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# The Effects of Education on Labor Market Outcomes

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## Introduction

Human capital represents the most valuable asset held by most individuals. Individuals accumulate human capital through investments in schooling and training. Governments subsidize many forms of human capital accumulation including primary, secondary and post-secondary schooling. This chapter surveys the literature on the labor market effects of human capital with a particular focus on higher education.

The seminal book by Becker (1962, p. 9) defines human capital as "activities that influence future real income though the imbedding of resources in people" Thus, skills as varied as basic literacy, helpfulness, familiarity with Ford Motor Company's parts manual, and expertise in cardiac bypass surgery all represent forms of human capital. Individuals acquire these skills in diverse settings, and over different periods of time, at different costs to themselves and society. These different aspects of human capital also imply different financial and non-financial rewards.

Individuals, firms, and governments make decisions about investments in human capital by focusing on the costs and benefits of particular investments "at the margin", that is, relative to the existing stock of human capital. Individuals care about their private costs, which include tuition, foregone earnings, and the effort cost of learning. These costs differ among people because they depend on individual ability, preferences, and access to financial resources. Individuals balance these private costs against the private benefits, which include not only higher wages, but also the non-pecuniary aspects of particular jobs, such as opportunities to do help others or to work outdoors, social opportunities, and effects on health. Firms, many of which provide vocational training, weigh the costs of training against the benefits of increased worker productivity. Firms, too, may face unique costs and benefits that depend on their financial situations, level of technology, and business outlook. Governments consider the private benefits to citizens and the public benefits from higher tax revenue, greater civic participation, lower crime, and so on.

Surveying the vast literature on the labor market effects of education presents a daunting task to both reader and writer. Our chapter builds on the surveys of Rosen (1977), Willis (1986), Card (1999), Ashenfelter, Harmon and Oosterbeek (1999), Harmon, Oosterbeek and Walker (2003), and Heckman, Lochner and Todd [hereafter HLT] (2006). This chapter differs from these earlier efforts on several dimensions: we assume a less technical and more policy-oriented audience, focus on evidence for the United States and devote most of our attention to higher education. Our central theme of variability among persons in the effects of education due both to the variety of available educational experiences and to individual variability in responses to common experiences draws inspiration from Card (1999) and HLT (2006). Drawing on the recent literature in economics and statistics, we call this variability the "heterogeneous effects" of education.

Our chapter begins by considering estimates of the labor market effects of years of schooling. This topic dominates the literature though we argue against the usefulness of many such estimates for all but the very broadest policy questions. We then consider how the labor market effects of schooling vary with the characteristics of the student and the school, such as college quality and program of study. We end with a discussion of heterogeneity in the labor market effects of education more generally, and consider its subtle but important implications for the interpretation of the empirical findings in the literature. Table 1 summarizes key papers selected to illustrate particular points and highlight the range of estimates in the literature.

## Years of Schooling

### The Mincer Model

Nearly all modern empirical work on the labor effects of education builds on the classic model of Jacob Mincer (1958, 1974) and Becker (1964). Using this model, researchers estimate the association between years of schooling and labor market outcomes such as wages and earnings using data on individuals. The model controls for years of experience (or age) in order to account for the fact that individuals who complete more schooling typically enter the labor market full time at a later age. Despite its simplicity, the Mincer model has multiple theoretical justifications; HLT (2006) lay out the various models.

Formally, the basic model has

$$\ln(Y_i) = \beta_0 + \beta_S S_i + \beta_1 E_i + \beta_2 E_i^2 + \varepsilon_i, \quad (1)$$

where $Y_i$ denotes earnings or wages of individual "*i*", $S_i$ denotes years of schooling, $E_i$ denotes years of experience and $\varepsilon_i$ denotes the "error term", which embodies the effects of all of the determinants of wages or earnings besides schooling and experience. In words, the equation states that the natural logarithm of annual earnings or of the hourly wage depends linearly on years of schooling controlling for experience and experience squared.

The coefficient of interest in the Mincer equation is $\beta_S$. The use of natural logarithms allows the interpretation of $\beta_S$ as (roughly) the percentage effect of an additional year of schooling. Under certain strong assumptions detailed in e.g. HLT (2006), including no direct costs (i.e. tuition) of education, this coefficient represents a private internal rate of return to schooling. Because of the implausibility of the assumptions underlying this interpretation we avoid the terms "rate of return" and "returns to schooling" in this chapter.

The Mincer model assumes the same effect of schooling for all individuals. For this reason, $\beta_S$ has no "*i*" subscript. Under this assumption individuals choose to complete different amounts of schooling for reasons other than differences in expected outcomes. For example, one person might choose to complete college while another stops at high school because of differences in money costs (e.g. they win different scholarships), differences in non-pecuniary costs and benefits (some individuals like reading, others do not), differences in other aspects of the pecuniary benefits such as the variance in earnings (risk averse persons will prefer schooling choices that imply a lower outcome variance) or differences in discount rates (some individuals weight the future more highly in making decisions). We return to the important implications of this assumption below.

The Mincer model lacks a causal interpretation because of the implicit assumption that individuals make choices about how much schooling to complete in ways unrelated to all of the other factors, such as ability, family background, family income, motivation, and non-cognitive skills not controlled for in the model. The early literature focused on "ability bias" but the same point applies to other factors affecting labor market outcomes both directly and indirectly via effects on schooling. Suppose that ability affects schooling choices, perhaps because more able individuals find school easier. In addition to this indirect effect operating via the schooling choice, suppose that ability also directly affects labor market outcomes because more able individuals produce more than less able individuals, even with the same years of schooling. As a result, the standard method of estimating the Mincer equation, called Ordinary Least Squares (OLS) regression, produces biased estimates of the causal effect of schooling, because the estimated coefficient on schooling reflects both its causal effect and the selection of more able individuals into higher levels of schooling.

The lack of a causal interpretation means that estimates of equation (1), though ubiquitous in the literature, do not provide a sound guide to policy. In particular, they do not provide a reliable guide to the effects of increases in years of schooling induced by policies such as dropout prevention programs or more generous grants to college students. Nor do they reliably estimate the effects of the existing stock of years of schooling. Despite these shortcomings, we discuss the Mincer model here because of its place in the literature, because it provides a useful introduction to the problem of estimating the labor market effects of education and because it provides a handy summary measure of the association between labor market outcomes and years of schooling that allows comparisons across groups, locations and time periods.

In that spirit, the first row of Table 1 presents estimates of equation (1) from HLT (1996) using log annual earnings from 1990 Census data and dropping individuals with zero earnings. On average, in that year the earnings of white men increased by about 13 percent (0.1292) for each year of schooling while the earnings of black men increased by over 15 percent (0.1524). These estimates comport with the large difference in mean earnings observed in recent years between high school and college graduates. Both estimates exceed the corresponding values for the 1950-1980 censuses and reflect the large increase in the labor market value of schooling documented in e.g. Autor and Katz (1999). Harmon et al. (2003) present estimates (for wages) for many countries using a common data source. See also Psacharopoulos and Patrinos (2004).

## Causal Effects

In the last couple of decades researchers have sought to obtain more credible estimates of the causal effect of schooling. Put differently, they have tried to go beyond the non-causal association represented by estimates of equation (1) to obtain estimates that correspond to what individuals would actually experience if they chose to undertake additional years of schooling. As noted in the preceding section, the fundamental problem associated with estimating causal effects springs from the fact that individuals do not choose their schooling level at random. If they did so, a simple tabulation of mean earnings levels for each possible number of years of schooling would yield unbiased estimates of the causal effect of schooling on earnings. In reality, individual schooling choices depend on many different factors, only some of which researchers typically observe.

**Selection on observables**

Two basic strategies exist for dealing with the non-random selection into different levels of schooling. Heckman and Robb (1985) call these strategies "selection on observables" and "selection on unobservables". These two basic strategies characterize all of the empirical literatures surveyed below.

Under selection on observables, researchers attempt to control for all of the factors affecting outcomes both directly and indirectly through schooling. The idea is to compare two individuals with similar values of background variables but different levels of schooling. Ideally, the researcher would like to measure and condition on any characteristic whose influence on labor market outcomes might get confused with that of schooling due to non-random sorting into different schooling levels. Statistical methods such as multiple regression hold such background factors constant, which allows estimation of plausibly causal effects of schooling.

Ability, typically measured by test scores, constitutes one potentially important background variable. Other commonly used background variables include the parental age, education and occupation, the presence of both parents in the home, family income, number of siblings and early health outcomes such as birthweight. For analyses that examine aspects of college attendance, variables measuring educational quality and performance through high school also play a useful role.

Table 1 presents estimates from Kane and Rouse (1995). They analyze the wage and earnings effects of years (measured in college credits) spent in two-year and four-year colleges. Their preferred specifications, which control for a rich set of variables including measures of ability and family background, indicate that a year at a two-year college increases average earnings for men by about 3.5 percent (0.035) while a year at a four-year college increases average earnings by about 5.6 percent. These estimates, and the modestly larger estimates for women, lie well below the simple Mincer estimates from HLT (2006). In general, controlling for observable characteristics substantially reduces the estimated effect of schooling; see e.g. Table 5 of Card (1999). His estimates, as well as those in Table 1 and others like them in the literature, make it clear that the causal effect of education is substantially smaller than the estimates from simple Mincer equations suggest.

**Selection on unobservables**

Controlling for the available observable characteristics often does not suffice to yield a plausibly causal estimate of schooling effects due to the absence of important factors that affect labor market outcomes both directly and indirectly via schooling. In such cases, researchers must rely on alternative strategies that deal with selection on unobservables. The main alternative consists of something that economists have given the unintuitive name of "instrumental variables" or IV for short.

Despite the obtuse name, IV estimation embodies a simple idea. Find a variable that affects labor market outcomes but only indirectly through its effect on years of schooling, not directly. The ultimate IV is random assignment: the random number that determines assignment to the treatment or control groups in an experiment affects outcomes only through its effect on treatment assignment. Instruments typically arise from institutions, such as state-level variation in the age of legal dropout from school or from costs, such as state-level variation in tuition at public colleges or individual variation in distance to the nearest college. The instruments employed in the schooling literature aim to mimic random assignment via naturally occurring variation.

To see how instruments work, consider a very simple example. Imagine two adjacent school districts and assume that the two districts do not differ in terms of the parents and students who live there, in the quality of their schools and so on. The only way they differ is that one district has a special program to prepare students for college and the other does not. Students from the program district, on average, complete more years of schooling (mainly in college) than students from the other district. Put somewhat formally, the program induces some exogenous variation in years of schooling.

Suppose that we collect data on earnings and years of schooling for a random sample of students from the two districts at age 30. In such data, much of the variation across individuals in schooling will result from factors such as ability and motivation that we have not measured. Thus, using the data to estimate equation (1) will not yield estimates of schooling effects we can plausibly call causal. However, we can use the variation in schooling resulting from the program to estimate a compelling causal effect. In particular, we can calculate the ratio of the difference in mean earnings in the two districts and the difference in the mean number of years of schooling. In our simple model, the difference in the district means of years of schooling results from the program. Moreover, the difference in mean earnings in the two districts also results solely from the program. Thus, dividing the difference in mean earnings by the difference in mean years of schooling yields an estimate of the casual effect of years of schooling that relies only on the variation induced by the program.

**Instrumental variable estimates**

Table 1 presents estimates from two representative studies that use IV methods to estimate causal effects of schooling. Card (1999) uses maternal education as an instrument. That is, his estimates assume that maternal education affects labor market outcomes solely through its indirect effect on years of schooling. Card finds that a year of schooling increases the average earnings of men by over 10 percent (0.106) and of women by 11 percent (0.110). Acemoglu and Angrist (2000) find a somewhat smaller effect using compulsory schooling laws as instruments. Card reviews many IV studies and concludes that ‘instrumental variable estimates of the return to schooling typically exceed the corresponding OLS estimates – often by 20 percent or more” (Card 2001, p. 1155). HLT (2006) also review the IV literature and criticize many of the instruments it relies on.

The general finding that estimates obtained using IV methods exceed those obtained by controlling for observable characteristics via OLS regression creates a puzzle. The usual selection story suggests that the OLS estimates should be too large rather than too small. For example, if motivation is not controlled for, but affects outcomes both directly and indirectly through schooling, we would expect an upward bias in the estimated effect of schooling because it combines the causal effect of schooling with the effect of its correlate, motivation. IV estimates should not suffer from this bias, and so, by this argument, should be smaller than the regression estimates. We have more to say about this puzzle later on.

**Wages or earnings?**

Whether to use wages or earnings as the dependent variable depends on the purpose of the analysis. In the simplest case in which everyone works every period for the same number of hours, it does not matter. When employment and hours choices vary among individuals, using earnings rather than wages captures additional effects of schooling on employment and hours worked. Using the logarithm of earnings raises the question of what to do with individuals with zero earnings (for whom the log is not defined). Using wages as the dependent variable allows estimation of the effect of education on worker productivity (what economists call the "marginal product") independent of its effect on hours worked and employment. Of course, examining wages means dealing with the sample selection bias that results from observing wages only for the non-random subset of individuals who work. Dropping observations with zero earnings or no observed wage can lead to misleading inferences about the effects of education. For examples in the context of black/white differentials, see Heckman, Lyons and Todd (2000), Chandra (2003) and Neal (2005).

**Synthetic cohorts**

Many analyses use data on individuals of different ages at a point in time (a "cross section") to estimate the causal effect of schooling. Such analyses assume that the labor market outcomes of older individuals with a particular schooling level provide an unbiased estimate of what younger individuals with the same schooling level will experience when they get old. The literature calls this the "synthetic cohort" assumption.

While we can relax this assumption for older cohorts – see the evidence in HLT (2006) – for younger cohorts we do not yet observe their labor market outcomes at older ages.

If we want to use estimates of schooling effects to guide (or study) the schooling choices of young people or the selection of policies that affect those choices, then this assumption matters. Individuals making decisions about the quality, quantity and type of schooling to undertake look not only at older cohorts, they also attempt to forecast future demand and supply for different skills. Students making such decisions can consult many forecasts, ranging from the sobriety of the Bureau of Labor Statistics, as in Hecker (2005), to the near hysteria of Carnavale and Fry (2001). They can also observe the size of their cohort and attempt to forecast how it will affect the value of schooling, as studied in e.g. Card and Lemieux (2001) and Welch (1979). Readers looking to project out the lifetime effects of schooling for current cohorts of young people should keep in mind future changes in the demand and supply of skills due to technology and other factors.

**Other issues**

A variety of other issues arise when estimating causal effects of schooling; we mention two here. First, including variables such as experience as controls changes the meaning of estimated schooling effects. Whalley (2006) finds that schooling has a causal effect on experience because individuals with more schooling spend more time in the labor force and less time unemployed. As a result, controlling for experience biases the effect of schooling downward. Moreover, the effect of schooling controlling for experience does not have the interpretation of a “net” effect unless the analysis corrects for non-random selection into both schooling and experience levels. Following a literature that starts with Rosenbaum (1984), we suggest that readers not rely on such estimates unless they have strong reasons for controlling for variables affected by the education whose effects they seek to understand.

Second, the literature shows that survey measures of years of schooling and degree receipt typically include measurement error. Measurement error unrelated to the true value of the variable, called "classical" measurement error, leads to bias toward zero ("attenuation") in the estimated effects, but can be dealt with using IV methods. But many measures of education are binary, such as degree receipt, or categorical, such as years of schooling. With such variables the measurement error necessarily correlates with the true value due to limits on the number of possible values and the simple intuitions (and statistical solutions) associated with random measurement error do not apply.

Empirically, Kane, Rouse and Staiger (1999) find strong evidence of non-random measurement error, particularly in regard to years of schooling between high school and college completion. Black, Sanders and Taylor (2003a) find evidence of substantial over-reporting of college and professional degrees. This evidence suggests a preference for analyses that rely on transcript data to measure schooling or on carefully verified survey measures.

## Other Effects of Schooling

### Non-pecuniary outcomes

A small literature examines the effects of education on outcomes other than earnings and wages. We highlight three examples here. First, education increases fringe benefits. For example, the incidence of employer provided health insurance increases with education in the US (see http://www.umich.edu/~eriu/fastfacts/cps2005_2.html). Second, education improves health. Lleras-Muney (2005) provides evidence of a causal effect of education on health using the variation induced by state compulsory schooling laws (as well as citations to the literature on other health outcomes). Third, getting more education means, on average, getting a more educated spouse, through what economists call "positive assortative mating"; see Becker (2005) or McCrary and Royer (2006). In short, education improves many outcomes beyond wages and earnings. As such, analyses of the effects of education that focus solely on wages or earnings generally understate the full gains from additional schooling.

**Public benefits**

Many authors have argued for the existence of positive external effects of education. Thomas Jefferson wrote to James Madison in 1787, "Above all things I hope the education of the common people will be attended to, convinced that on their good sense we may rely with the most security for the preservation of a due degree of liberty." Such externalities play an important role in social cost-benefit calculations for policies that increase schooling. Indeed, much of the rationale for public funding (and to a lesser extent public provision) relies on such externalities. Bowen and Bok (1998), Rizzo (2004) and Courant, McPherson and Resch (2006) offer related discussions.

Solid evidence for the existence of externalities remains scarce and their extent at the post-secondary level remains controversial. Table 1 summarizes two of the more convincing studies. Lochner and Moretti (2004) find that each additional year of schooling reduces the probability of committing a serious crime, particularly for whites. Milligan, et al. (2004) presents evidence that completing high school increases voting. Dee (2004) reaches a similar conclusion. Overall, while the literature remains sparse (and thus provides fertile ground for future research), we think that the full social effect of schooling likely exceeds its effect on individual earnings by a modest amount.

**Spillover effects on wages**

A small recent literature empirically evaluates the claim that workers with high levels of schooling increase the productivity of workers with lower levels of schooling via spillover effects. Moretti (2004a) examines spillovers at the city level from the number of residents with a college degree. He finds that individuals with a high school education or less have higher wages in cities with more college educated workers. Moretti (2004b) finds higher productivity growth in firms located in cities with a more rapidly growing proportion of college graduates. In contrast, Acemoglu and Angrist (2000) examine spillovers at the state level using the variation induced by compulsory schooling laws and find very little.

Ciccone and Peri (2005) discuss econometric problems with the approach used in these papers. Their alternative approach, which avoids these problems, yields “no evidence of significant … externalities” in U.S. Census data. Table 1 summarizes the key papers. Besides the differences in methods, two complications that arise in comparing the studies concern the different geographic units (cities versus states) and the different ways in which they measure schooling, with Moretti (2004a) analyzing spillovers from college educated workers and the other papers analyzing spillovers from the stock of schooling. Overall, we think that Ciccone and Peri's (2005) methodological critique and evidence suggest the absence of substantial spillovers.

## Heterogeneous Effects

### Subgroups

The simplest form of heterogeneity consists of differences in average effects across groups defined by characteristics such as race or sex. Many of the studies summarized in Table 1 present separate estimates for men and women and/or blacks and whites. The evidence from those studies (and the broader literature) suggests that education often has surprisingly different effects for such groups. Thus, we encourage the reporting of subgroup estimates, further investigation into the causes of subgroup differences, and reliance on such estimates when considering the effects of policies likely to differentially affect particular subgroups.

### Sheepskin effects

Many analyses assume a constant (in logs) effect of each year of schooling. Different effects of particular years of schooling might result from diminishing returns, in which each additional year of schooling has a smaller effect, or from "sheepskin" affects associated with degree receipt. Sheepskin effects raise the issue of signaling. In this view, first introduced by Spence (1974), part of the value of a high school diploma or college degree results from the signal it provides about the recipient's ability or persistence above and beyond the value of the skills provided by the schooling underlying the degree. We focus here on the empirical evidence on sheepskin effects rather than entering the debate about signaling.

An early review by Layard and Psacharopoulous (1974) found no evidence of sheepskin effects. More recent work by Hungerford and Solon (1987), Kane and Rouse (1995), Jaeger and Page (1996), Heckman, et al. (1996), Card (1999) and HLT (2006) finds evidence of substantively important sheepskin effects, typically at both high school and college completion. The point estimates (and the importance attached to them by the authors) vary across these studies. Whether or not the analysis controls for ability and family background variables and whether degree completion is measured by number of years of schooling or via a separate survey question account for some of the variation.

Table 1 provides estimates from two papers of the additional effect of high school completion relative to the effect of other years of schooling. Hungerford and Solon (1987) treat high school completion as synonymous with 12 years of schooling while Jaeger and Page (1986) directly measure receipt of a diploma. The larger estimated effect in the latter paper may result from the more recent time period examined or from the reduction in measurement error associated with using actual degree receipt. Both papers estimate simple Mincer specifications so neither carries a causal interpretation.

The evidence points to (at least) modest sheepskin effects. As such it makes sense to allow the effects to vary by year when estimating the effects of years of schooling. Policymakers should keep in mind that, for example, policies that increase college degree attainment may have larger benefits than policies that increase the number of individuals with “some college”.

**College quality**

Educational programs at all levels differ in the quality of the schooling they offer, where we can define quality either in terms of inputs such as expenditures per student, teacher quality (e.g. ability and experience) and peer quality or directly in terms of labor market effects. We focus here on the literature on college quality, which provides an interesting contrast to that on primary and secondary school quality. While evidence of quality effects often seems elusive at the primary and secondary levels it leaps out of almost every study at the college level.

Key issues in this literature include how to measure college quality and how to deal with the non-random selection of students into colleges of different quality. In regard to the first issue, most studies use a single variable to proxy for quality, typically the average SAT score of the entering class or some measure of selectivity such as that provided by Barron's magazine. Black, Daniel and Smith (2005) argue that estimating the separate effects of quality measures such as selectivity and expenditures per student proves difficult in practice because the high correlation among the quality measures leaves little variation in one conditional on the other.

Using a single variable as a proxy for quality (a latent concept that we cannot directly observe) may mean measuring quality with substantial error. As a result, the estimated effect of the single proxy may provide a biased guide to the effect of quality defined more broadly. Black and Smith (2005) demonstrate that combining multiple quality measures reduces the measurement error implicit in using a single measure; they estimate that using a single measure understates the quality effect by about 20 percent.

Their preferred estimate, shown in Table 1, indicates that moving one standard deviation up the quality distribution increases wages by over four percent.

In regard to the second issue, the literature features both of the broad strategies discussed above. Black, Daniel and Smith (2005) provide estimates that control for a rich set of background characteristics including multiple dimensions of ability in a linear regression context while Black and Smith (2004) use the same data but apply matching methods. The latter paper also documents strong sorting by ability and family background into colleges of different qualities; see Reynolds (2007) for a similar analysis that includes two-year colleges. In contrast, Brewer, Eide and Ehrenberg (1999) use variables related to the price of college as instruments while Behrman, Rosenzweig and Taubman (1996) use twins to deal with the selection problem. All of these papers find substantial labor market effects of college quality.

The most influential paper in the recent literature, Dale and Krueger (2002), both controls for a rich set of background variables and compares students admitted to roughly the same set of colleges but who make different choices within that set. The fact that college admissions officers observe otherwise unobservable information about the student that gets incorporated into their admissions decisions motivates this strategy. As shown in Table 1, Dale and Krueger (2002) find little effect of college selectivity on wages for either of the data sets they examine, with the exception of a positive effect for low income students.

The literature has not yet reconciled the conflicting results but one possibility, suggested by Dale and Krueger (2003), concerns what it means to get admitted to two schools and then choose one or the other. Consider a student admitted to Princeton and

Michigan. Princeton ranks higher on most scales; why then might a student choose Michigan? Dale and Krueger's strategy requires that students choose in a way unrelated to the labor market outcomes associated with each school. If the deciding factor is, say, a social or family tie, then this condition should hold. If, in contrast, Michigan has a certain program that fits better in terms of interests and abilities, then the condition likely fails.

Overall, we think college quality matters. At the same time, the Dale and Krueger (2003) finding represents a provocative challenge to the literature, one that awaits resolution. The literature also has little to say about the mechanisms that underlie the influence of college quality on labor market outcomes. Such knowledge would have great value for policymakers.

**College major**

Educational programs differ in the type of human capital they seek to provide. Our discussion focuses on one important example of human capital type: college major. Variation in labor market outcomes across majors may arise from many factors, including differences in the amount of human capital provided (as opposed to consumption value while in school), institutional barriers that restrict access to certain majors and thereby raise wages in related occupations, differences in the average ability of students across majors, and differences in other characteristics valued by the labor market such as career focus, soft skills and so on. They may also represent what economists call "compensating" or "equalizing" differences associated with the non-pecuniary aspects of the major itself and the occupations to which it leads. For example, we would expect pleasant jobs to pay less than unpleasant ones, and dangerous ones to pay more than safe ones, all else equal; see Rosen (1986) for a survey.

Black, et al. (2003b) examines how the wages of full time workers in the National Survey of College Graduates vary across 85 different majors. Controlling only for demographic variables (because their data lack measures of ability and family background) their results lack a causal interpretation but do provide valuable descriptive information. As shown (in part) in Table 1, they find that engineering students have the highest wages, followed by economics students, with generally lower wages for students majoring in education, business, pre-professional studies, social sciences, and the humanities. Moreover, they find support for both ability differences in sorting across majors (MBAs with economics degrees earn more than MBAs with undergraduate business degrees even though they presumably possess roughly the same skills) and for differences in market value added across majors (math and physics majors make less than engineers but likely have at least equal ability).

Arcidiacono (2004) directly examines ability sorting by major using data from the National Longitudinal Study of the Class of 1972. He finds large differences across major even when controlling for ability measured by test scores. As shown in Table 1, he finds much larger earnings effects of college attendance for those who major in the natural sciences or business than for those who major in education, with humanities and social science majors in between.

In other papers Koch (1972) and Grogger and Eide (1995) find some evidence that students respond to market signals regarding what to major in. However, the long term persistence of higher wages for certain fields of study such as engineering – see e.g.

James et al. (1989) for evidence from an earlier cohort – suggests more to these differences than just transitory changes in demand for particular skill sets. Overall, the literature suggests causal effects of college major that do not represent simply differences in unobserved characteristics, institutional constraints or compensating differences. In thinking about policy reforms to increase college attendance and completion, it matters what the marginal students affected by the policy choose to study. At the same time, this particular aspect of the economic effects of education warrants further study.

**Other types of human capital**

Much of the literature reads as if formal schooling from primary school through graduate school represents the only means of human capital accumulation. In fact, both other forms of formal education, such as courses provided by firms to their workers and by the government through programs such as the Workforce Investment Act, and on-the-job training of various degrees of formality add importantly to the total stock of human capital.

On-the-job training has attracted much attention from economists. However, difficulties in measuring both formal and informal on-the-job training due to firm's reluctance to share data, problems with survey measurement as documented in Barron, Berger and Black (1997a) and the lack of ability and other background measures (or of good instruments) in many data sets that do attempt to measure on-the-job training has hampered efforts to produce credible estimates of causal effects. Unlike years of schooling, courses on the job come in different lengths as well, meaning that analysts must deal with selection problems related to both incidence and duration.

Recent papers focusing primarily on formal training courses include Barron, Berger and Black (1989, 1997b), Blundell, Dearden and Meghir (1996) and Frazis and Loewenstein (2005). Most studies find that individuals with more formal schooling (and higher ability) tend to receive more on-the-job training; see e.g. Mincer (1989) or Carniero and Heckman (2003). This pattern has two important implications. First, it suggests that part of the effect of years of schooling in the studies reviewed above comes indirectly via increases in on-the-job training. Second, it suggests that studies of the effects of training on wages and earnings that do not control for ability overstate the effects of such training. Even studies like Frazis and Loewenstein (2005) that control for ability (as proxied by test scores) likely attribute differences in other unmeasured characteristics like motivation and social skills to on-the-job training. Table 1 presents estimates from two of the better studies on this topic; both show substantial effects of on-the-job training on wages.

Government funded training for the disadvantaged and for displaced workers includes activities such as classroom training in occupational skills (often provided by community colleges), basic skills upgrading (often aimed at GED receipt) as well as subsidized on-the-job training at private firms and lessons in how to look for work and how to hold on to a job. Smith and Whalley (2007) demonstrate that conventional survey measures tend to underestimate the extent of such activities, with the amount missed higher for activities that look less like formal schooling. Heckman, LaLonde and Smith (1999) survey the methodological literature on how to evaluate such programs and the empirical literature on their effects on earnings and employment. Kluve (2006) provides a meta-analysis of European programs.

The literature on publicly provided training dominates that on training at private firms in both size and average quality as a result of the availability of more and better data as well as the occasional use of random assignment. This asymmetry is not appropriate given the much greater importance of training at private firms to the overall stock of human capital. Table 1 highlights results from experimental evaluations of two public programs: the Job Training Partnership Act (JTPA) and the Job Corps. The former has moderate and persistent impacts on adults, particularly adult women, but essentially zero impacts on youth (an as yet unexplained pattern it shares with many similar programs). The latter has substantial positive impacts on earnings (and negative impacts on crime) in the short term, but the impacts fade out well before they outweigh the costs the program. The use of random assignment designs in both cases makes the estimates clear and compelling by removing concerns about non-random program participation.

The study of on-the-job training at firms would benefit greatly from the collection of better data. Both that literature and the literature on public programs would benefit from greater integration with the literature on formal schooling, with particular attention to patterns of lifelong learning.

## General heterogeneity

The recent literature goes beyond looking at how the impacts of education vary along observable dimensions of the student and the schooling and focuses on heterogeneity in the effects of schooling among individuals with the same observed characteristics undertaking the same schooling. Put differently, recent research considers the case where each individual has their own person-specific effect of any particular educational program. The econometric literature calls this the “correlated random coefficient model”; we call it simply “general heterogeneity”. Though it might seem a minor innovation at first blush, moving from thinking about the world in terms of everyone having the same effect of particular years or types of schooling (or having different effects that they do not know in advance and so do not act upon in making their schooling choices) has important implications.

First, rather than just one parameter of interest (or perhaps a small number of parameters of interest corresponding to variation in common coefficients based on individual characteristics or school characteristics), we now have many. We can think about the average schooling effect in the population, among particular subgroups defined by observable characteristics, or among groups defined by the amount or type of schooling completed. We can, most importantly for policy, think about the average effect of schooling among those on particular policy margins, such as the average effect of college for those induced to attend college by a new student loan or grant policy.

In a simple economic model in which potential college students have some idea of the effect of attending college on their labor market outcomes, we would expect that those who presently attend have larger effects of doing so than those who presently do not attend, including those at the margin who would change their mind about going to college in response to policies that reduced its cost. This simple model suggests, for example, that potential students on the margin of attending college will not realize the substantial (on average) labor market benefits from attending college received by those already attending. This is a very important point for policy and one that suggests the value of analyses designed specifically to estimate the effects of college attendance on individuals on the margin of attending.

Second, with general heterogeneity, instrumental variables estimates based on instruments related to program costs or institutions do not estimate the average effect of schooling but rather the average effect of schooling on those individuals whose schooling status depends on the value of the instrument. To see this, consider distance to college in a model of heterogeneous effects of college. Individuals who live near a college will attend even if they benefit relatively little because they face a low cost of attendance. In contrast, individuals who live far away from a college will attend only if they reap benefits high enough to overcome their higher cost. Using distance to college as an instrument then estimates the mean effect of college attendance on those at the margin, which means those who would attend if they lived near a college but would not attend if they lived far away. We expect these individuals to have a lower average benefit from college attendance than individuals who attend regardless of where they live. Of course, if the policy under consideration subsidizes the cost of living away from home while attending college, then the average effect for individuals on the margin of attending due to distance may represent exactly the effect of interest. See e.g. Imbens and Angrist (1994), Card (1999), Kling (2001) and HLT (2006) for more technical treatments of this important point.

Third, general heterogeneity suggests two possible solutions to the puzzle of IV estimates of the effect of schooling that exceed OLS estimates. First, Card (1999, 2001) and others have suggested that individuals whose years of schooling depend on the instrumental variables commonly used in the literature, such as the minimum school leaving age, may have larger effects of education at the margin than other individuals, perhaps because they lack access to the assets or credit required to finance such investments. Second, HLT (2006) suggest instead that in a world of comparative advantage, OLS estimates of schooling effects embody two conflicting biases: failure to control for all the variables that affect outcomes both directly and indirectly via schooling makes them too high and selection on comparative advantage (wherein each individual does what they do best) makes them too low. To see the latter effect, consider a simple example of selection on comparative advantage with no covariates: suppose that college graduates earn 20 but would have earned 10 as high school graduates. In contrast, high school graduates earn 15 but would have earned only 12 as college graduates. Comparing the earnings of college and high school graduates (the analog of running an OLS regression in the absence of covariates) suggests an effect of college completion of five (= 20 - 15), while completing college actually increases earnings by 10 (= 20 - 10) for those who do so (and decreases earnings for those who do not). In this case, the IV estimates exceed the OLS estimates because the latter have a net downward bias, rather than the net upward bias traditionally assumed. This view makes the observed pattern consistent with the simple economic notion that individuals who do not attend college would benefit less from doing so than those who do attend.

We sum up this section simply: the notion of general heterogeneity has very important implications for this literature. We find this model more plausible than the common coefficient model that dominated the literature until recently, but note that adopting this model makes life harder, or at least more subtle, for both researchers and policymakers.

## Concluding Remarks

Our survey omits a number of worthy topics including the sequential nature of schooling choices, in which information revealed at each step of the schooling process affects decisions regarding whether and how to continue on to the next step, the difference between ex ante and ex post effects (and the related issue of how much individuals know about how schooling will affect them) and the effect of school type (e.g. public versus private, or two-year versus four-year college).

We have focused instead on three main points. First, standard Mincer equation estimates of the effect of years of schooling that control only for experience (or age) have little relevance to policy because they lack a causal interpretation. They remain useful for comparisons of the association between labor market outcomes and schooling for different groups, different points in time and different locales. Second, we have emphasized heterogeneity in the effects of schooling that results from both differences in average effects across subgroups and differences in average effects for different levels, qualities and types of education. These forms of heterogeneity have implications for policy because particular policies encourage or discourage particular forms of schooling for particular subgroups. Finally, we have emphasized the importance of what we called general heterogeneity in the effects of education. When individuals make choices based on their person-specific effects of education, evidence-based policymaking requires careful attention to estimating average effects for precisely those individuals whose educational choices change in response to particular policy changes.

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| Citation | Data | Summary | Control variables | Parameter of interest | Estimates |
|---|---|---|---|---|---|
| *OLS Papers on Years of Schooling* | | | | | |
| Heckman et al. (2006) -- Table 2 | US Census (1930-1990, results from 1990 data for men only reported here, sample includes only men with non-zero earnings) | The authors estimate simple Mincer earnings equations using Census data for various years. | experience and experience squared | effect of a year of schooling on log annual earnings | 0.129*** (whites) 0.152*** (blacks) |

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| Kane and Rouse (1995) -- Table 2 | NLS-72, NLSY-79 (The sample is limited to people who were working and not self employed in 1986, had non-zero earnings in both 1984 and 1985, and had average annual earnings in 1984 and 1985 of at least $1000.) | The authors test whether or not a year's worth of credits at a two year college have the same effect on earnings as a year's worth of credits at a four year college by running OLS regressions with controls for test scores and family background. | race, parents' income, percentile rank in high school, NLS-72 test score, experience, experience squared, indicators for region and size of high school, and educational attainment after 1979; separate estimates for men and women | effect of a year's worth of credits at a two or four year college on log annual earnings | 0.035** (two year colleges, men)<br>0.056*** (four year colleges, men)<br>0.066*** (two year colleges, women)<br>0.086*** (four year colleges, women) |

*IV Papers on Years of Schooling*

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| Card (1999) -- Table 5 | General Social Survey (1974-1996; sample restricted to heads of households ages 24-61) | Card estimates the effect of years of schooling using mother's education as an instrument for own education. | cubic in age, race, survey year, region; separate estimates for men and women | effect of completing an additional year of schooling on log annual earnings | 0.106*** (men) 0.110*** (women) |
| Acemoglu and Angrist (2000) -- Table 6 | Census IPUMS (1950-1990; sample restricted to white men who were 40-49 in the Census year) | The authors use compulsory schooling laws as instruments for years of schooling | indicators for Census year, year of birth, and state of birth | effect of completing an additional year of schooling on log wages | 0.081*** |

*Non-pecuniary benefits*

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| Lochner and Morreti (2004) -- Table 12 | Census, NLSY-79, Uniform Crime Reports (results reported here use NLSY-79; sample restricted to men between 18 and 23 in 1980) | The authors use cumpulsory schooling laws as instruments in estimating the impact of years of schooling on subsequent criminal activity. | current enrollment, parents' highest grade completed, indicator for living with natural parents at age 14, indicator for the following variables: being the child of a teen mother, region, age/cohort, ability, SMSA status, local unemployment rate | effect of completing an additional year of schooling on the probability of committing a serious crime over a seven year period (using self reports of criminal activity) | -.022** (whites) 0.005 (blacks) |
| Milligan et al. (2004) -- Table 7 | National Elections Study, November CPS | Using variation generated by changes in compulsory schooling laws in both the United States and the United Kingdom, the authors estimate the effect of education on civic participation. The authors correct for misreporting of voting behavior. | survey year, quartic in age, indicators for year of birth, indicators for state | effect of graduating from high school on the probability of voting in general elections | 0.438*** (US) |

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| Lleras-Muney (2005) -- Table 4 | Census (1960-1970; sample restricted to white respondents who were 14 years old between 1915 and 1939) | The author uses changes in compulsory schooling laws between 1915 and 1939 as instruments for education. | indicator for Census year, gender, state of birth, cohort of birth, and interaction terms between region of birth and cohort | effect of completing an additional year of education on mortality between 1960 and 1970 | -0.051** |
| *Spillovers* | | | | | |
| Moretti (2004a) -- Table 5 | Census (1970-1990), NLSY-79 | The author uses the presence of land grant colleges as an instrument for the supply of college-educated workers in a given city. He then estimates the impact of an increased supply of college educated workers on wages in the city. | year effects, city effects, unemployment and other city controls, Katz and Murphy index of demand shifts | effect of an increase of 0.1 in the fraction of workers who are college graduates on the log hourly wages of other workers | 0.191** (less than HS degree) 0.167** (HS graduate) 0.124** (some college) 0.047 (college graduate) |

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| Ciccone and Peri (2006) -- Tables 4 and 5 | Census (1970-1990) | The authors separate the effect of externalities from the effect of a downward sloping demand curve for highly educated workers, both of which imply higher wages for workers with low levels of schooling in locations with a higher proportion of workers with high levels of schooling. The authors use the age and racial composition of cities, plus indicators for region, as instruments for the proportion of college educated workers. | log change in average experience, log change in average employment, indicators for region | effect of a one-year increase in average educational attainment within a city between 1970 and 1990 on the change in log of average wages in that city between 1970 and 1990 | -0.010 (all workers) -0.001 (white males only) |

*Sheepskin Effects*

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| Hungerford and Solon (1987) -- Table 1 | May 1978 CPS (sample restricted to white males age 25 to 64) | OLS estimates of Mincer's model with additional indicators for different levels of educational attainment to estimate non-linear effects of completing key grades. The data do not indicate degrees received, so the authors can estimate the effects of completing "diploma years" but do not know who actually receives a diploma. | none | effect of completing 12 or more years of schooling, above and beyond the linear effect of the number of years of schooling, on log hourly wages | 0.0375** |
| Jaeger and Page (1996) -- Table 2 | 1991 and 1992 March CPS (sample restricted to white men aged 25-64) | A change in the wording of the CPS question about educational attainment gives the authors information on both the number of years of schooling and the highest degree completed. They use this information to compute "sheepskin effects" for completing a degree, rather than completing a diploma year. | none | effect of receiving a high school diploma, controlling for years of education, on log hourly wages | 0.123** |

*College Quality*

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| Dale and Krueger (2002) -- Table 7 | College and Beyond, NLS-72 (results reported here use NLS-72) | The authors compare the future earnings of students admitted to the same set colleges who choose to attend different schools. They measure the quality of schools by a single parameter, the average SAT score of admitted students. | log predicted parental income, own SAT scores, indicators for race, indicator for top 10% in high school class, average SAT score of schools applied to, indicator for recruited athlete, indicators for one to four additional applications | effect of a 1/100th of a point increase in the school average SAT scores, relative to the SAT scores of other schools to which the respondent was admitted, on log annual earnings | 0.013 |
| Black and Smith (2005) -- Table 5 | NLSY-79 men | The authors use a composite measure of five indicators of college quality (faculty-student ratio, the rejection rate among those who applied for admission, the freshman retention rate, the mean SAT score of the entering class, and mean faculty salaries), instead of a single proxy for quality. | years of schooling, ASVAB scores, indicators for race, quartic in age, indicators for region of birth, home characterisistics, parental characteristics,and high school characteristics | effect of a one standard deviation increase in college quality on log hourly wages in 1988 | 0.043 |

*Program of study*

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| Black et al. (2003b) -- Table 3 | 1993 National Survey of College Graduates (sample restricted to full time workers aged 25 to 55) | The authors run OLS regressions of wages on indicators for particular college majors and other controls. To make comparisions between majors, one must be excluded as a reference category; the authors choose economics. | indicators for race/ethnicity and age; separate estimates for men and women (effects for men only reported here) | effect of majoring in a selected fields, relative to studying economics, on log hourly wages | -0.187*** (biology) -0.270*** (elementary education) -0.111*** (business administration) |
| Arcidiacono (2004) -- Table 8 | NLS-72 | Wage premiums for different majors could reflect sorting by ability into different majors. The author finds large ability differences between majors, and controls for this selection in his estimates of wage premiums. | coefficients reported are for a hypothetical respondent with SAT scores, grade point averages, college quality, and calendar year equal to the sample mean; separate estimates for men and women (effects for men only reported here) | effect of majoring in different fields on log annual earnings, relative to no college | 0.197 (natural sciences) 0.159 (business) 0.094 (social sciences/ humanities) -0.012 (education) |

*Firm Provided Job Training*

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| Barron et al. (1989) -- Table 2 | Survey of the Equal Opportunity Pilot Project (1982) | The authors run OLS regressions of log starting wages and growth in log wages on log hours of training and controls. | age, age squared, experience, experience squared, education, plus indicators for the following variables: vocational education, gender, firm unionization, temporary position, size of firm, occupation, and industry | effect of an increase in the log of hours of formal on-the-job training in the first three months of employment on the log of starting wages and wage growth over the first two years in the job | -0.003 (starting wage) 0.035** (wage growth) |
| Frazis and Loewenstein (2005) -- Table 4 | NLSY-79, Employer Opportunity Pilot Project (NLSY results reported here) | The authors focus on identifying the proper functional form for the effects of employer-provided job training. They determine that the best specification is log wages as a function of log hours of training, and estimate this via OLS. | age, AFQT score, number of missing training spells, indicators for the following variables: calendar year, black, Hispanic, female, union, managerial position in first year at firm, other white collar position in first year at firm, missing AFQT score, missing union variable, any ongoing training | total effect of the log of current hours of formal training and log of hours of formal training in the previous 1, 2, and 3 years, on log hourly wages, at the median hours of training | 1.37** |

*Public Job Training*

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| Bloom et al. (1997) -- Table 2 | National JTPA Study (results for adult men and adult women ages 22 and above only presented here) | Experimental evaluation of the Job Training Partnership Act (JTPA) program, at 16 sites, with random assignment from 1989-1991. JTPA provided classroom training in occupational skills, subsidized on-the-job training at private firms, job search assistance and other services to disadvantaged youth and adults. | Impact estimation via OLS regression controls for a variety of covariates, but these act only to increase the precision of the estimates, which are unbiased due to random assignment | Impact on earnings in the 30 months after random assignment. Because of many treatment group members dropped out and many control group members received similar services elsewhere, the estimate represents the incremental effect of JTPA services, above and beyond the services received by control group members. | $1,176*** (adult women) $978* (adult men) |

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| Schochet et al. (2003) -- Table III.1 | National Job Corps Study | Experimental evaluation of the Job Corps program, which provides an intensive (and expensive) residential training experience to disadvantaged youth. The sample consists of eligible applicants in 1994 and 1995. | Impact estimation via OLS regression controls for a variety of covariates, but these act only to increase the precision of the estimates, which are unbiased due to random assignment | effect of being randomly assigned to Job Corps in 1994 or 1995, on average calendar year earnings (in 1995 dollars) | -$176.8*** (1996 earnings) $171.8** (1997 earnings) $219.8** (1998 earnings) $32.9 (1999 earnings) -$17.4 (2000 earnings) $7.0 (2001 earnings) |