, is called the “coefficient” on *initial education*. It is consistent with our intuition that *initial education* should have a positive effect on *growth*, but it is not a very large positive effect.

For each country in figure 1, there is a difference between the *actual* value of *growth* for that country, and the value of *growth* that is predicted for that country by the regression line. The actual value of the dependent variable (in this case, *growth*) minus the predicted value is called the “residual” or “error.” In other words, the residual for each country is the height of that country’s “dot” minus the height of the regression line at that country’s level for the explanatory variable (*initial education*). Countries with dots above the line have positive residuals, and countries with dots below the line have negative residuals. To help illustrate this, in figure 1, the dots for two particular countries, Japan and Guyana, are labeled with the country names. Japan's value of *initial education* is 8.43, and based on that, the regression line predicts a value for *growth* of 1.00 + 0.16×8.43 = 2.35. Japan's *actual* value of *growth* was 3.88. Japan's residual is its actual *growth* minus its predicted *growth*, 3.88 – 2.35 = 1.53. Guyana, on the other hand, has a value of *initial education* of 4.78, and the regression line predicts a value of *growth* for Guyana of 1.76. But Guyana's actual value for *growth* was -0.25, so its residual equals -0.25 -1.76 = -2.01 (that is, the residual for Guyana is *negative* 2.01). The residual can be thought of as the portion of the dependent variable (in this case, *growth*) that is not predicted by the explanatory variable (in this case, *initial education*). Many different factors, including random chance, affect *growth*, and the residual reflects the influence of these other factors.

The value of the dependent variable for each observation (country) always equals the value for that variable predicted by the regression, plus the residual. We will express this relationship in abstract terms as:

$$\overset{\text{Dependent variable}}{G_i} = \underset{\text{Intercept}}{b_0} + \overset{\text{Coefficient}}{b_1}\,\underset{\text{Explanatory variable}}{E_i} + \overset{\text{Residual}}{\ddot{G}_i} \tag{2}$$

where G_i is the value of *growth* for country *i* (the dependent variable) b_0 is the vertical axis intercept of the regression line (equal to 1.00 in figure 1), b_1 is the slope of the regression line (that is, the coefficient on E_i, which is equal to 0.16 in figure 1), E_i is the value of the explanatory variable *initial education* for country *i*, and $\ddot{G}_i$ is the estimated residual for observation *i*. For each distinct regression equation that we discuss in this paper, we will use different symbols to represent the intercept, coefficients, and residuals, because they will represent different numbers in different regressions.

There are actually many different possible methods for estimating a regression line. As noted above, in figure 1 we used the "ordinary least squares" (OLS) method to estimate the regression line, which is the most commonly used approach. OLS picks the unique values for the intercept and slope for the regression line that minimize the sum of squared residuals. This boils down to a calculus problem. First, recall that the residual ($\ddot{G}_i$ in this case) equals the actual value of the dependent variable minus the value of the dependent variable predicted by the regression. So,

$$\ddot{G}_i = G_i - [b_0 + b_1 E_i] \tag{3}$$

Minimizing the sum of squared residuals, as OLS does, involves solving the following calculus problem:

$$\textit{Choose } b_0 \textit{ and } b_1 \textit{ to minimize } \sum_i [G_i - (b_0 + b_1 E_i)]^2 \tag{4}$$

We will not get into the details of how the math works here, but it is a straightforward application of calculus.[7] Many different software packages, including Excel and Stata, can calculate OLS estimates of intercept and slope parameters for you.

Before moving on, we'll note a couple of properties of the OLS estimator that will be useful to know later. First, the OLS predicted value of the dependent variable is an estimate of the *conditional mean* of the dependent variable, given the value of the explanatory variable for that country. To put it another way, in figure 1, the value of *growth* predicted by the OLS regression line at each level of education represents what is in some sense the best estimate of the *mean* level of growth among countries with that level of *initial education* that we can get when assuming that the relationship between *initial education* and *growth* is described by a straight line. A second implication of the math of OLS worth noting here is that the mean value of the estimated residuals will, by construction, always be zero under this approach. That of course does not mean that each individual estimated residual will be zero – rather, it simply means that the residuals,

[7] See, for example, Stock and Watson (2007, Appendix to Chapter 4) for the derivation of the formulas for the OLS intercept and slope parameters using calculus. An example of another method for estimating a regression line, besides OLS, would be a "median regression," which minimizes the sum of the absolute values of the residuals.

some of which are positive and some of which are negative, will average out to zero over the whole sample.

Multiple Regression

Figure 1 illustrates the basic idea of a regression, but it does not do a very good job of answering the question we care about, "what is the causal effect of education on economic growth?" One reason is that countries with different values of *initial education* also differ from each other in all sorts of other characteristics that might also affect *growth*, and some of those other characteristics are systematically related to *initial education*. Therefore, the estimated effect of education on growth shown in figure 1 probably reflects some mixture of the true causal effect of education on growth, and the effects of many other influences on growth that tend to be systematically higher or lower in countries with more or less initial education.

Initial income is an example of another variable that influences growth which helps illustrate the point. First, for reasons noted above, there are good theoretical reasons for us to expect that, holding other things constant, countries with higher *initial income* will have lower subsequent economic growth. Second, even before we look at the data, it should be obvious that *initial education* and *initial income* are strongly correlated with each other – if you think about which countries had high levels of education in 1960, it of course would tend to be the countries that had higher incomes in 1960 (we will demonstrate this is empirically true below). If both of those things are true, then the coefficient on *initial education* that we estimated in figure 1, 0.16, represents some combination of the positive effect on *growth* of *initial education*, and the negative effect on *growth* of the high *initial income* that tended to go along with high *initial education*. In other words, the coefficient on initial education in figure 1 is probably unfairly blaming *initial education* for some of the negative effects of *initial income* on *growth*.

What we really want to know is: if we could *hold constant* the other factors (such as *initial income*) that influence *growth*, what would be the effect of *initial education* on growth? Or in other words, among countries with identical values of *initial income*, what was the effect of an additional year of education on *growth*? If you are a policy maker, that would be much more useful information than what we get from figure 1. If the 0.16

estimate that we came up with in figure 1 reflects the combined positive effect of *initial education* and negative effect of *initial income*, then it does not tell us anything about what would happen to growth in any particular country if the government of the country managed to increase average educational attainment by one year, without changing *initial income* (obviously, governments have no direct control over initial income). Our 0.16 estimate from figure 1 is a *biased* estimate of the thing that we are really interested in measuring, the causal effect of education on growth (holding other factors that influence growth constant) -- meaning that it will be systematically wrong on average. Our story above suggests that 0.16 is probably a *downwardly* biased estimate of what we want to know – in other words, the story we told about *initial income* gives us good reason to believe that 0.16 is systematically lower than the true causal effect of effect of education on growth holding other things constant.

This is where a *multiple regression* can help us. A multiple regression is a technique that is analogous to a regression with a single explanatory variable, like the one we described above, except now there are multiple explanatory variables. So for example, we can write out a linear equation that relates the dependent variable (*growth*, or G_i) to *two* explanatory variables (*initial education,* or E_i, and *initial income*, or Y_i). Equation (5) below does exactly that:

$$G_i = \beta_0 + \beta_1 E_\mathrm{i} + \beta_2 Y_i \tag{5}$$

where β_0 is the intercept, β_1 is the slope coefficient on *initial education* (E_i), and β_2 is the slope coefficient on *initial income* (Y_i).

The right-hand side of the equation above now represents the predicted value of a country's *growth* given its values of *initial education* and *initial income*. In each case, the *actual* value of the dependent variable (*growth*$_i$, also known as G_i) may differ from the value predicted by the regression equation, and the difference (actual G_i minus predicted G_i) is the residual. We'll label the residual in this equation ε_i. The regression equation can be estimated by ordinary least squares, which now selects β_0, β_1, and β_2 to minimize the sum of squared residuals ε_i. Doing so yields the following estimated relationship:

$$G_i = 0.94 + 0.49E_\mathrm{i} - 4.15Y_i \qquad (6)$$

In the multiple regression equation, β_1, which is estimated to be 0.49, now represents the change in *growth* (G_i) associated with a one unit increase in *initial education* (E_i) holding *initial income* (Y_i) constant. So, we would say that controlling for *initial income*, a one year increase in *initial education* is associated with a 0.49 percentage point increase in the annual economic growth rate. This is a much larger effect than the 0.16 percentage point increase suggested by figure 1. β_2, the coefficient on *initial income* (Y_i), is -4.15, which means that controlling for *initial education*, increasing *initial income* by one unit (where one unit is equal to the U.S. level of per capita GDP in 1960, a very large change) is associated with an annual growth rate that is 4.15 percentage points lower. When a one unit change in the explanatory variable is a very large change, it is sometimes useful to report the predicted effect of a smaller change that is more within the range of typical differences we see in the data. For example, dividing the β_2 coefficient by 10 gives us the effect of increasing *initial income* by $1/10^{\text{th}}$ of the U.S. level of *initial income*, holding initial education constant. So a country that starts out with initial income equal to 50 percent of that of the U.S., instead of 40 percent, holding initial education constant, is predicted to experience an annual growth rate that is 0.415 percentage points lower as a result. Thus, once we control for *initial education*, the sign of the effect of *initial income* on *growth* is negative, consistent with what the "convergence" theories we discussed earlier would predict.

To better understand what the β_1 coefficient in a multiple regression means, consider the following example. Imagine there were a large number of pairs of countries, where each pair of countries had identical levels of *initial income*, but differed in *initial education* by exactly one year. If we estimated multiple regression equation (5) above on such data, the coefficient β_1 on *initial education* would be precisely the growth rate of the country with one more year of education minus the growth rate of the country with one less year of education within each pair of countries with identical income, averaged over all pairs. So for example, if there were three such pairs of countries, and the growth rate of the country with one extra year of education in each pair was 0.2 percentage points higher in the first pair, 0.7 percentage points higher in the second pair, and 0.3 percentage points higher in the third pair, our estimate of β_1 would be the average of those differences, (0.2 + 0.7 + 0.3) / 3 = 0.4. So in other words, β_1 would be the answer to the question: “among countries with exactly the same level of *initial income*, what is the average difference in growth rates associated with one more year of *initial education*.” That’s what we want to know.

In practice, available data rarely include a complete set of perfectly matched pairs, each with identical values of the control variable and with values of the key explanatory variable of interest that differ by exactly one unit. In the more general case where the relevant variables vary more continuously than that, the OLS multiple regression coefficient β_1 still gets at exactly the same concept as described in the previous paragraph, but how it gets there is a bit more complicated to understand, and it relies more heavily on the assumption of straight-line relationships among the variables. In this setting, the OLS estimate of β_1 uses the data we actually have to compute our best estimate of what the average difference in growth rates between countries with identical *initial income* levels but *initial education* values differing by one year *would be*, under the assumption that the relationships among variables *growth*, *initial income*, and *initial education* are well-described by equations for straight lines. In the next section, we’ll work through some graphs which provide a clearer sense of what this means.

An equation for a multiple regression with two explanatory variables, such as (5), is an equation for a two-dimensional plane in 3-dimensional space. Imagine a graph with 3 axes. There are two horizontal axes that are perpendicular to each other – one measuring the value of *initial education* and the other measuring the value for *initial income* – and one vertical axis measuring *growth*. Imagine starting with figure 1 above, and adding a third axis for *initial income* that starts at the origin of figure 1, and pops out of the page at you, exactly perpendicular to the two-dimensional plane formed by the page. The cloud of dots would then be floating in 3-dimensional space, with 3-dimensional coordinates reflecting the values of the three variables for each country. Countries with higher values of *initial income* would have dots that pop out of the page more (i.e., closer to your eyes). The OLS multiple regression is a 2-dimensional plane that summarizes that cloud of points, minimizing the sum of squared residuals.

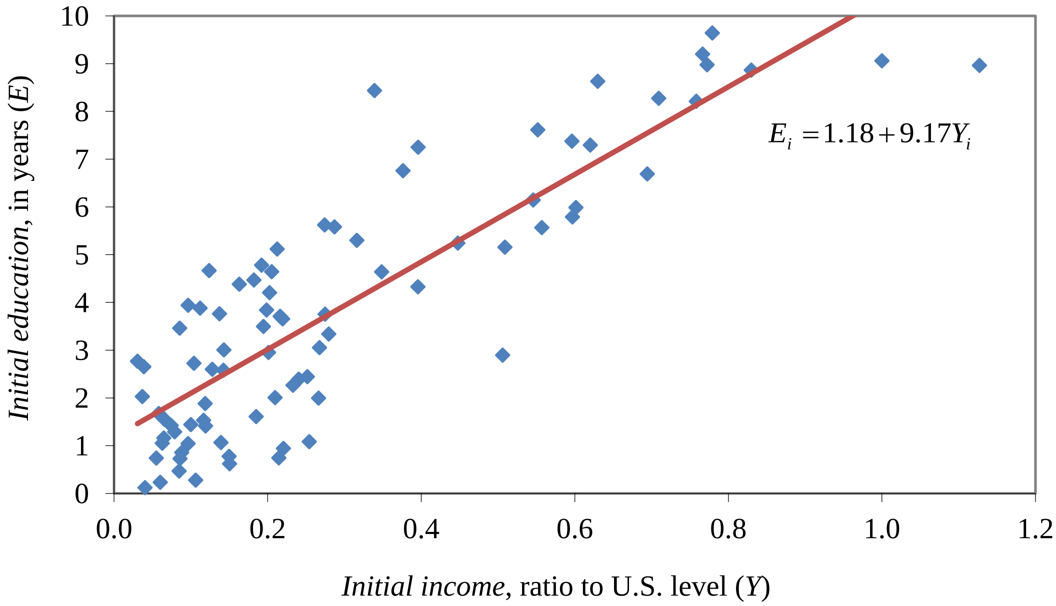
The "Regression Anatomy" Way of Understanding Multiple Regression

If you are like most humans, attempting to visualize a multiple regression in 3-dimensional space is not very helpful to understanding what the thing actually means, and attempting to visualize a multiple regression with more than two explanatory variables (thus necessitating more than 3 dimensions) is impossible. For this reason, it is helpful to break down what is going on in the multiple regression equation described above into component 2-dimensional parts. Angrist and Pischke (2009) call this the "regression anatomy" approach. Just as physical anatomy shows how the component parts of the body fit together to make the whole work, the regression anatomy approach mathematically decomposes a multiple regression into a set of regressions that each have just one explanatory variable, and it shows how they fit together to make the whole multiple regression work.

Figures 2, 3, and 4 below illustrate the regression anatomy approach to estimating β_1. The purpose of our multiple regression is to estimate the effect of *initial education* on *growth*, removing the effects of *initial income* from each one. The regression anatomy approach does exactly that. Figures 2 and 3 show how we can decompose *growth* and *initial education* into the parts that are and are not predicted by *initial income*, and then figure 4 shows the relationship between the portions of *growth* and *initial education* that are not predicted by *initial income*. The slope of the relationship in figure 4 will be exactly the β_1 coefficient that we are looking for.

Figure 2 shows the relationship between *initial education* (measured on the vertical axis) and *initial income* (measured on the horizontal axis) for each of our 84 countries. As we suspected, they are strongly positively correlated with each other.

Figure 2 -- Average years of education in 1960 (*E*) versus per capita GDP relative to U.S. in 1960 (*Y*)



Residual ($\widetilde{E}$) = actual *initial education* minus *initial education* predicted based on *initial income* = height of dot minus height of regression line.

The OLS regression line through the cloud of points in figure 2, in abstract terms, is:

$$E_i = a_0 + a_1 Y_i \tag{7}$$

The estimated value of a_1 is 9.17, meaning a one unit increase in *initial income* (i.e., from zero to the level of U.S. per capita GDP in 1960) is associated with 9.17 more years of *initial education*.

In figure 2, the main thing we care about for the purpose of ultimately estimating β_1 is the residual, which we will call $\widetilde{E}_i$. Recall that the residual represents the actual value of the dependent variable (in this case, *initial education*), minus value of that variable that would be predicted based on the explanatory variable (in this case, *initial income*). Or in symbols, $\widetilde{E}_i = E_i - (a_0 + a_1 Y_i)$. Also recall that our goal is to get β_1, which represents the effect of *initial education* on *growth* removing the effects of *initial income* from each one. The residuals in figure 2 give us one part of what we need to do that: $\widetilde{E}_i$ is a measure of the portion *initial education* that is different from what you would predict based on *initial income*, or in other words, it is a measure of *initial education* removing the effects of *initial income*. Or to put it another way, our goal is to get a measure of how *initial education* differs among countries with similar levels of *initial income*, and $\tilde{E}_i$ is one good way of measuring that.

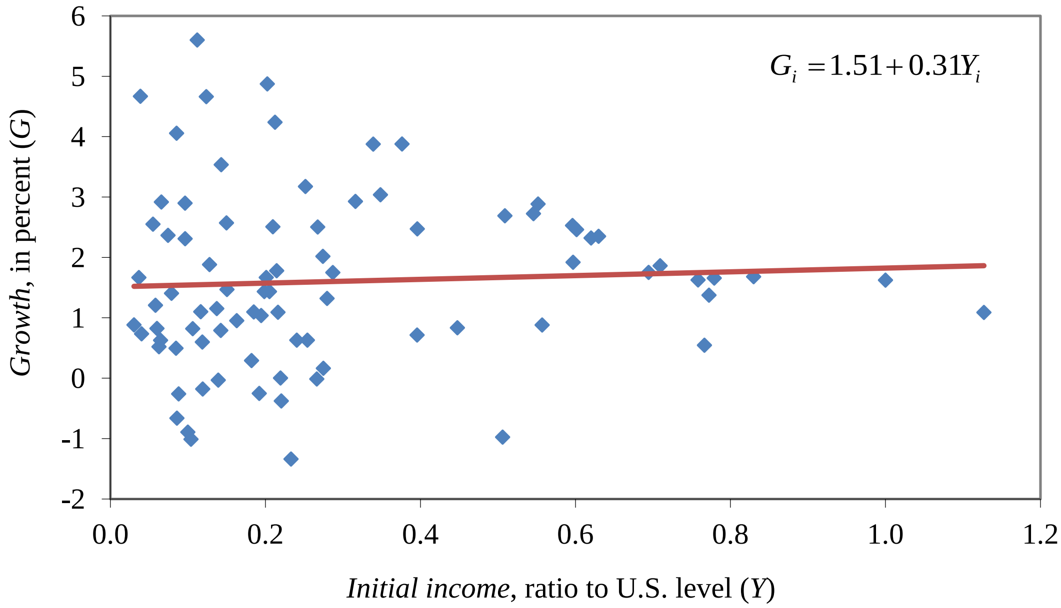
Figure 3 shows the relationship between *growth* (measured on the vertical axis) and *initial income* (measured on the horizontal axis) for each of our 84 countries. In figure 3, the correlation between *growth* and *initial income* is positive but not very strong.

The OLS regression line through the cloud of points shown in figure 3, in abstract terms, is:

$$G_i = c_0 + c_1 Y_i \tag{8}$$

The estimated value of c_1 is 0.31, which suggests that a one unit increase in *initial income* is associated with a 0.31 percentage point increase in *growth*. We should not take this as a refutation of our earlier theory that high *initial income* hurts *growth*, however. That theory said that higher *initial income* should be associated with lower subsequent economic growth *holding other things constant*. Figure 3 does not hold anything else constant. In particular, it does not hold *initial education* constant. So (as we will prove later below), the 0.31 slope coefficient estimate reflects a combination of the negative effect on *growth* of *initial income*, and the positive effect on *growth* of the high *initial education* that goes along with high *initial income*. Remember, back in equation (6), we estimated that the effect of *initial income* on *growth* holding *initial education* constant was indeed negative.

Figure 3 -- Average annual growth rate in real GDP per worker, 1960-2000 (G), versus per capita GDP relative to U.S. in 1960 (Y)

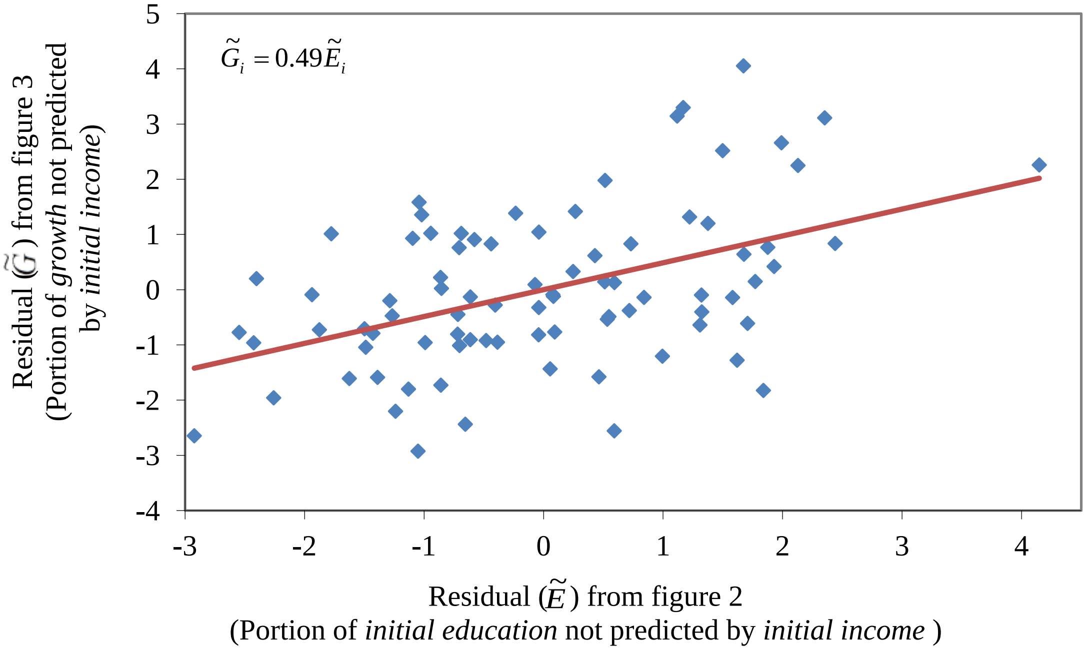


Residual ($\tilde{G}$) = actual *growth* minus *growth* predicted based on *initial income* = height of dot minus height of regression line.

Once again, the main thing we care about in figure 3 is the residual, which we will call $\tilde{G}_i$. $\tilde{G}_i$ represents the portion of *growth* that is different from what you would predict based on *initial income*, and it is the part that we want to keep in order to estimate β_1.

Figure 4 plots the portion of *growth* that is not predicted by *initial income* (that is, the residual $\tilde{G}_i$ from figure 3) on the vertical axis, against the portion of *initial education* that is not predicted by *initial income* (that is, the residual $\tilde{E}_i$ from figure 2) on the horizontal axis.

Figure 4 -- Portion of *growth* not predicted by *initial income*, versus portion of *initial education* not predicted by *initial income*
($\tilde{G}$ from figure 3 vs. $\tilde{E}$ from figure 2)



The slope of the OLS regression line in figure 4, 0.49, is *mathematically identical* to β_1, the coefficient on *initial education* in our multiple regression equation (6) above. That is, figure 4 gives us exactly what we want – this is a mathematically equivalent way of computing β_1 by a different method.[8] So the slope of regression line in figure 4 tells us that when we hold *initial income* constant, a one year increase in initial education is associated with a 0.49 percentage point increase in the annual growth rate in real GDP per worker. Consistent with our expectations, the estimated effect of *initial education* on *growth* is larger once we remove the effects of *initial income* from both variables, because high *initial education* is no longer being blamed for the negative effects on growth caused by the high *initial income* that goes along with it.[9]

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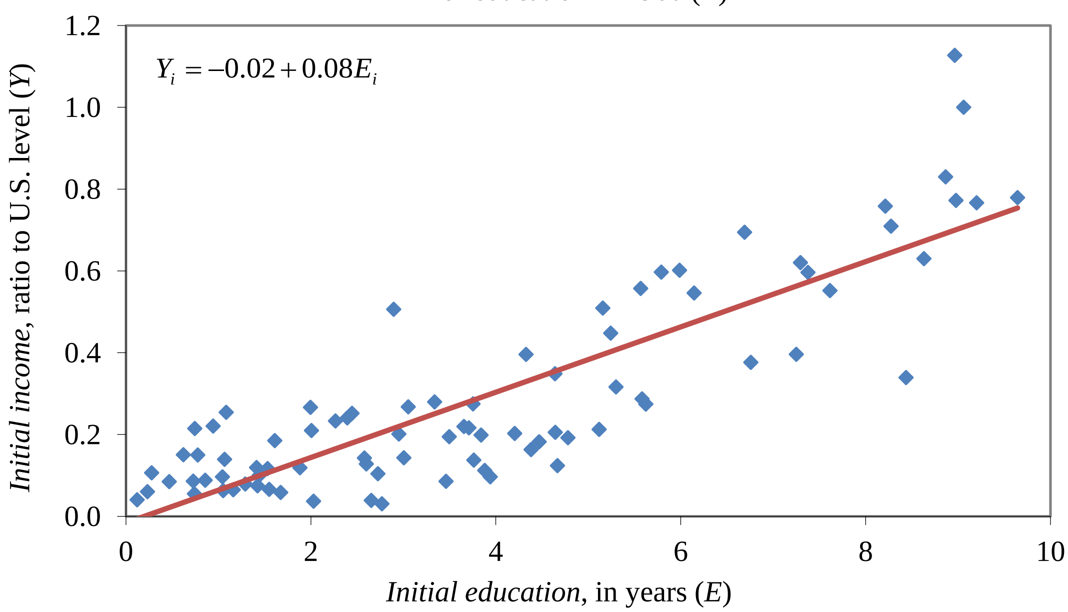
[8] Further explanation for why the regression anatomy approach works is provided in Angrist and Pischke (2009, Section 3.1.2). They attribute the idea originally to Frisch and Waugh (1933). You can also verify this is true for yourself by using Excel or Stata to estimate the multiple regression represented by equation (6), and then estimating the 2 regressions represented by figures 2 and 3, saving the residuals, and using them to estimate the regression represented by figure 4.

[9] The intercept for the regression line in figure 4 is precisely zero. This occurs because the intercept of a single-variable regression is always equal to the mean of dependent variable, minus the mean of the

Figure 4 thus illustrates in two dimensions what an estimate of β_1 from a multiple regression actually means. Even though we can easily estimate a multiple regression like equation (5) above using statistical software, going through this regression anatomy exercise is still useful, both because it can help you better appreciate what the multiple regression means, and also because a graph like figure 4 illustrates, in an easy-to-interpret picture, the nature of the evidence that is provided by the coefficient estimate from your multiple regression.

We can also construct the coefficient β_2, that is, the effect of *initial income* on *growth* holding *initial education* constant, in a manner analogous to how we constructed β_1. Figure 5 below shows the scatter plot relating *initial income* (on the vertical axis) to *initial education* (on the horizontal axis).

Figure 5 -- Per capita GDP relative to U.S. in 1960 (*Y*) versus average years of education in 1960 (*E*)



Residual ($\ddot{Y}$) = actual *initial income* minus *initial income* predicted based on *initial education* = height of dot minus height of regression line.

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explanatory variable times the coefficient on the explanatory variable. So if the OLS regression line shown in figure 4 is expressed using the equation $\tilde{G}_i = \alpha_0 + \alpha_1\tilde{E}_i$, then the intercept $\alpha_0 = (mean\ of\ \tilde{G}_i) - \alpha_1(mean\ of\ \tilde{E}_i)$. Because both $\tilde{G}_i$ and $\tilde{E}_i$ are residuals from other regressions, by construction, each one has a mean of zero, so the intercept α_0 equals zero. This will be true for the OLS regression of any residual variable against any other residual variable, so it also explains why the intercept is zero in figure 6 below.

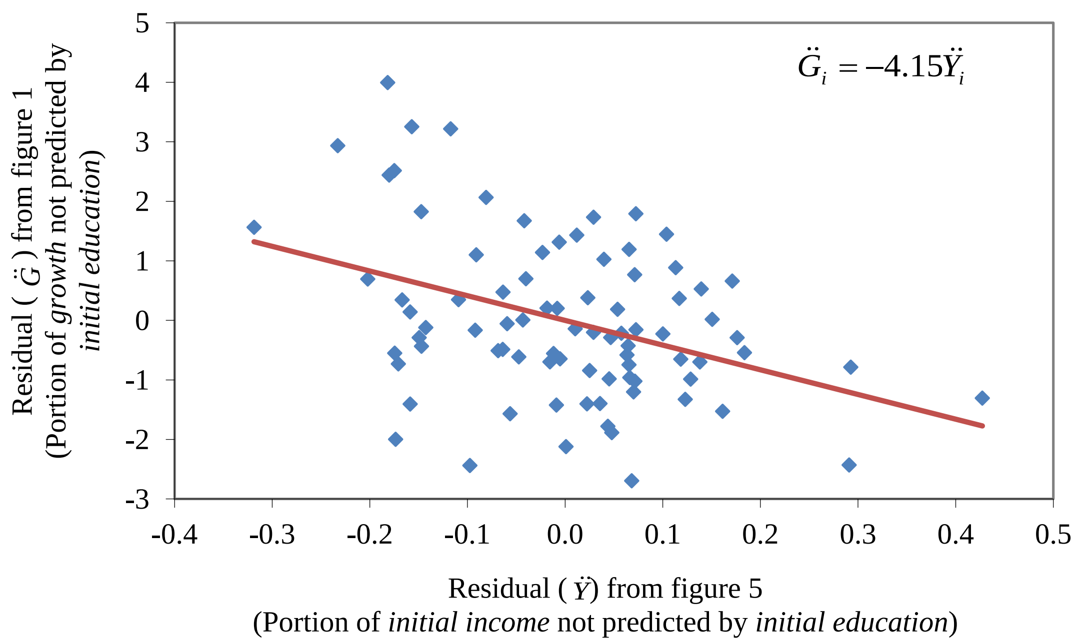
We'll express the equation for the OLS regression line in figure 5 as:

$$Y_i = d_0 + d_1 E_i \tag{9}$$

The residual in figure 5, which we'll call $\ddot{Y}$, represents the portion of *initial income* not predicted by *initial education*, and it is the part that we want to keep in order to estimate β_2. The other thing we need to estimate β_2 is the portion of *growth* not predicted by *initial education*, which is the residual ($\ddot{G}$) from figure 1 (which plotted the relationship between *growth* on the vertical axis and *initial* education on the horizontal axis).

Figure 6 plots the portion of *growth* not predicted by *initial education* (the residual $\ddot{G}$ from figure 1) on the vertical axis against the portion of *initial income* not predicted by *initial education* (the residual $\ddot{Y}$ from figure 5) on the horizontal axis. The slope of the OLS regression line through the cloud of points in figure 6 is -4.15, and this is our estimate of β_2. It is mathematically identical to the coefficient on Y_i that we get if we estimate the multiple regression equation (5) directly by OLS.

Figure 6 -- Portion of *growth* not predicted by *initial education*, versus portion of *initial income* not predicted by *initial education* ($\ddot{G}$ from figure 1 vs. $\ddot{Y}$ from figure 5)



Now we have a basic understanding of what a multiple regression means. But to be an informed consumer of regression analysis, it is also important to have a solid understanding of why a regression analysis might provide misleading answer to the question it is meant to investigate. There are many different ways that a regression analysis can go wrong. In what follows, I'll provide brief introductions to four of the most important categories of potential problems: omitted variable bias, bad control, reverse causality, and sampling error. After that, I'll discuss ways to quantify the degree of uncertainty arising from sampling error, introducing concepts such as "standard error," "confidence interval," and "statistical significance," and will point out some common confusions about these concepts that you should be careful to avoid right from the outset. Finally, I'll go through an example of a table presenting coefficient estimates from regressions based on the data described above, and explain how to interpret such a table.

Omitted Variable Bias

When the purpose of our regression is to estimate the causal effect of one variable on another variable, holding other things constant, as it usually is, one of the most serious challenges we face is the problem of *omitted variable bias*. Omitted variable bias means that our estimate of the effect of one variable on another variable is a biased (systematically wrong on average) estimate of the causal effect we want to estimate, because we've omitted (left out) another variable from the regression. It occurs whenever we omit a variable which affects the dependent (outcome) variable and is correlated with one or more of the explanatory variables that are included on the right-hand-side of our regression. The example we worked through above can help us to understand omitted variable bias, and to understand the likely direction of bias in a variety of scenarios.

Consider again figure 1. In that figure, the estimated effect of an additional year of *initial education* on *growth*, 0.16, is a biased estimate of what we want to know, the causal effect of *initial education* on *growth* holding other things constant. As we demonstrated above, *initial income* also affects growth and is correlated with *initial education*, so it is a source of omitted variable bias in the short regression of *growth* on *initial education* -- equation (1), illustrated in figure 1. Including *initial income* as a control variable, as we did in the long (multiple) regression shown in equation (5), solves that particular source of omitted variable bias.

It turns out that there is a precise mathematical relationship among the coefficients in the "short" regressions (the various regressions with just one explanatory variable) and the coefficients in the long regression (the multiple regression in equation 5, with two explanatory variables) that we estimated in our example above. We can express the relationship between a short regression coefficient and the corresponding long regression coefficient with the omitted variable bias formula.[10] In the context of our example, the omitted variable bias formula for the effect of *initial education* on *growth* is:

Short-regression coefficient for effect of *initial education* on *growth*

$$b_1 = \beta_1 + \underbrace{\beta_2 d_1}_{\text{Bias}} \qquad (10)$$

Long-regression coefficient for effect of *initial education* on *growth*

Remember that β_2 was the coefficient on *initial income* from the long regression (i.e., the slope of the regression line in figure 6), and d_1 was the coefficient on *initial education* from the short regression of *initial income* on *initial education* (i.e., the slope of the regression line in figure 5). Another way of writing the omitted variable bias formula that makes its meaning a little clearer is as follows:

$$\frac{\Delta G}{\Delta E} = \frac{\Delta G}{\Delta E}_{|Y\ constant} + \frac{\Delta G}{\Delta Y}_{|E\ constant} \times \frac{\Delta Y}{\Delta E} \qquad (11)$$

That is, the unconditional change in *growth* associated with a one unit increase in *initial education* ($\frac{\Delta G}{\Delta E}$, also known as b_1) is the sum of two things: (1) the change in *growth* associated with a one unit increase in *initial education* holding *initial income* constant

[10] For more information on the omitted variable bias formula, see for example Angrist and Pischke (2009), section 3.2.2, or Stock and Watson (2007), section 6.1.

($\frac{\Delta G}{\Delta E}_{|Y\ constant}$, also known as β_1); and the change in *growth* associated with a one unit increase in *initial income* holding *initial education* constant ($\frac{\Delta G}{\Delta Y}_{|E\ constant}$, also known as β_2), times the unconditional change in *initial income* associated with a one unit increase in *initial education* ($\frac{\Delta Y}{\Delta E}$, also known as d_1).

We can verify that the omitted variable bias formula is correct by substituting in the estimated values of each coefficient into equation (10):

$$0.16 = 0.49 + (-4.15)\times(0.08) \tag{12}$$

Do the math in equation (12) and you'll see that the omitted variable bias formula is indeed correct (aside from a bit of rounding error arising from the fact we've rounded the coefficients to two decimal places).

If our goal were to estimate β_1, then b_1 is a misleading indicator of that. The omitted variable bias formula clarifies what we would need to know to determine the size and direction of the bias. The formula says that the b_1 coefficient combines the effect of *initial education* on *growth* holding *initial income* constant (β_1) with a bias term $\beta_2 d_1$. Even if we had only estimated the short regression of *growth* on *initial education* (equation 1), we could come up with a pretty good guess at the *direction* of bias (up or down) if we thought carefully about the problem. This is an important part of critical thinking about regression estimates, especially given that we can often think of relevant variables that are not included in the analysis and that may not even be measurable. The direction of bias can matter a lot for the practical implications of the regression evidence. For instance, if we have a plausible omitted variable story that probably suggests the estimated coefficient on initial education is too small, that would tend to strengthen the case for investing in education, whereas if the omitted variable bias story suggests that the estimated coefficient is too big, that would weaken the case. The omitted variable bias formula helps us think clearly about these sorts of things.

In general, the sign of the bias depends on the sign of the long-regression coefficient on the omitted variable, times sign of the correlation between the included explanatory variable and the omitted variable. Recall that our intuitive story for the bias caused by omitting *initial income* was as follows. In the short regression of *growth* on *initial education* shown in figure 1, higher levels of *initial education* were being unfairly blamed for the negative effects on growth of the higher levels of *initial income* (implicitly, a statement that β_2 is negative) that tend to go along with higher *initial education* (implicitly a statement that d_1 is positive). So, we guessed that the true effect of *initial education* on *growth* holding *initial income* constant (β_1) would be larger than b_1 (or in other words, we surmised that b_1 was downwardly biased as an estimate of β_1). The omitted variable bias formula confirms our intuition that when β_2 is negative and d_1 is positive, β_1 will be greater than b_1. Our subsequent empirical analysis confirmed that β_2 was indeed negative and d_1 was indeed positive, so that β_1 (0.49) was indeed greater than b_1 (0.16), and that b_1 equaled β_1 plus the bias term, $\beta_2 d_1$ = (-4.15)×(0.08) = -0.33. When we control for *initial income* in the multiple regression (equation 5), we correct this bias, and are able to estimate β_1.

The omitted variable bias formula for the effect of *initial income* on *growth* is:

$$c_1 = \beta_2 + \beta_1 a_1 \tag{13}$$

In words, this means that the coefficient on *initial income* from the short regression of *growth* on *initial income*, which was 0.31, equals the coefficient on *initial income* from the long regression (-4.15), plus the coefficient on *initial education* from the long regression (0.49) times the change in *initial education* associated with a one unit increase in *initial income* (9.17). The bias term $\beta_1 a_1$ equals 0.49×9.17 = 4.49 (this is close to the difference between 0.31 and -4.15, but slightly off due to rounding error). Thus, our short regression estimate of the effect of *initial income* on *growth*, 0.31 was very upwardly biased as an estimate of β_2, which turned out to be -4.15.

Omitted variable bias is a ubiquitous problem in regression analysis. In cases where it is possible to measure variables that we suspect might matter, we can fix the problem by measuring those variables, and then including them as control variables in a multiple regression. But it is frequently the case that we can think of variables that might matter but which are unobservable, or at least are not measured in any available data. If you study econometrics further, you can learn about strategies that, under certain conditions, will enable you to solve omitted variable bias problems even when relevant omitted variables are unobservable.[11]

Bad Control

Including additional control variables in a multiple regression can be good, by helping to solve omitted variable bias problems. But there are some situations where including certain control variables in a regression could actually be bad, giving us a worse answer to the question we are interested in instead of a better one. An example is the problem that Angrist and Pischke (2009) call "bad control." Generally speaking, the problem of "bad control" occurs when you include a control variable that is part of the causal effect you are trying to estimate, or in other words, where the control variable is a channel through which the main explanatory variable of interest influences the outcome variable.[12]

An example can make the idea clear. Suppose we have data on a large number of adults, including information on the years of education each person completed, his or her wage, and a dummy variable equal to one if the person has a highly skilled, white-collar professional job (e.g., doctor, lawyer, executive), and zero if the person is in a lower-skilled, blue collar job (e.g., manual laborer). If our goal was to estimate the causal effect of years of education on wage, it would be a bad idea to control for the dummy variable for type of job. The reason is that one of the main ways that education affects one's wage is through its affect on what kinds of jobs you are qualified to do. If we controlled for a rich enough array of information on the type of job one has, we might find that our estimated coefficient on years of education was pretty close to zero. If that were the case, it would obviously be stupid to conclude that education was worthless. Education might in fact have had a very large causal influence on one's wage -- it just had all of its effect through its influence on one's type of job. Controlling for job type absorbed that effect, and left little variation in wage for the years of education variable to explain. If the

[11] Examples include difference-in-differences, fixed effects estimation, instrumental variables, and randomized experiments. See, for example, Stock and Watson (2007) chapters 10, 12, and 13.

[12] Angrist and Pischke (2009), section 3.2.3, offers a more formal treatment of the problem of "bad control." The same problem is sometimes called "post-treatment bias" – see, for example, King (2010).

question we really want to answer is "how does education affect one's adult wage?", the answer to the question "how does education affect one's adult wage holding type of job constant?" does not give us what we want to know at all. You would be better off omitting the occupation indicators from the regression.

The omitted variable bias formula can help us see the nature of the problem. Suppose W_i is an individual's wage, E_i is an individual's years of education, and J_i is the aforementioned dummy variable that is equal to one if the person has a high-skill job and zero if the person has a low-skill job. The left-hand-side of the equation is the coefficient from a regression where W_i is the dependent variable and where E_i is the only explanatory variable, and the first term on the right-hand-side is the coefficient on E_i from a regression where W_i is the dependent variable and both E_i and J_i are included as explanatory variables:

$$\frac{\Delta W}{\Delta E} = \frac{\Delta W}{\Delta E}_{|J\ constant} + \frac{\Delta W}{\Delta J}_{|E\ constant} \times \frac{\Delta J}{\Delta E} \qquad (14)$$

On the right hand side, $\frac{\Delta J}{\Delta E}$ is probably positive because more education causes you to get a higher-skill job, and $\frac{\Delta W}{\Delta J}_{|E\ constant}$ is probably positive because a higher-skill job tends to pay better. Thus the "bias" term $\frac{\Delta W}{\Delta J}_{|E\ constant} \times \frac{\Delta J}{\Delta E}$ is positive, so $\frac{\Delta W}{\Delta E}$ will be larger than $\frac{\Delta W}{\Delta E}_{|J\ constant}$. But in this case, for most purposes (such as deciding whether more education is a good investment), $\frac{\Delta W}{\Delta E}$ is a lot closer to what we actually want to know than $\frac{\Delta W}{\Delta E}_{|J\ constant}$. Controlling for type of job gives us an estimated effect of education on wage that is a downwardly biased answer to the question we are interested in.

In some cases, it is not obvious whether including additional control variables will give us a better or worse answer to the question we are interested in. Some portion of the variation in a potential control variable might be a channel through which the main explanatory variable of interest affects the outcome variable, while some other portion of the variation in the potential control variable might not be caused by the main explanatory variable of interest, yet might be correlated with the explanatory variable of interest and might affect the outcome variable. In that case, we might get biased estimates of the causal effect of the main explanatory variable of interest whether we include the potential control variable or not. Including the control variable might absorb some of the causal effect we are trying to estimate, while excluding it induces omitted variable bias. In that case, there's no easy solution.[13]

Here is an example that applies to our cross-country regression on the effects of education on growth. Another variable that might influence economic growth, and that is undoubtedly positively correlated with education, is quality of governance (e.g., lack of corruption, degree of accountability and transparency in government, checks and balances, effectiveness of government at getting things done, etc.). Suppose we had an indicator of the quality of governance and included it as a control variable in our regressions meant to estimate the effect of education on growth. Would that give us a better or worse answer to the question "how does education influence economic growth?" It is not clear. On the one hand, quality of governance might be a channel through which education might improve growth. A more educated populace may be better able to hold its government accountable and reduce corruption, for example because high rates of literacy enable people to read the newspapers and stay informed about what is happening in politics. In that case, if we controlled for quality of governance, maybe our coefficient on education would not be giving education enough credit. On the other hand, omitting the indicator of quality of governance is not necessarily a good solution either. Perhaps high quality governance caused by factors other than education causes both high educational attainment (because the education system works better when government is more effective, competent, and uncorrupt) and also causes high growth (by improving the security of property rights and improving incentives to invest, for example). In that case, omitting quality of governance from the regression might give too much credit to education for fostering growth.

Reverse Causality

In our regression analysis example above, we made *growth* our left-hand-side variable and *initial education* our right-hand-side variable. But of course, this does not guarantee

[13] King (2010) argues that this is an especially important "hard unsolved problem" in the social sciences.

that the direction of causality runs only from *education* to *growth*. It could well be that faster economic growth causes people in a given country to choose to get more education, for example because education is easier to afford when you are richer, or maybe because peoples' tastes change in a way that is more favorable to education as they become richer. If *growth* causes *education*, then the coefficient on *education* in a regression where growth is treated as the dependent variable might give us a very misleading impression of the true causal effect of *education* on *growth*. Even if *education* had no causal effect on *growth* at all, we might still estimate a positive coefficient on *education* because of the reverse causality running from *growth* to *education*. That reverse causality would induce a positive association between growth and education in the data, and the coefficient on *education* would pick up that positive association. Thus, we might conclude that education has a causal effect on growth when in fact it has no effect at all. It is just responding to growth.

Focusing on the effect of *initial* education (in 1960) on subsequent growth (from 1960 through 2000) is one strategy for dealing with this problem. The hope is that something in the past cannot be caused by something in the future. This is far from foolproof, though. For example, *growth* tends to be positively correlated over time for a given country – the countries that grow faster in one period tend to grow faster in the next period. So *initial education* might have been caused by past *growth*, and maybe we estimate a positive effect of *initial education* on subsequent growth only because past *growth* caused the *initial education* and *growth* is correlated over time. Or maybe people are forward-looking, and people who expect their country to experience high economic growth in the future respond by investing more in education today as a result, because investments in education pay off more when future growth is expected to be higher. In addition, for various reasons it might make more sense to investigate the effects of *changes over time* in education on *growth*, as opposed to the effects of initial levels of education. For example, theoretically, it may not make so much sense to think of the level of education at a given point in time having a permanent effect on the growth rate. But comparing growth with changes over time in education would undoubtedly exacerbate the any reverse causality problems. Reverse causality is always a difficult problem to solve.

Further study of econometrics offers some strategies which can solve the problem under certain conditions.

Sampling Error

Sampling error is another reason why a regression estimate could provide a misleading answer to the question we are interested in. Regression estimates are generally based on a sample, rather than on data for an entire population. If we were to estimate our regression over and over again on different samples randomly selected from a population, we would get somewhat different estimates of our regression intercept and coefficients each time, because in each sample we draw a different set of observations, each of which has a different amount of random residual variation in the outcome variable. As a result, there is some risk that just due to random chance, we will estimate a relationship between the explanatory variable and the outcome variable that is very different from that in the population, and we might even estimate a strong relationship between them when in fact there is no systematic relationship between them in the population at all.

A simple example can illustrate the nature of the problem. Suppose we want to estimate the average height of students in a 40-person class. We'll treat the class as the relevant population. If we were to estimate the height of the class by randomly selecting 5 people, measuring them, and calculating their average height, and then did this repeatedly, we would *on average* get an unbiased estimate of the average height of the class, but sometimes due to random chance we would happen to select 5 unusually tall students and substantially overestimate the average height of the class, and on other occasions due to random chance we would happen to select 5 unusually short students and would substantially underestimate the average height of the class. So any particular estimate based on a sample of 5 students could be very different from the mean height of the population (the entire class). The smaller is the sample, the larger is the probability that we are getting a misleading estimate of the population parameters in any particular estimate. If we estimated the average height of the class based on just one person, there's a pretty high probability we'd be off by a wide margin on any given estimate, whereas if we estimated the average height of the class based on a sample of 39 out of the 40 students, the probability of the estimate being very different from the population mean would be much smaller.

The same problem applies when we estimate a regression. In our example where we used multiple regression to examine the effects of *initial education* and *initial income* on *growth*, we used a sample of 84 countries, and we used a single 40-year time period for each country to measure *growth*. We can think of the relevant population here as all possible countries and all possible time periods from the past and future. Even if we're really only interested in the relationship between education and growth for this particular set of countries in this particular time period, we still have to worry about the fact that we could be estimating a positive relationship between them due purely to random chance, which is always possible when you have a relatively small sample size. So it is useful to think of this in terms of sampling from a population even if the particular sample you are investigating is of interest in and of itself. When we estimate a regression using data from a particular sample, we get sample estimates of the various *parameters* in our multiple regression equation: the intercept, the two coefficients, each country's error term (residual), etc. The estimated parameters in this particular sample might be very different from the values of these parameters for the population, just due to random chance. For example, figure 4 shows a strong positive relationship between *initial education* and *growth* holding *initial income* constant. But it is at least possible that this finding is due to random chance. Perhaps we just happened to select a sample where an unusually large number of the countries that had high levels of *initial education* for their *initial income* levels and also happened to have very large positive *true* residuals (that is, residuals computed using the population parameters rather than the parameters estimated in this particular sample), thus putting lots of dots in the upper-right-hand corner of figure 4. In that case, it could be that *in the population* there is no systematic relationship between *initial education* and *growth* holding *initial income* constant, and maybe we found a strong relationship in our sample due to random chance.

Standard Errors, Confidence Intervals, and Statistical Significance

Fortunately, statistics provides us with ways to quantify the degree of uncertainty arising from sampling error that is associated with each parameter of our regression model. One such indicator of the degree of uncertainty arising from sampling error is the “standard error.” Further study of statistics can teach you the details of how a standard error is calculated and why the things I’m about to say about it are true. Here, we’ll just consider the basic idea of what standard error means, and discuss in pragmatic terms how you can use it to interpret the uncertainty associated with regression estimates that is due to sampling error.

One particularly useful thing to do with a standard error for a regression coefficient is to construct a “confidence interval.” A “95 percent confidence interval” around a particular coefficient is a range of numbers, where the bottom of the range is the estimated coefficient minus approximately two times the estimated standard error, and the top of the range is the estimated coefficient plus approximately two times the estimated standard error. To put it in symbols, when the sample is large, the 95 percent confidence interval for a particular estimate of a coefficient β_1 is approximately $[\hat{\beta}_1 - 2\widehat{SE_{\beta_1}}, \hat{\beta}_1 + 2\widehat{SE_{\beta_1}}]$. A “hat” over a parameter (for example, the pointy thing on top of $\hat{\beta}_1$) indicates that we are talking about an estimated value of the parameter based on this particular sample, as opposed to the “true” value of the parameter in the population, which goes hatless. $\hat{\beta}_1$ is our estimate of the coefficient β_1 based on data from this particular sample, and $\widehat{SE_{\beta_1}}$ is our estimate of the standard error of $\hat{\beta}_1$ in this particular sample. The 95% confidence interval for our estimate of β_1 in this particular sample is thus a range of numbers going from $\hat{\beta}_1 - 2\widehat{SE_{\beta_1}}$ to $\hat{\beta}_1 + 2\widehat{SE_{\beta_1}}$. [14]

What does the 95 percent confidence interval mean? Imagine that you could randomly select samples from a population over and over again, and that each time you did this, you re-estimated $\hat{\beta}_1$ and $\widehat{SE_{\beta_1}}$ and constructed a 95 percent confidence interval using those estimated parameters. Each time, you’d get a somewhat different estimate of the parameters and a somewhat different confidence interval. It turns out that approximately

[14] To be precise, the width of the confidence interval depends not only on the estimated coefficient and standard error, but also on the “degrees of freedom,” which is *n* – *k* – 1, where *n* is the number of observations in the sample, and *k* is the number of explanatory variables in the regression. When the degrees of freedom is greater than about 600, the 95 percent confidence interval is $[\hat{\beta}_1 - 1.96\widehat{SE_{\beta_1}}, \hat{\beta}_1 + 1.96\widehat{SE_{\beta_1}}]$. In a regression with 84 observations and 2 explanatory variables like the one we’re considering in this article, the 95 percent confidence interval is $[\hat{\beta}_1 - 1.99\widehat{SE_{\beta_1}}, \hat{\beta}_1 + 1.99\widehat{SE_{\beta_1}}]$. For the purposes of this non-technical introduction, rounding to approximately two times the standard error is close enough.

95 percent of the times that you did this, the confidence interval you constructed would contain the true value of the population coefficient β_1. Or to put it a different way, for any given large sample, if we conclude that the population coefficient β_1 is somewhere between $\hat{\beta}_1 - 2\widehat{SE_{\beta_1}}$ and $\hat{\beta}_1 + 2\widehat{SE_{\beta_1}}$, there is about a 95 percent probability that we are correct.

It would also be possible to construct a 90 percent confidence interval, which is approximately $[\hat{\beta}_1 - (1\tfrac{2}{3})\widehat{SE_{\beta_1}},\ \hat{\beta}_1 + (1\tfrac{2}{3})\widehat{SE_{\beta_1}}]$.[15] There's nothing magical about the use of 95 percent or 90 percent to construct the confidence intervals. Those are just commonly-used conventions. Study of statistics teaches you how to construct confidence intervals using whatever percentages you want.

Another useful thing you can do with a standard error is to use it to determine whether a particular estimated coefficient is "statistically significant" in its difference from some number (usually zero). We say that a parameter estimate is "statistically significant in its difference from zero at the 5 percent significance level" if the estimated 95 percent confidence interval around that estimated parameter does *not* include zero.[16] This means that if the true value of the population parameter were zero, there is less than a 5 percent probability that we would have estimated a parameter as large (in absolute value) as we did. Thus, we can be fairly confident that the population parameter involves *some* non-zero effect. We say a parameter estimate is "statistically *in*significant in its difference from zero at the 5 percent significance level" if the estimated 95 percent confidence around that estimated parameter *does* include zero. That simply means that if the population value of the parameter were truly zero, there is *more* than a 5 percent probability that we would estimate a parameter as large (in absolute value) as we did in a sample of this size. Analogously, you can determine whether or not a parameter is

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[15] To be more precise, the 90 percent confidence interval is $[\hat{\beta}_1 - 1.64\widehat{SE_{\beta_1}},\ \hat{\beta}_1 + 1.64\widehat{SE_{\beta_1}}]$ when degrees of freedom is greater than about 600, and is $[\hat{\beta}_1 - 1.66\widehat{SE_{\beta_1}},\ \hat{\beta}_1 + 1.66\widehat{SE_{\beta_1}}]$ with the 81 degrees of freedom that we have in the main multiple regression example in this article. For our purposes, rounding to $[\hat{\beta}_1 - (1\tfrac{2}{3})\widehat{SE_{\beta_1}},\ \hat{\beta}_1 + (1\tfrac{2}{3})\widehat{SE_{\beta_1}}]$ is close enough.

[16] Technically, this is for a "two-tailed test." In some situations a "one-tailed test" might be more appropriate for the question at hand. Consult a statistics textbook for further discussion of these issues.

statistically significant at the 10 percent level by constructing the 90 percent confidence interval and seeing whether or not it includes zero.

While this is not the place to go into exactly how a standard error is calculated, we can get a rough idea of what it depends on based on the graphs shown earlier in this paper. In figure 4, which showed the relationship between the portion of *initial education* that is not predicted by *initial income* and the portion of *growth* that is not predicted by *initial income*, the slope of the OLS regression line through the cloud of points gives us our estimated $\hat{\beta}_1$. If the cloud of points was very loosely arrayed around the regression line in figure 4, with many points very far away from the line and many points that don't fit the general upward sloping pattern of the cloud, then we would tend to get a large estimated standard error on our estimate of $\hat{\beta}_1$. By contrast, if the cloud of points were very tightly arrayed around the OLS regression line in figure 4, we would tend to get a smaller estimated standard error on our estimate of $\hat{\beta}_1$. Intuitively, when the points are tightly arrayed around the regression line, that suggests that the true residuals are likely to be pretty small (in absolute value) in most cases. The variation in estimates of $\hat{\beta}_1$ from sample to sample is caused by variation in the true residuals of the observations that happen to be included in each sample. Other things equal, when those true residuals tend to be smaller in absolute value, the variation in our estimates of $\hat{\beta}_1$ from sample to sample will be smaller, leading to less uncertainty about how far $\hat{\beta}_1$ might be from the population parameter, and thus a smaller standard error.

Common Misinterpretations of the Meaning of Statistical Significance

There are a number of ways to misinterpret what statistical significance means, and it is best to get these sources of confusion out of the way early. The first is to confuse "statistical significance" with "economic significance." When interpreting a coefficient estimate, we always care about at least *three* things: the *sign* of the coefficient (that is, the estimated direction of the effect), the *size* of the coefficient, and the *degree of uncertainty* in the estimate. "Economic significance" (also known as "social significance," or "importance") is about the size of the coefficient, or in other words, it is about whether the estimated coefficient is "big" or "small" in a meaningful economic or social sense. By contrast, statistical significance is about the degree of uncertainty arising from sampling error, and does not necessarily tell us anything about the size or importance of the effect at all. Statistical significance and economic significance are totally different concepts, and one does not necessarily imply the other. “Statistical significance” is a very poorly named concept, because it actually has nothing at all to do with “significance,” which in the English language means “importance.” This unfortunate naming convention is a source of much confusion, but it is too late to change it now.

To take an example, in our multiple regression (equation 6), we estimated that an additional year of *initial education* is associated with an annual economic growth rate that is 0.49 percentage points higher, controlling for *initial income*. That is a big effect, especially given that the mean annual growth rate was only 1.60 percent. A country that grows at 1.6 percent per year for 40 years will be 1.89 times as rich 40 years from now as it is today. A country that grows at 2.09 percent per year (that is 1.6 percent + 0.49 percent) for 40 years will be 2.29 times as rich 40 years from now as it is today, or 21 percent richer than it would be if it had only grown at 1.6 percent per year. That is a substantial difference. You’d be pretty happy if your income was 21 percent higher, no? If the standard error on that estimate was 0.1, we’d conclude that the estimate is statistically significant at the 5 percent level, whereas if the standard error were 0.4, we would conclude that it is not statistically significant at the 5 percent level.[17] But either way, the estimate would have the same *economic* significance.

If we find that an estimate is *not* statistically significant, we cannot conclude that the effect is zero, and we cannot necessarily conclude that the effect is economically unimportant either. If the confidence interval includes both zero and economically significant effects, all we can conclude is that *we are not sure* whether the effect is zero or big. We simply can’t answer the question that we set out to answer. Sometimes you have to admit you are not sure. The only way to become less unsure about this would be to go out and get better data that might lead to a more informative estimate with a smaller standard error.

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[17] You can see this quickly by noting that an estimate will be statistically significant in its difference from zero at the 5 percent significance level if the coefficient is at least twice as large as the standard error (speaking approximately). When that is true, the 95 percent confidence interval won’t include zero.

In some other cases, we might be able to conclude that an estimate is economically unimportant whether or not it is statistically significant. Suppose both of the following two conditions are true at the same time: (1) the confidence interval is a tight band around a very small number, and only includes economically unimportant effects; and (2) the confidence interval does *not* include zero. In that case, we would call the estimate statistically significant in its difference from zero, but we would still conclude that the effect is economically unimportant (or "small"). Alternatively, if condition (1) above were true, but condition (2) were not (i.e., the confidence interval *does* include zero), we would call the estimate statistically insignificant, and we could also conclude that the effect was economically insignificant as well.

The definition of "economic significance," i.e., whether the effect is big or small, depends on the context. You need to consider things like how the size of the effect compares with the mean value of, and typical amount of variation in, the outcome variable, whether a one unit change in the explanatory variable is a big change or a small change, etc., like we did in interpreting the estimated size of the effect of *initial education* on *growth* three paragraphs ago.

The important things to remember are that whether an estimate is statistically significant or insignificant, by itself, does not tell us whether an effect is important, or whether an effect is precisely zero. Rather, it just tells us something about how the degree of uncertainty in our estimate arising from sampling error relates to the size of the estimated coefficient.

Another common way to misinterpret statistical significance is to conclude that if you have a statistically significant estimate, then you have a good answer to the question you've asked. That may or may not be true. If you have a statistically significant estimate, you might still have a very misleading answer to the question you are interested in, for example because of the omitted variable bias, bad control, and/or reverse causality problems discussed above. Standard errors only quantify the uncertainty arising from sampling error. Standard errors don't tell you anything at all about whether you have any of the other problems that can make regression estimates misleading. To figure out whether you might have those other problems, you need to *think*, not just apply mechanical rules about statistical significance. A narrow confidence interval merely makes us confident that our estimate is probably close to the population parameter. But the population parameter could be a biased indicator of the causal effect we're really interested in. So there is no substitute for thinking critically.

How to Interpret a Table of Regression Coefficients and Standard Errors

When economics journal articles present evidence from regression analysis, they tend to present it in a table that looks roughly like table 2 below. Typically, estimates from different regression equations will be presented in different columns, and each column will report estimated coefficients on each variable in different rows, with estimated standard errors presented in parentheses under each coefficient. Table 2 follows these conventions.[18]

In table 2, column (1) displays estimates from the regression of *growth* on *initial education* from figure 1, column (2) displays estimates from the regression of *growth* on *initial income* from figure 3, and column (3) displays estimates from our multiple regression of *growth* on *initial education* and *initial income* from equation (5). Column (4) displays estimates from a regression like that in column (3), except that *landlocked*, *frost area*, and *ethnolinguistic diversity* are added as additional control variables.

Remember that each estimated coefficient represents the change in the dependent variable associated with a one unit increase in that particular explanatory variable, holding the other control variables constant. So to be able to interpret what each coefficient means, you need to pay careful attention to the units of measurement for the dependent variable and for each explanatory variable – this was discussed in connection with table 1 above, so you might want to go back and review that now.

Focusing first on column (3), we see that the coefficient on *initial education* is 0.49 and the standard error on that estimate is 0.10. That coefficient means that one additional year of *initial education* is associated with an annual growth rate in real GDP per worker that is 0.49 percentage points higher, holding *initial income* constant. The 95 percent confidence interval around our estimate of the effect of *initial education* on *growth*

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[18] Sometimes a regression table will instead report "t-statistics" in parentheses below the coefficients. A t-statistic is just the coefficient divided by the standard error. As a rough rule of thumb, a t-statistic greater than approximately 2 is indicative of an estimate that is statistically significant in its difference from zero at the 5 percent level.

ranges approximately from 0.29 to 0.69, that is, the estimated coefficient of 0.49 plus or minus approximately 2 times the standard error of 0.10. Since the confidence interval does not include zero, we can say that the estimated effect is statistically significant in its difference from zero at the 5 percent level. All of the effects within the 95 percent confidence interval are arguably economically significant as well (see earlier in the paper for some ways help you think about whether any particular coefficient estimate here is economically important). The coefficient on *initial income* in column (3) is -4.15 with a standard error of 1.09. The coefficient means that a one unit increase in *initial income* (that is, an increase from zero to the level of per capita GDP of the U.S. in 1960) is associated with an annual growth rate of real GDP per worker that is 4.15 percentage points lower, holding *initial education* constant. This estimated coefficient is highly statistically significant in its difference from zero (as a rough rule of thumb, if the coefficient is more than about two times as large as the standard error, it is statistically significant at the 5 percent level).

Table 2 -- Regression estimates of the effects of education and other variables on the average annual percentage growth rate in real GDP per worker, 1960-2000

	(1)	(2)	(3)	(4)
intercept	1.00	1.51	0.94	0.96
	(0.27)	(0.24)	(0.25)	(0.32)
initial education (*E*)	0.16		0.49	0.41
	(0.06)		(0.10)	(0.09)
initial income (*Y*)		0.31	-4.15	-5.74
		(0.64)	(1.09)	(0.99)
landlocked (*L*)				-0.65
				(0.35)
frost area (*F*)				1.97
				(0.39)
ethnolinguistic diversity (*D*)				0.04
				(0.45)
Number of countries	84	84	84	81

Standard errors are in parentheses.
Source: author's regressions based on data from Bosworth and Collins (2003).

Column (4) shows estimates from a regression which includes three additional control variables (*landlocked*, *frost area*, and *ethnolinguistic diversity*), which helps illustrate some additional points about omitted variable bias and statistical versus economic significance. First of all, including the additional control variables causes the coefficient on *initial education* to fall from 0.49 to 0.41. The standard error for the coefficient on *initial education* is 0.09, which is much less than half the value of the coefficient, so the estimated effect of education is still highly statistically significant in its difference from zero. The fact that the coefficient changed suggests that one or more of the three variables that we previously omitted and have now included are correlated with *initial education* and affect *growth* (otherwise, the coefficient on initial education would not have changed). It is only relatively weak evidence of this, however, as the change in the coefficient is small relative to the standard error. The coefficient on *initial income* changes more substantially, from -4.15 to -5.74, suggesting that omitted variable bias was having a more important effect on this particular coefficient in column (3).

In column (4) of table 2, the coefficient on *landlocked* is -0.65 with a standard error of 0.35. Remember that *landlocked* is a dummy variable, meaning that it can only take on two possible variables, zero (corresponding to *not* landlocked) and one (corresponding to landlocked), so a one unit increase in *landlocked* means changing from not being landlocked to being landlocked. The coefficient suggests that, holding the other explanatory variables constant, landlocked countries grew about 0.65 percentage points less per year than did countries with access to the sea, on average. This estimate is statistically significant in its difference from zero at the 10 percent level of significance, but not at the 5 percent level of significance (the 90 percent confidence interval ranges roughly from -1.23 to -0.07, whereas the 95 percent confidence interval ranges roughly from -1.35 to 0.05). *Frost area* is estimated to have a large positive effect on growth. The coefficient of 1.97 on *frost area* suggests that, holding the other explanatory variables constant, a country where 100 percent of the land area experiences at least 5 days of frost per month in the winter grows 1.97 percentage points per year faster than a country where none of the land area experiences at least 5 days of frost per month in the winter, on average. The standard error of 0.37 is much less than half of the coefficient on 1.97, so the estimate is highly statistically significant in its difference from zero.

The coefficient on *ethnolinguistic diversity* in column (4) of table 2 is 0.04, with a standard error of 0.45. The 95 percent confidence interval around this estimate ranges roughly from -0.86 to 0.94, which includes zero, so the estimate is clearly not statistically significant in its difference from zero. To help us think about whether that confidence interval includes any economically significant effects, we need to think about the context. Recall that *ethnolinguistic diversity* represents the probability that two randomly selected people from a country are from different ethnolinguistic groups, and it ranges from 0 to 1. The minimum value is approximately 0 (in South Korea) and the maximum value is approximately 0.93 (in Tanzania), and the mean value is 0.39. So a change from zero to one in the *ethnolinguistic diversity* index variable is a very big difference in the degree of ethnolinguistic diversity, but is not that far from the difference between South Korea and Tanzania. Also recall that the mean annual *growth* rate is 1.6. Thus point estimate of 0.04 suggests a relatively small effect of *ethnolinguistic diversity* on *growth*, but the confidence interval suggests that differences in *ethnolinguistic diversity* that are well within the range of variation we see in the data *could* be associated with very *large* changes in the annual growth rate. Given the degree of uncertainty arising from sampling error, we can't be sure. For instance, the confidence interval for the effect on *growth* associated with a change in *ethnolinguistic diversity* of 0.5 would be half as wide as for a 1 unit change, thus ranging from -0.43 to 0.45, and this confidence interval includes fairly important effects on economic growth. Thus, I would categorize this as a case where we cannot be sure whether *ethnolinguistic diversity* has large positive, small positive, zero, small negative, or large negative effects on economic growth. The confidence interval allows for all these possibilities. We would need better data to answer the question of how *ethnolinguistic diversity* influences *growth*, as our regression does not provide decisive evidence one way or the other. If, by contrast, the standard error had been only, say, 0.03, and the coefficient on *ethnolinguistic diversity* had still been 0.04, then the 95 percent confidence interval would have included only relatively small effects on *growth*, and we could have concluded that any effect of *ethnolinguistic diversity* on growth is likely to be small. Either way, we should maintain a healthy degree of skepticism and not be *too* confident in our estimates, given all the other ways that a regression analysis can go wrong, such as omitted variable bias, reverse causality, and bad control, among other things.

Conclusion

There is of course plenty more to learn about regression analysis and econometrics. Further study can give you a deeper, more mathematical understanding of the issues discussed here, including demonstrations for why some of the claims made here are true, and it can teach you about various clever strategies for addressing some of the challenges and problems raised above. My hope is that this paper has made the basic idea of regression analysis reasonably transparent, that it has equipped you to read, understand, interpret, and think critically about papers involving multiple regression analyses, and that it has inspired you to want to learn more.

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