

# EDUCATION

## AND ECONOMIC MOBILITY

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### KEY FINDINGS:

- Increases in parent education lead to better educational outcomes of children, especially reducing the probability of very low achievement.
- Scholars disagree about whether a significant number of students fail to attend college due to limited family financial resources. Some argue there is no effect while others argue for modest impacts.
- Remedial programs such as the GED and job training for youths appear to have limited impact and so likely do not affect mobility. Job training for adults appears to have effects that may enhance both absolute and relative mobility within and across generations.
- Debate surrounds the effect of K-12 school quality improvements (e.g., reducing class size). A sizable majority of studies suggest school quality improvements raise earnings supporting absolute mobility. Moreover, many studies find the effects are greatest among children in low-income families, suggesting greater relative mobility as well (though on this point there is disagreement).
- Studies of early education (preschool) initiatives report large impacts, though the most effective programs are very intensive (and so expensive). Aside from the well-known Head Start program, most of these interventions are untested on a large scale.

*Education policy is important to the discussion of mobility because it serves both as an end and a means to an end in eliminating inequalities. In addition to fostering mobility among those directly benefited by it, the children of beneficiaries may indirectly benefit as well. Thus, properly targeted education programs may enhance outcomes in both present and future generations. By improving the outcomes of children in general, education investment increases absolute intergenerational mobility. And if, as a good number of studies suggest, children from low-income households are especially benefited, education investments may also raise relative intergenerational mobility. Intragenerational mobility, on the other hand, is less likely to be enhanced by these policies because most education is completed before the individual enters the workforce. GED and job training programs are exceptions to this general rule. Only insofar as some workers make these investments after a few years of work will mobility within a generation be affected.*



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This review summarizes economists' understanding of the connections between education and mobility. Because we have limited direct evidence of the impact of education on intergenerational mobility, this review also surveys the literature on the returns to education interventions. While this evidence speaks most directly to absolute mobility, relative mobility will be enhanced by programs that especially assist children in low-income families.

There are several points on which there is agreement, while healthy debate surrounds others:

- Increases in parent education lead to better educational outcomes of children, especially reducing the probability of very low achievement.
- Scholars disagree about whether a significant number of students fail to attend college due to limited family financial resources. Some argue there is no effect while others argue for modest impacts.
- Remedial programs such as the GED and job training for youths appear to have limited impact and so likely do not affect mobility. Job training for adults appears to have effects that may enhance both absolute and relative mobility within and across generations.
- Debate surrounds the effect of K-12 school quality improvements (e.g., reducing class size). A sizable majority of studies suggest school quality improvements raise earnings supporting absolute mobility. Moreover, many studies find the effects are greatest among children in low-income families, suggesting greater relative mobility as well (though on this point there is disagreement). While less developed, some recent studies suggest that school institutions also matter by increasing competition for student dollars.
- Studies of early education (preschool) initiatives report large impacts, though the most effective programs are very intensive (and so expensive). Aside from the well-known Head Start program, most of these interventions are untested on a large scale.

### **Literature Summary**

Research has repeatedly documented a strong *correlation* between parent and child education ( $\Delta = 0.35$ ). While **genetic transmission** from parents to their children can explain part of this intergenerational correlation, **recent research suggests that a portion of the connection is causal**. Scholars estimate that an additional year of father's education reduces the probability that a child is held back in school by 10 to 20 percent and an additional year of mother's education reduces the probability of low birth-weight by roughly 10 percent. Because both of these outcomes are especially relevant to the well-being of at-risk children, there is good reason to suspect that parent education fosters greater intergenerational mobility in both absolute and relative terms. Thus investments in education of the current generation may simultaneously increase mobility in this generation and the next.

Both state and federal governments provide substantial financial support to education. **Economic theory** suggests that such investment may simultaneously increase economic efficiency and reduce inequality by providing low-income families educational access. **Studies looking directly at intergenerational mobility** have found only limited evidence of distortions caused by financial constraints. While relatively few studies have looked at education programs' direct effects on intergenerational mobility, the huge number of **studies looking at college attendance patterns** can shed indirect evidence on the question. While some scholars conclude that family finances play a role in determining higher education investments, others explain correlations between family income and college attendance as artifacts of intergenerational correlations in ability. Scholars in the former group estimate that perhaps 5 percent to 10 percent of intergenerational earnings persistence is accounted for by differential access to credit.

Where higher education funding supports investments in high-ability children, GED and public job training programs address post-secondary skill deficiencies. [Research on these programs](#) finds that economic outcomes (earnings, unemployment, and job stability) for GED recipients fall well short of non-college-bound high school graduates and are indistinguishable from high school dropouts. To the extent that the GED is used by some as a credential for advanced training, it may have positive effects. Similarly, low-intensity job training programs for youth have shown little impact. Programs with extensive classroom time (equivalent to roughly a year of high school) produce positive effects. That said, even the positive effects are relatively modest: the cost-effectiveness of the programs rests on strong assumptions about the lasting effects of program participation—assumptions which are contradicted by empirical studies. Job training for adults appears to hold more promise.

Of course, post-secondary programs of all types depend heavily on effective primary and secondary education. Due to data limitations, few studies directly explore [the impact of K-12 school quality on intergenerational mobility](#) and what work has been completed is inconclusive. Fortunately, an extensive literature examining the [returns to class size reductions, increased teacher pay, and other school quality measures](#) informs the issue. While researchers have not reached a consensus, a large majority of studies report positive effects of school quality improvements. (For example, of studies which find statistically significant class size effects, 80 percent report a positive impact.) Moreover, with a few notable exceptions, evidence points to larger gains among at-risk students suggesting that school quality investments may enhance relative mobility. That said, even optimistic accounts of the benefits and costs find a return of around 2-to-1. Given the [sensitivity of cost-benefit estimates to small methodological changes](#), opponents continue to question the cost-efficiency of school quality improvements.

If early learning paves the way for later educational achievement, then interventions before age five may be the most potent. While less is known about what particular interventions work at the preschool level, there is stronger consensus that [the impacts of early childhood education](#) are substantial. First, studies consistently find a large, near-term increase in cognitive skills. (*Effect sizes* often run as high as 0.3.) However, as demonstrated in the extensive literature studying Head Start, these cognitive gains fade once the child leaves the program and enters school. Some early critics cited this “fade out” as evidence of program failure. Most economists now view this conclusion as premature. While test scores do not show large long-term improvement, participants are less likely to require special education or to be held back in school, less likely to drop out, less likely to be arrested in the teen years, and more likely to be employed as young adults. In total, the benefits of early childhood education appear to far exceed the costs (with estimated returns exceeding 8-to-1 in some demonstration projects). However, because much of the reported gain results from societal improvements (like lower arrest rates) and not higher participant earnings, it is not immediately clear that early childhood investments will necessarily alter intergenerational mobility more than investments at later points in the lifecycle.

The literature on education interventions points to two important issues. First, much of the improvement in student performance stems from gains in non-cognitive skill rather than cognitive ability. The recognition that “ability” is multidimensional has important implications for economic theory and related empirical work. Second, the intensity of an intervention likely matters. For instance, the most promising job training programs are the most intensive. Similarly, Head Start evaluations report considerable variation with less lasting impact on black children. Some researchers connect this variability in effectiveness to disparities in funding and administration across Head Start programs. The most successful programs also engage parents, helping them to create a more supportive environment for children as they progress through school. While such program intensity is not cheap (costs run from \$10,000 to \$20,000 per child in

demonstration programs), Grunewald and Rolnick (2006) lay out a [practical plan for funding intensive preschool](#) for all children in low-income families.

Finally, it cannot be emphasized enough that our understanding of the connection between education policy and mobility is conditional on current levels of funding. For example, if policy makers were to significantly expand preschool education, we would expect the skills gained by students in these programs to raise returns to K-12 schooling investments. If these higher returns motivated greater funding for K-12, the resulting increase in performance among high school graduates may lead to greater demand for college education with a commensurate increase in pressure on family finances. The inter-related nature of investments at different stages means that we must re-evaluate the various drivers of mobility as economic conditions and policy change.

## THE CAUSAL IMPACT OF PARENT EDUCATION ON CHILD EDUCATION

Because children share many characteristics with their parents, it is hardly surprising to find similar levels of educational attainment in both parent and child. Policy-minded researchers want to discern the degree to which this pattern represents a causal relationship: If a policy innovation raised the parents' level of education by one year, by how much would children's educations increase? And is this effect uniform across all levels of parent education?

Oreopoulos et al. (forthcoming) use U.S. Census data to study the effects of increased parent education resulting from compulsory schooling laws instituted between 1960 and 1980. They find that one additional year of education for either mother or father reduces the probability of grade retention anywhere from two to 17 percentage points. Black et al. (2005) study the effects of a similar change in compulsory schooling in Norway. While they find weak effects in general, they do find an impact for mothers with particularly low education. Chevalier (2004) similarly finds positive effects in Britain.

In the United States, Currie and Moretti (2003) study changes in parent education resulting from college openings in the area where the parent lived. They find that an increase in mother's education significantly improves birth outcomes for the child. One additional year of education decreases the probability of a low birth weight child by 10 percent and decreases the probability of a pre-term birth by 6 percent. **Low birth weight may in turn impact health, educational, and economic outcomes later in life.** Page (2006) considers the impact of the GI Bill on the World War II generation. (Some birth cohorts were substantially more likely to benefit from the Bill due to the timing of the war.) She finds that a one-year increase in father's education reduces the probability of the child retaining a grade by between two and four percentage points—a 10- to 20-percent reduction.

Other researchers attempt to account for common parent and child characteristics by using the education of the parent's sibling as a control. The effect of parent education on child education controlling for the aunt or uncle's education is then interpreted as the causal component. Using this method, Rosenzweig and Wolpin (1994) find mother's education increases test scores for children between the ages of five and eight. Behrman and Rosenzweig (2002) go one step further, controlling for genetic differences by using education of twin siblings of the parent as the control variable. Although they find no impact of the mother's education, they find a positive effect of father's education on child's years of schooling.

While none of these studies speak directly to intergenerational mobility, many results speak to outcomes relevant to children at the lower end of the achievement distribution. In particular, the findings suggest that policies such as increased compulsory schooling might address both absolute and relative intergenerational mobility by reducing particularly adverse child outcomes.

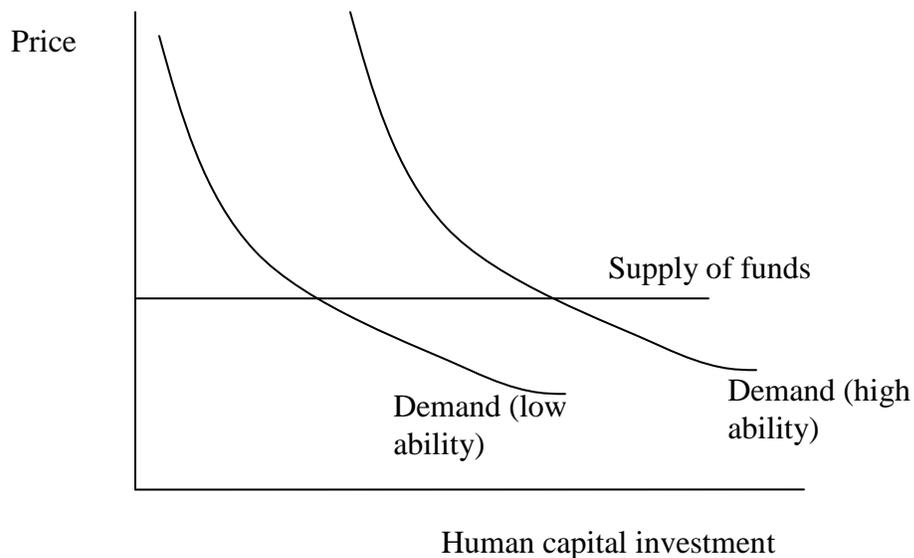
## HUMAN CAPITAL THEORY AND MOBILITY

Many aspects of human capital theory have some bearing on mobility, yet two models capture the key concepts in the literature: the model of intergenerational investment in Becker and Tomes (1986) and the critique of single-dimension models of ability found in Carneiro and Heckman (2002). This section briefly summarizes the key elements of these models and their contributions to the study of mobility.

Becker and Tomes present a model of altruistic parents whose utility depends on their own consumption and that of their children. Wages are modeled as a function of two factors, innate ability and acquired human capital (i.e., education) with assumed diminishing returns to educational investments. Based on empirical evidence, the authors also assume that the return to educational investments is higher for children with greater ability.

To begin, Becker and Tomes consider a family that is able to borrow against future earnings to fund investments in the child's education. In this case, the price of an additional dollar invested in the child's human capital is the opportunity cost of borrowing a dollar (with interest) from the child's adulthood. With access to credit, the relevant interest rate is the market rate. This cost is represented by the horizontal supply of funds curve in the figure below.

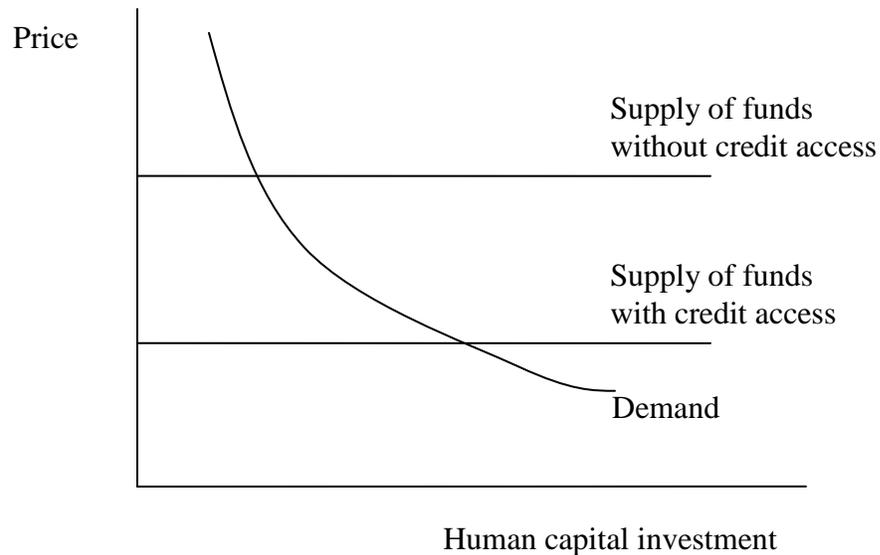
### The Supply and Demand for Educational Investments



Given the assumed properties of the wage function, the quantity of education demanded falls as price increases due to diminishing returns on that investment. Moreover, demand is higher for more able children due to the greater returns earned by such children. Parents continue to invest in education so long as the marginal return exceeds the cost (i.e., until the intersection of the relevant demand curve with the supply of funds curve). Notice that the level of human capital investment (and subsequent child earnings) is determined solely by the child's ability and, in particular, is not a function of the parents' income.

If parents cannot borrow to fund education expenditures, then families that would have borrowed to finance their child's education must now reduce their own current consumption to pay for schooling. Because this choice entails a higher opportunity cost, the supply of funds curve is higher for such families as noted in the figure below. As a result, less investment will be made in children with credit constrained parents than in children of like ability born to unconstrained parents. In an economy with binding credit constraints, child incomes are a function of both ability and parent income.

## The Effect of Credit Constraints on Educational Investments



The intergenerational implications of this theory appear once we add a model of ability transmission. Becker and Tomes assume that parent and child ability are correlated due to **cultural and/or genetic transmission**. As a result, even when all parents have access to credit, parent and child earnings are positively correlated. Credit constraints produce an additional, direct connection between parent and child earnings as parents with higher incomes face a lower opportunity cost of human capital investment. Thus, if we examined two identical economies, one with and one without credit constraints, the economy with extensive access to credit would possess greater intergenerational mobility. In the economy with poorly functioning capital markets, earnings of children would be more closely associated with those of their parents.

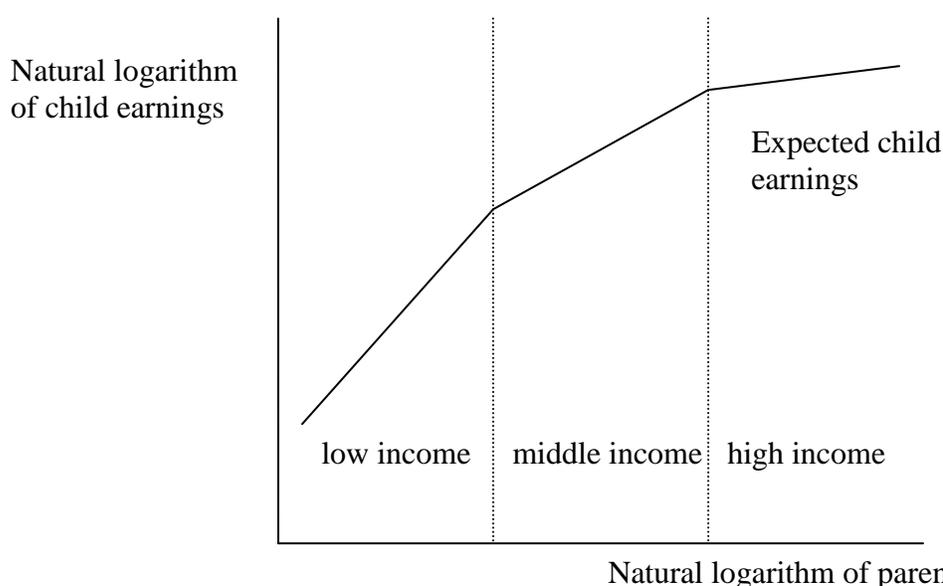
Pointing to a substantial literature summarized in Heckman (2000), Carneiro and Heckman (2002) question the single-dimensional nature of ability in the Becker and Tomes model. They argue that cognitive and non-cognitive skills are distinct: individuals blessed with high levels of one skill type may not be so blessed in the other dimension. The empirical results in Willis and Rosen (1979) and Carneiro et al. (2001) are consistent with this contention.

Allowing for multiple abilities, Carneiro and Heckman model education investments in the context of comparative advantage. The choice to attend college may be as much about low returns in the non-college sector as high returns in the market for college graduates. Adding a second dimension to ability has the potential to substantially alter our understanding of evidence for **credit constraints in higher education**. Where many authors have pointed to high returns among marginal college students as evidence for binding credit constraints, Carneiro and Heckman argue the evidence may as easily reflect greater non-cognitive ability among non-college-attendees.

## EVIDENCE OF THE IMPACT OF CREDIT CONSTRAINTS ON INTERGENERATIONAL MOBILITY

The [theory of intergenerational mobility](#) presented in Becker and Tomes (1986) has been widely taken to suggest a relationship between access to credit and variation in relative earnings mobility across parent income levels. If low-income families are the ones most likely to limit educational investments due to lack of credit access, financial constraints will increase earnings persistence at the bottom of the income distribution. Based on this line of reasoning, Becker and Tomes suggest that credit constraints will lead to a stronger parent-child earnings association among low-income families than among high-income families, as seen in the figure below.

### Becker and Tomes' Conjecture of Intergenerational Earnings Patterns in the Presence of Credit Constraints



Note: Because Becker and Tomes suppose that credit constraints become less and less relevant as parent income rises from low- to middle-income and from middle- to high-income status, they predict that the association between parent and child earnings will be strongest among low-income families and weakest among high-income families.

Contrary to the conjecture, many studies exploring U.S. data find precisely the opposite relationship: there is, in fact, a stronger association between parents' and children's earnings among high-income families than among low-income families. (See Behrman and Taubman 1990, Solon 1992, Mulligan 1997 and 1999, and Mazumder 2005.) Shea (2000), who takes a slightly different approach, is the lone exception. He examines how parental earnings differences due to union status, industry, and job loss relate to child earnings. While he finds constant mobility across parent income in the general population, in a sample of low-income families he finds less relative mobility at the bottom of the income distribution.

Recent research questions the validity of these tests for credit constraints. Using a much larger, Canadian dataset, Corak and Heisz (1999) find an S-shape relationship (high relative mobility at low income levels, lower relative mobility at middle income levels, and high relative mobility at high income levels). In discussing their results, the authors note that the Becker and Tomes conjecture may be incomplete. Because theory suggests that high-earning parents have more able

children, low-income families may not be constrained—especially in a society with free K-12 education, because these families may not demand as much higher education. Corak and Heisz speculate that it may, rather, be middle-income families whose (somewhat more able) children struggle to finance educational investments.

Grawe (2004) builds on Corak and Heisz's observation, demonstrating that theory is actually silent concerning the shape of relative earnings mobility across the family income distribution: absolutely any shape can be produced by economies with and without credit constraints. Thus, observations of the shape cannot tell us whether credit constraints are present or absent. Grawe goes on to find that patterns of intergenerational mobility across the *sons'* earnings distribution in Canada are not consistent with the simple credit constraint story. Han and Mulligan (2001) explore a calibrated simulation and report that any impacts of credit constraints on aggregate mobility would likely be too small to show up in the data.

While the literature questions the use of patterns in relative earnings mobility to identify the impact of credit constraints, it supports several related approaches. Mazumder (2005) finds greater mobility among families with large wealth stocks—a finding consistent with credit constraints. Although theory is ambiguous on the connection between credit constraints and earnings mobility, it produces very sharp predications concerning mobility in consumption. Due to challenges in collecting consumption data, Mulligan (1997a, 1999) is the only researcher to pursue this line of work. His estimates of consumption persistence are inconsistent with credit constraints.

## CREDIT CONSTRAINTS AND ENROLLMENT IN POST-SECONDARY SCHOOLING

In the last 50 years, the college-high school wage premium has nearly doubled from around 30 percent to just over 60 percent, reaching levels not seen since the turn of the twentieth century (Goldin and Katz 2007). Not only do college graduates earn much more than high school graduates, but much of the returns appear to be caused by the college experience (Card 1999, Heckman and LaFontaine 2006). There is little doubt that post-secondary schooling aids absolute mobility. With such high stakes, it is little surprise that economists have carefully studied access to college and its relationship to family income (and so, to relative mobility).

Many authors identify a strong correlation between parent income and college enrollment among children. For example, Carneiro and Heckman (2002) report an 80 percent college participation rate among 18- to 24-year-old high school completers with parents in the top half of the income distribution. By comparison, the participation rate falls to roughly 67 percent when parents' incomes lie in the third quartile and to just over 50 percent for children with parents in the bottom quartile. What is more, Ellwood and Kane (2000) and Haveman and Smeeding (2006) find that the correlation between family income and child educational attainment has increased in recent years.

Theory suggests two important explanations for this correlation. To the extent that children are similar to their parents, high income parents who excelled in school likely have children with strong cognitive skills. But parental income may also directly limit educational choices due to financial constraints. In theory, if students can borrow against future earnings, they will continue to enroll in college so long as the returns exceed the costs. However, if access to credit is not available, low parent income may directly cause lower educational attainment. (Alternatively, children from low-income families may be unwilling to borrow even if credit is available. Because the tests discussed below cannot distinguish between credit constraints and unwillingness to use available credit, all estimates of the impact of credit access are upper bounds on the true effects of credit constraints.) If credit constraints are more prevalent among low-income families, financial constraints may diminish relative mobility as well.

In assessing the empirical importance of credit in higher educational attainment, many authors cite evidence for substantial financial constraints based on several intuitive tests. Krueger (2003b) points to four empirical results which he argues indicate the importance of credit access to college enrollment. One of these (non-linearity in the intergenerational earnings association) is taken up in another section: here we discuss the other three.

Perhaps the most cited argument contends that credit constraints can be inferred from the repeated finding that returns to college education are higher among marginal college students—those who are just on the fence between enrolling or not—than among average college students (Card 1999, 2001, Kane 2001, and DeLong et al. 2003.) The idea for this test is rooted in the single-ability theory of education investments. Families invest in education so long as the benefit exceeds the cost. Ultimately, the last unit of education purchased must have a return just equal to its cost. So, if returns are higher among the students who are just indifferent between attending or not, this would suggest they face a higher cost than the average college student. Many authors interpret the higher return as a reflection of a higher opportunity cost of financial resources due to credit constraints.

By contrast, Carneiro and Heckman (2002) argue that the higher return for marginal college students should rather be understood in the context of a model which allows for multiple skill

types. They expand the standard economic model of education choice to allow that skills involved in non-college occupations may differ from those in college occupations. Willis and Rosen (1979) and Carneiro et al. (2001) report evidence that two distinct skill types exist and, importantly, those with greater “white-collar” skill tend to have less “blue-collar” skill. Thus, a student’s decision to forgo college may reflect greater opportunities in the non-college sector. And so marginal students do indeed have a higher opportunity cost, but it may be for reasons other than financial constraints. (Carneiro and Heckman additionally argue that the observed pattern in rates of return can be explained without credit constraints by expanding the model to allow for different quality across colleges.)

The second intuitive test for the importance of family finances in college decisions focuses on the effects of tuition changes on enrollment. Kane (1994, 1999) and Ellwood and Kane (2000) find greater enrollment response to tuition changes among low-income families than among middle- and high-income families. (Cameron and Heckman 1999, however, find only statistically insignificant differences in tuition responsiveness across family income groups.) Similarly, Brown et al. (2007) find that the effects of sibling overlap in college years (which leads to lower tuition costs under federal financial aid formulas) on educational attainment is positive among children who do not receive post-college financial gifts from their parents and zero among those who do. Ellwood and Kane reason that these patterns reflect the differential importance of credit limitations across the income distribution. In middle- and high-income families who are assumed to be unconstrained, the level of tuition is of less importance than in financially strapped, low-income households.

While the intuition for this test is plain, theory is actually silent on which families’ enrollment decisions ought to be most affected by tuition changes. Mulligan (1997b) shows that the most widely used model of intergenerational education investment actually predicts the opposite pattern. And Carneiro and Heckman (2003) provide a simple model in which either pattern is possible.

Finally, Behrman and Taubman (1990) find a stronger relationship between parent and child earnings when parent earnings are measured during the teen years than when parent earnings are observed at later points in the lifecycle. They point to this result as evidence of credit constraints because it appears that parent income at the time of the college enrollment decision takes on particular significance. Duncan and Brookes-Gunn (1997) find that this pattern is more general: the impact of parent earnings decreases from early childhood through adolescence and into adulthood. Using the same logic as Behrman and Taubman, Duncan and Brookes-Gunn also associate larger estimates of parent income impact as evidence that financial constraints must limit parents’ ability to provide for children at these early stages of development.

Grawe (2006) and Haider and Solon (2006) point out that patterns of earnings variance over the lifecycle explained in Mincer’s (1958) human capital investment model predicts exactly this pattern (without any reference to credit constraints). Mincer’s model explains the often-documented fact that lifecycle earnings growth is positively correlated with income. A consequence of this predictable pattern is that when lifetime earnings are measured based on annual earnings early in the lifecycle, the earnings of high-earning fathers are actually understated while the earnings of low-earning fathers are over-stated. Later in the lifecycle it is low-earning fathers’ whose earnings are understated and high-earning fathers’ whose earnings are overstated. Grawe (2006) and Haider and Solon (2006) show that these lifecycle patterns of measurement error lead to larger estimates of parental income impacts when fathers are measured when young than when old.

Carneiro and Heckman (2003) propose a more direct test for the importance of parent financial resources. They estimate the effect of parent income on the probability of post-secondary enrollment while controlling for student cognitive ability and long-run family characteristics. Cameron and Heckman (1998, 1999, 2001) and Carneiro and Heckman (2002) find that little or no role remains for family income. In total, Carneiro and Heckman (2002) find that 4.2 percent of young men and women fail to enroll in college due to financial constraints. These constrained children are equally spread across families in the first, second, and third income quartile. By contrast, Dynarski (2003) find that in the early 1980s elimination of the Social Security Student Benefit program resulted in reduction in college even after controlling for student cognitive skill.

When Cameron and Heckman look at college completion rather than college enrollment, they find no evidence of credit constraints in four-year college graduation. In fact, children from low- and middle-income families are more likely to complete four-year college when child ability and family characteristics are controlled for. Two-year college graduation rates do suggest a role for financial resources: as many as 7.7 percent of young men and women fail to complete two-year college due to issues of credit. Roughly half of those who fail to complete two-year college for what appear to be financial reasons come from families in the lowest income quartile. Because this is the only dimension of college achievement in which Carneiro and Heckman find a disproportionate effect among families in the lowest income quartile, it is also the dimension that is most relevant to relative intergenerational mobility.

Dynamic, structural models of educational choice presented in Keane and Wolpin (2001) and Restuccia and Urrutia (2004) also find limited effects of credit constraints on post-secondary schooling attainment. While the baseline model in Keane and Wolpin assumes credit constraints are tight, the authors find “essentially zero” change in schooling as this constraint is relaxed (p. 1089). Nearly all of the impact of credit constraints is found in student consumption expenditures—not in their educational investment decisions. Similarly, Restuccia and Urrutia assume credit constraints bind. But when they relax the constraint by simulating a grant to parents equal to 30 percent of college tuition, they find the college going decision is affected for only 2.5 percent of parents.

The debate over the prevalence of children whose educations are limited by credit has very practical implications for education finance. If many students are locked out of post-secondary education for lack of financial means, both efficiency and equity considerations argue for augmented higher education subsidies. But if low-income students rarely hampered by access to credit, then broad programs offering subsidies to educational investments will accomplish little in the way of aiding low-income students while benefiting middle-income students who would have enrolled without assistance. Estimates in Dynarski (2000) and Cameron and Heckman (1999) suggest that this is precisely the impact of the HOPE Scholarship. Both studies find that over 90 percent of benefits flowed to those already bound for a college degree. Given the large college wage premium, the net result is highly regressive and contrary to goals of increasing relative mobility.

Although the debate in this area is vigorous, there is perhaps a fair bit of common ground when it comes to the question at the heart of this project. To what degree is mobility diminished due to limited access to credit when financing higher education? While Ellwood and Kane (2000) are found on the side of the debate arguing that credit constraints are relevant, they ultimately conclude that parent financial resources account for a modest fraction of intergenerational earnings persistence. Based on their estimates, completely eradicating credit constraints in higher education might increase relative intergenerational mobility by roughly 5 to 10 percent.

When considering this estimate, two points must be kept in mind. First, it only considers the effects of credit access in higher education decisions. If restricted access to credit at earlier stages of the lifecycle limits earlier investments in a child's education such that the child arrives at age 18 unprepared for college-level work, additional financial assistance at the point of the college attendance decision would have little effect even though financial aid earlier in life could have a substantial impact. For instance, the [literature that finds high returns to preschool interventions](#) might be viewed as evidence that credit constraints matter, just at an earlier point in the child's life cycle.

Second, as the previous point makes clear, this estimate is conditional on current patterns of public expenditure on education. In particular, if increased funding at preschool and K-12 levels raised student ability so that more students become prepared for college, it may be that these new potential college attendees would be credit constrained in the absence of additional funding for higher education. Thus, as public funding patterns change over time we must re-evaluate the extent to which family finances affect relative (and absolute) intergenerational mobility.

Readers interested in a fuller discussion of both sides of the debate over college financing should consult Carneiro and Heckman (2003) and Krueger (2003b).

## THE EFFECTIVENESS OF GED AND JOB TRAINING PROGRAMS

Cameron and Heckman (1993) report that between 1968 and 1987 the fraction of high school diplomas awarded via general equivalence degrees (GEDs) increased from 5 percent to 14 percent. Today, one in five high school graduates acquired their degree through the GED (Carneiro and Heckman 2003). With the returns to skill reaching record levels, it is little wonder that so many view certification as an important goal for high school dropouts. But given that the median recipient spends just 30 hours in preparation for the exam (see Baldwin 1990), does the GED achieve the “equivalence” standard it proclaims?

Cameron and Heckman (1993) completed the first, influential review of the program and conclude that it does not. In their study of men aged 25 to 27, they find important ability and education differences between those acquiring the GED as compared with other dropouts. In particular, students who complete the GED stay in school longer than non-GED dropouts and score more highly on cognitive tests. In fact, the test scores of GED recipients in the early 1990s nearly matched those of high school graduates who did not enroll in college. (Heckman and LaFontaine 2006 find that the average ability of GED recipients has diminished over the last decade. As the number of students pursuing the GED has increased, this may simply reflect a change in the composition of the population.) Recognizing the large ability gap between GED recipients and other high school dropouts is key to understanding this literature because it underscores the potential selection bias present in studies which compare the two groups without controlling for differences in ability.

Given these skill differences, it is unsurprising that GED recipients earn more than other dropouts. Controlling for differences in cognitive ability and years of high school attendance, is there any remaining earnings advantage attributable to the GED? Cameron and Heckman conclude that there is none. In other words, a good deal of the GED “effect” claimed by studies that do not control for ability is actually a reflection of differences in student aptitudes and time in high school.

Subsequent studies question Cameron and Heckman’s pessimistic conclusions. The effects of the GED may not mature until after young adults reach the ages of 25 to 27. This might be the case if much of the gains in the GED were tied to access to subsequent education. Alternatively, the small, National Longitudinal Study of Youth (NLSY) sample used in Cameron and Heckman may be unable to detect small earnings differences. Moreover, the returns to the GED may differ across subgroups—a possibility impossible to fully explore with the small NLSY study.

Murnane et al. (1997) find evidence to support the idea that the GED is a gateway to further training. The probability of college attendance increases for men by 2 to 3 percent per year and for women by 2 to 5 percent. (Women are also slightly more likely to acquire training.) Using a fixed-effects model to control for unobserved ability, Murnane et al. (1995) report earnings trajectories that seem consistent with these greater human capital investments. GED receipt produces an initial (very slight) drop in earnings followed by faster earnings growth. After five years of post-GED experience, workers with GEDs earn more than do dropouts who lack certification. Murnane et al. (2000) further find that wage gains are stronger for students with weak math test scores. This suggests that the value of the certificate may be greatest for the least able students.

Heckman and LaFontaine (2006) respond to this literature with a re-analysis of the NLSY data. With more than a decade passing between this and the original Cameron and Heckman study, the authors are able to estimate returns to the certificate for older individuals. Unlike Murnane et al.

(1995), Heckman and LaFontaine do not find evidence of higher earnings or higher earnings growth for GED recipients after controlling for actual years of education and cognitive ability. The authors do, however, find substantial returns to college enrollment, “clear evidence of investment occurring in college” (p. 684), but not in GED programs.

Despite the disagreements, much common ground remains. All studies agree with Murnane et al. (1995 p. 144) when they conclude, “Do GED-holders fare as well in the labor market as conventional high school graduates?...The answer to this question is no.” If the GED has value, it appears to be as an entrance pass to more powerful investments. With the very low educational content of the typical GED course of study, these results are hardly surprising.

The job training literature mirrors the GED literature in many ways. Bloom et al. (1997) perform a large, randomized impact study of the Job Training Partnership Act (JTPA). JTPA participants were given access to basic schooling (often in preparation for the GED exam) and skills training. While adult participants experienced annual earning gains of between \$1600 and \$1800, the program was ineffective for out-of-school youths. Bloom et al.’s cost-benefit analysis finds that among the adult population, benefits exceed costs by a 2-to-1 margin. However, Carneiro and Heckman (2003) note that the welfare costs of raising the required taxes all but eliminate excess benefits. (The rate at which future benefits are discounted relative to current costs is also an important [methodological issue in assessing cost-benefit analyses](#).)

Some proponents of job training argue that “the [JTPA] treatment effect might have been small because the treatment was *small*” (Krueger 2003 p. 50, emphasis in original). Job Corps, represents a much more intensive alternative. Annually serving more than 60,000 16- to 24-year-old high school dropouts, the program provides around 90 percent of participants with academic, vocational, and health education along with job placement assistance at a cost in excess of \$14,000 per participant. Burghardt et al. (2001) perform a randomized impact study of program. They find the Job Corp participation increases the probability of GED receipt by nearly 50 percent (from 27 percent to 42 percent), but decreases the probability of acquiring a high school diploma by an equal degree (from 8 percent to 5 percent). It has no effect on college attendance. While earnings were diminished during the program itself, post-training earnings of participants exceeded those of the control group by roughly \$1150 per year.

While Burghardt et al. estimate a total lifetime benefit of \$31,000 per participant, Carneiro and Heckman (2003) point out that the results hinge critically on out-of-sample extrapolation. At the last point of observation, total societal benefits fell short of costs by \$10,000. To arrive at the larger calculated benefit, Burghardt et al. assumed that program benefits would persist indefinitely. However, Ashenfelter’s (1978) study of JTPA impacts suggests an annual benefit depreciation rate of 13 percent. If this rate of depreciation also applies to Job Corps, benefits will never exceed the costs.

Private firms also engage in significant amounts of training. Estimates of gains in private training programs are quite large. (See Bishop 1994 and Osterman 1995, for example.) However, these estimates do not control for selection bias. Given that we know firms hire and train more able applicants, these estimates are surely heavily tainted with selection bias. Without more careful study akin to that applied to public initiatives, we cannot draw firm conclusions on the effectiveness of these programs. As a means for increasing mobility, private programs’ tendency to cherry-pick participants suggests they would not be a particularly strong policy option.

In summary, the evidence of effectiveness for GED and public job training programs appears to be weaker than that for traditional education programs. The programs do preferentially treat low-

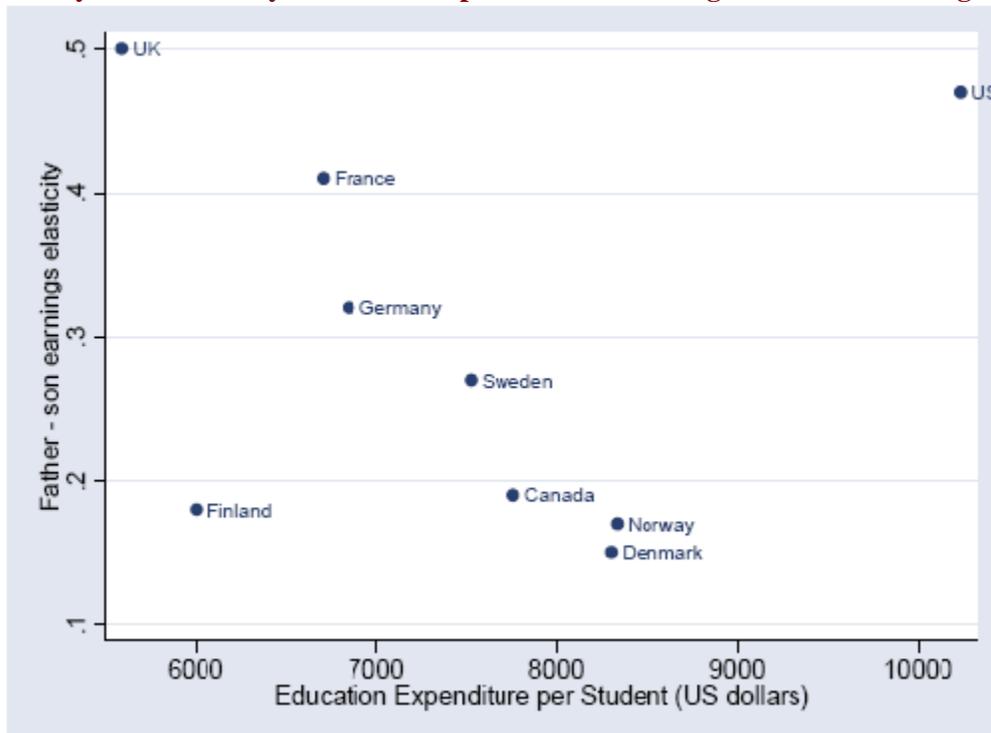
income individuals and some evidence suggests they are especially effective for those with the least skill, suggesting some potential to address relative mobility. However, all but the most optimistic estimates find costs in excess of benefits. Moreover, the literature consistently points to the superiority of other available education options. To the extent possible, it appears both absolute and relative mobility would be enhanced more by providing all students with an effective education in primary and secondary school.

## DIRECT EVIDENCE ON K-12 EDUCATION SPENDING AND INTERGENERATIONAL MOBILITY

Attempts to identify drivers of *intergenerational mobility* are stymied by the strong data requirements inherent to such studies. To estimate intergenerational mobility in even one economy, a dataset including earnings of a son must be linked to earnings of his father 20 or 30 years prior. Because these datasets are expensive and longitudinal, intergenerational datasets even more so, we have estimates for a relatively small set of countries.

Corak (2006) carefully examines existing estimates, adjusting for methodological differences, to arrive at comparable figures for mobility in nine OECD countries. He then examines the relationship between estimated intergenerational earnings persistence (or “elasticity”) and public expenditures on primary and secondary education. The *correlation* between education funding and relative mobility is very weak (-.062). (See figure below.)

### Primary and Secondary Education Expenditure and Intergenerational Earnings Persistence



Source: Corak (2006). Reprinted with permission.

While removing the outlying observations from the United States and Finland substantially improves the association, this only serves to emphasize what we do not know: why, despite large public education expenditures, is earnings persistence so high in the United States? Persuaded by the literature on [early childhood interventions](#) and noting the public support for child care in mobile Nordic countries, Esping-Andersen (2004 p. 289) suggests “the ‘cultural capital’ of families” may be very important in driving mobility. While this interpretation is plausible, the reality is that we have too few observations from too few economies to draw clear conclusions.

A recent study by Holmlund (2007) takes a more direct approach, studying the change in intergenerational mobility in Sweden surrounding an extension of compulsory schooling. Holmlund’s estimates are too imprecise to draw strong conclusions, but suggest that raising the

minimum school-leaving age from grade seven to grade nine reduced intergenerational earnings persistence by 12 percent. The implications for U.S. policy are muddled, however, by the fact that Sweden also reformed school tracking at the same time. Thus, the results may have been caused by this institutional reform as well.

Mulligan (1999) uses a relatively small panel data set to explore variation in relative mobility across states' class sizes, teacher pay, and per pupil expenditures. He found no difference in mobility associated with any of these school quality measures. Mayer and Lopoo (forthcoming) repeat the analysis with the same dataset taking advantage of more recently collected data. Using Mulligan's basic specification they too find no effect of government expenditures on elementary and secondary schooling. However, when they divide states into thirds by government expenditure, they report significant differences across the groups with greater relative mobility in the high-spending states. Grawe (2007), however, finds less relative mobility among states with low pupil-to-teacher ratios using U.S. Census data.

The direct evaluation of the effects of K-12 schooling on mobility is still in its very early stages and the literature is thin. One deficiency in all three of these studies is that the school quality measures are state aggregates. Improvements in school quality as reflected in a state average may not equally apply to students in all school districts. For instance, Stetcher et al. (2005) find that the recent class size reduction initiative in California resulted in a decrease in average teacher qualifications and that the biggest losses in qualifications were found in the poorest districts. The lack of data on school quality at the district level means we must be cautious in drawing strong conclusions from this very young line of literature.

## THE EFFECT OF K-12 SCHOOL QUALITY ON CHILD PERFORMANCE

Recent trends in school quality and student performance are very discouraging (Hanushek 1999, 2003). While the Department of Education reports substantial declines in pupil-to-teacher ratios and increases in per pupil expenditure, teacher qualifications, and teacher pay (see below), Hanushek finds “flat” performance on the National Assessment of Educational Progress exam (2003 p. 256) and routinely mediocre performance relative to students in other countries on math and science tests.

### Improvements in Primary and Secondary School Quality, 1955–2000

	Year					
	1955	1965	1975	1985	1995	2001
Real current expenditure per pupil in average daily attendance (constant 2004–05 dollars)	\$2,098	\$3,230	\$5,197	\$6,616	\$7,627	\$8,884
Pupil/teacher ratio	26.9	24.7	20.4	17.9	17.3	15.9
Percent of teachers with masters degree or higher		23.3	37.5	51.4	56.2	56.8
Average annual salary as classroom teacher (in constant 2005 dollars)		\$6,253	\$12,005	\$24,504	\$35,549	\$43,262

Source: *Digest of Education Statistics 2005*

All scholars recognize the folly of comparing these time trends to ascertain the effect of school quality on child achievement. The same time period has seen massive changes in other determinants of child success—some aiding and some undermining child development. (See Hanushek 2003 p. 257 for several examples with summary statistics.) Studies examining the effect of school quality on child achievement must look to variations across schools, districts, and/or states.

While this is a hotly debated area of research, a majority of studies suggest improvements in school quality increase test scores and later lifetime earnings. The literature considers many measures of school quality, but for the sake of brevity this section will focus on two representative topics: class size (“more resources”) and school competition (“better administered resources”).

One notable exception to the general consensus is Hanushek (1997, 1998) who surveys the literature and finds 377 estimates of returns to school quality in general with 277 estimates specifically examining the impact of class size. He finds that a vast majority (72 percent) of studies find no statistical evidence of a class size effect. Only 15 percent find a significant positive effect as compared to 13 percent that find a significant negative effect. Of those estimates which are not statistically significant, they are about as likely to be positive as to be negative. He summarizes the distribution as reflecting “what one would expect to find if there was no systematic relationship between class size and student performance” (1998 p. 22).

Krueger (2003a, 2003b) disagrees with Hanushek’s reading of the literature on two counts. First, he questions the relevance of the results from several of the surveyed studies because they include controls for both per-pupil expenditure and class size. Because smaller class sizes go hand in hand with higher per-pupil expenditures, including both measures in a single regression almost

ensures that neither will appear to have an *independent* effect. Second, Krueger argues that Hanushek has improperly weighted the studies in his survey. In particular, Hanushek's 277 estimates come from just 59 studies. Some studies contribute only one estimate of class size effects while others contribute as many as 24. Krueger argues that each publication should be given equal weight. By this accounting, for every study finding a significant negative class size effect, four find a significant positive effect.

One reason such policy debates are hard to end is that we rarely have experimental data in which students are randomly assigned to receive better/worse school quality. And so skeptics will suggest that the perceived positive effects actually stem from the differences in communities that lead them to choose different policies. When clean experiment does come along, it is given exceptional attention. The Tennessee STAR program is just such a case. At the entrance to kindergarten, children were randomly assigned to either a class of 15 students or to an average sized class of 22–24 pupils. Students placed in small classes remained in small classes through 3<sup>rd</sup> grade. Over 11,600 students from 79 schools participated in the study.

Krueger (1999) studies the effects of the treatment. Students in the small classes scored 0.2 standard deviations higher on achievement tests after just one year of treatment. These advantages remained stable through third grade when the small class size treatment ended. In fourth grade testing, the advantage for the treatment group was cut in half, but then persisted at this lower level through eighth grade. Krueger also found the effect to be roughly 25 percent stronger in disadvantaged groups (blacks and recipients of free lunch) suggesting that such treatments may raise relative mobility as well as absolute mobility.

While the majority of studies point to positive school quality effects, the magnitude of these effects is discouragingly small. In part, this may be because higher tests scores do not translate well into higher earnings (Cawley et al. 1999, Heckman and Vytlačil 2001). Whatever the reason, the benefits may fall short of costs even accepting the largest estimates for the returns to schooling quality. While Krueger (2003) calculated a 2-to-1 benefit-cost ratio for of school quality improvements, Carneiro and Heckman (2003) argue that the results are [sensitive to small methodological changes](#). They contend that even if earnings increase 4 percent in response to a five-pupil reduction in class size (a large estimate based on the literature), lifetime costs exceed benefits by between \$2,600 and \$5,500 per student under more conservative methodological assumptions. Of course, we may still be willing to pay such a price if absolute and relative mobility are enhanced.

Hoxby (1999) argues that the large number of studies on the amount of K-12 spending may be missing an important, related question: Can we do better with the resources we have by modifying the market for education? Hoxby points to the example of higher education, where colleges and universities face extensive competition. By contrast, K-12 schools typically experience very little competition. This market structure may affect administration decisions such as hiring and pay scale. For instance, Ballou and Podgursky (1995) find that schools give little weight to candidates' academic records in hiring decisions. Hanushek and Rivkin (2003) and Hoxby (2002) find that public schools respond to competitive pressure with more market-driven teacher contracts.

A variety of programs fall under the general heading of "school choice" from open enrollment programs which allow students to transfer between public schools within a district to publicly funded charter schools to vouchers which can be used in private schools. As Hoxby (2003) points out, these various programs differ dramatically in their market structures. In particular, open enrollment within a district has no financial consequence because under no circumstance can the

family's choice affect district funding. Hoxby (2004) argues these institutional limitations explain why Cullin et al. (2005) find little causal effect of the Chicago open enrollment policy on student achievement.

By comparison, voucher programs have significant consequences to school enrollments and, in some cases, school funding. Hoxby (2004) studies a randomized voucher program in Milwaukee and reports that schools which allowed competition in this form posted large gains in student performance on national tests: 8 percentile points in math, 14 in science, and 8 in language. Hoxby also studies the effect of charter school creation in Arizona and Michigan and finds substantial increases in the level and growth of student performance.

Not only do public schools facing competition improve, students who transfer to an alternative school post test gains as well. Hoxby (2004) summarizes five studies in New York City NY, Washington DC, Dayton OH, Milwaukee WI, and Cleveland OH. (These studies were completed by Myers et al. 2000, Wolf et al. 2001, West et al. 2001, Greene et al. 1997, and Peterson et al. 1999). (See also Howell et al. 2002.) All find that voucher programs improve the standardized test scores of aided students with the gains ranging between 4.3 and 9.0 national percentile ranks. Similarly, Neal (1997) and Grogger and Neal (2000) find access to private schools raises outcomes among black students. Interestingly, all of these studies suggest the impact may be greatest for black students. To the extent that school choice programs especially aid those from disadvantaged backgrounds, we would expect them to increase relative intergenerational mobility.

The potential relevance of institutional reforms to enhanced relative mobility has been highlighted by two recent studies of education reform in Scandinavia. Both Holmlund (2007) and Pekkarinen et al. (2006) find that mobility increased when schools eliminated or reduced tracking. (The former reports a 12 percent decrease in intergenerational earnings persistence in Sweden while the latter finds a 7 percent reduction in Finland.) Because U.S. schools have never engaged in such tracking, these results are not directly applicable. But they do suggest that how we spend resources may be as important as how much support we provide to education.

## EARLY CHILDHOOD INTERVENTION AND CHILD PERFORMANCE

Investments in early childhood education have risen dramatically in the past 40 years. At the inception of Head Start in 1966, the program served 733,000 students at a total cost of \$1.3 billion (in current dollars). Today the program engages roughly 25 percent more students for a total of 908,000 participants. And expenditures have increased more than 400 percent to \$6.8 billion. In addition, states have begun funding their own preschool initiatives: Barnett et al. (2005) report that as of 2004–05, 28 states spent an additional \$3 billion on such programs. A growing literature suggests these investments may yield large returns (raising absolute mobility) and enhance relative mobility as well by improving outcomes for children from low-income homes.

With its long history and large footprint, it is not surprising that Head Start has received more research attention than other preschool initiatives. Studies report initial gains in cognitive skill, but test score improvements diminish with time (Barnett 1992, 1995; Currie and Thomas 1995; and Karoly et al. 1998). Despite this erosion in test score improvements, Garces et al. (2002) find that Head Start produces important long-run effects. Among whites, high school graduation and college enrollment rates increased by 20 percentage points. For those born to mothers with no more than a high school degree, the effect was even greater. While blacks did not see statistically significant gains in educational attainment, they posted significant decreases in crime.

The findings relating to Head Start are corroborated in studies of other preschool interventions. Heckman (2000) and Waldfogel (2002) summarize a wide range of studies. These surveys summarize that while preschool programs produce only short-term improvements in cognitive test scores, studies repeatedly document long-term improvements in educational attainment, employment, and crime—improvements that appear more related to non-cognitive skill enhancements. Similarly, Ludwig and Miller (forthcoming) find large reductions in mortality for children ages 5 to 9 due to causes addressed by Head Start's health programs. While the economics literature has paid less attention to the non-cognitive dimensions of human capital, a substantial literature documents its relevance to educational and economic achievement (Bowles and Gintis 1976, Edwards 1976, and Klein et al. 1991). Research further documents the important role of non-cognitive skill in determining intergenerational mobility.

Given this prominent role of non-cognitive skill, it is not surprising that some of the most successful programs have been very intensive, including in-home support and parent mentoring. Rigorous, comprehensive programs of this nature are, of course, quite expensive. Despite these high, up-front costs, three demonstration programs—the Abecedarian Project, the Chicago Child-Parent Centers, and the Perry Preschool Program—conclude the benefits may exceed the costs many times over. Carneiro and Heckman perform a cost-benefit analysis of the Perry Preschool Program based on the findings of Karoly et al. (1998) and Barnett (1993). Based on only the tangible benefits at age 27 (the point of latest observation), they find the lifetime benefit-to-cost ratio exceeds 5-to-1. If returns are projected out to cover the entire lifecycle, the rate of return nears 9-to-1.

The results of Head Start and other preschool interventions are very promising for policy-makers interested in increasing both absolute and relative mobility. Because these programs target at-risk children, the gains reported are likely to raise the outcomes of the lowest earners and enhance mobility. What is more, even within the population served by these programs, studies estimate that the greatest gains are experienced by children in particularly disadvantaged groups.

Those interested in a fuller discussion of the literature on Head Start should consult the symposium in the summer 2007 issue of the *Journal of Policy Analysis and Management* or Ludwig and Phillips (2006). For a fuller survey of preschool demonstration programs, see Waldfogel (2002) and Carneiro and Heckman (2003).

## **AN ENDOWMENT MODEL FOR EARLY EDUCATION FUNDING**

The literature on education interventions, preschool to post-secondary, consistently finds that effective interventions are intensive. At the preschool level, [effective programs](#) often include a public health element and consistently involve parenting programs designed to foster a more successful home environment. Of course, this kind of programming is expensive. For instance, Head Start currently enrolls around 900,000 students at a total cost of \$7 billion or about \$7,200 per pupil. The highly touted Perry Preschool and Abecedarian programs cost more than twice as much.

Grunewald and Rolnick (2006) present a model for providing and funding preschool education for all at-risk children. In Minnesota, they estimate an \$85 million annual price tag for providing extensive child education combined with parent mentoring for all children living in poverty. While this is a non-trivial cost, it represents only one percent of current public education spending in Minnesota.

Because Grunewald and Rolnick argue that uncertainties in funding undermine program performance, they propose that the initiative be sustained indefinitely through the creation of a \$1.5 billion endowment invested at six to seven percent in AAA corporate bonds. (To place this endowment figure in perspective, in other publications the authors compare it to the cost of two sports stadiums.) Moreover, Grunewald and Rolnick urge us to remember that multiple studies find that preschool investments reduce the probability of grade retention and special education use, thus offsetting as much as half of the cost of the program by diminished demands on K-12 education.

Given the claimed social benefits stemming from preschool investments, the authors also suggest seeking private support for the endowment. In “Ready for School” Minnesota’s School Readiness Business Advisory Council (2004) proposes just such a partnership.

## **RECURRING ISSUES IN ASSESSING COST-BENEFIT SUCCESS OF EDUCATION PROGRAMS**

While many issues are important to the calculation of cost-benefit ratios for education initiatives, several issues show up often enough in the debate to warrant brief explanation. One of the largest challenges in assessing program benefits is the fact that these gains typically play out over an entire lifetime.

This has two implications. First, given the practicalities of data gathering, we often must estimate benefits based on a short panel of observations. This can lead to either over- or underestimating the true benefit of the program. On one hand, we expect learning gains to decay with time. For instance, Ashenfelter (1978) finds that the effects of the Job Training Partnership Act interventions decayed at a rate of 13 percent per year. Ignoring such decay leads to overstatement of program benefits.

On the other hand, sometimes it takes time for the full effects of an intervention to play out. For example, Murnane et al. (1995) estimate that the effect of the GED on earnings is initially negative, but turns positive after five years—consistent with the GED serving as a gateway to further human capital acquisition. Krueger (2003) makes the same observation about the delayed positive effects of the Job Corps program which only posted positive earnings effects more than two years after entrance in the program.

In addition, Krueger (2003) argues that interventions may affect earnings growth rates as well as levels. Clearly, focusing exclusively on the levels in such case ignores a substantial part of the potential program benefit. In one example, Carneiro and Heckman (2003) find that estimates of benefits of class size reductions which ignore effects on earnings growth rates consistently produce negative cost-benefit ratios where calculations including changes to growth rates often produce positive ratios. For both of these reasons, assessments based on short program evaluations may understate program benefits.

The fact that program costs are up front whereas benefits extend over a long horizon introduces another important methodological issue: To what degree should we discount future benefits relative to current costs. All scholars agree that it would be inappropriate to equate \$1 of benefits attained five years in the future with \$1 of costs experienced in the present. But scholars disagree about just how much the future benefit should be discounted.

Krueger (2003) argues internal rates of return of 6 to 11 percent for education and training programs for disadvantaged children compare favorably to the historical annual real rate of return on the stock market (6.3 percent). But Summers (2003) points out that this rate of return ignores the taxes paid on corporate profits. Using the pre-tax rate of return sets a much higher standard in the “10 to 11 percent range” (p. 286). The debate over which discount rate to use can easily drive the results of a cost-benefit analysis even if the contested estimate is substantial. For example, Carneiro and Heckman (p. 158 2003) show that the cost-benefit ratio for a class-size reduction can fall from 5.2-to-1 to -3.3-to-1 to -6.9-to-1 as the discount rate increases from 3 percent to 5 percent to 7 percent.

Summers (2003) also argues that we must recognize that the programs we are evaluating are public. And unlike private investments, these public initiatives require distortionary taxation. The degree to which taxes distort household decisions is a matter of wide debate. Summers summarizes the literature by suggesting that the cost of raising \$1 of tax revenue is between \$1.20

and \$2.00. If the latter estimate is correct, even an estimated cost-benefit ratio of 2-to-1 would just break even.

Finally, Thurow (1971) suggests that equality is a public good. Similarly, absolute and relative mobility may be public goods for which we are willing to pay. Insofar as this is true, even cost-benefit analyses that consider societal benefits such as reductions in crime may understate the full benefits of programs. Of course, it is impossible to assign incontrovertible dollar values to the public benefit of greater mobility. But we may nevertheless choose to keep the issue in mind as we consider cost-benefit ratios which exclude such gains.

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# EDUCATION AND ECONOMIC MOBILITY

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