Building the Stock of College-Educated Labor

Susan Dynarski

ABSTRACT

Half of college students drop out without completing a degree. This paper establishes a causal link between college costs and degree completion. I use quasi-experimental methodology to analyze two state scholarship programs. The programs increase the share of the exposed population with a college degree by three percentage points, with stronger effects among women. A cost-benefit analysis indicates that the programs are socially efficient at rates of return to schooling as low as 5 percent. Even with the offer of free tuition, many students continue to drop out, suggesting tuition costs are not the only impediment to college completion.

I. Introduction

College attendance has risen substantially over the past 30 years. In 1968, 36 percent of 23-year-olds had gone to college. By 2000, that figure had grown to 55 percent. Over the same period, the share of young people that has completed a college degree rose relatively slowly. In 1968, the share of 23-year-olds with a bachelor's degree was 14 percent, while in 2000 it was 19 percent.[1] As these figures make clear, many young people enter college but drop out before completing a degree. In the 2000 Census, just 57 percent of those age 22 to 34 with any college experience

---

1. These data, tabulated from the October Current Population Surveys of 1968-2000, are from Turner (2003). She shows that college students are taking increasingly longer to complete their degrees, contributing to the slow growth in completion among young workers.

Table 1

College Experience vs. College Completion, 22- to 34-year olds, Census 2000, One Percent Sample

	Any College	College Degree (AA or BA)	BA	Share with Any College Not Completing a Degree
Black or Hispanic	60.8%	26.8%	18.8%	55.9%
White non-Hispanic	71.4%	44.8%	35.4%	37.3%

had completed an associate's or bachelor's degree. Thirteen percent had not completed even a year.[2] See Table 1.

Sluggish growth in the stock of educated labor in the United States can be contrasted with much faster growth in other nations. In 1991, two countries (Canada and Finland) had higher shares of young people with a college degree than the United States. By 2002, 13 countries had equaled or exceeded the benchmark achieved by the United States in 1991, while four nations were now ahead, with Japan and Korea ahead by more than ten percentage points (Organization for Economic Cooperation and Development 2004).

Two demographic trends further retard growth in human capital: The well-educated baby boomers are exiting the labor force while increasing numbers of Blacks and Hispanics, who historically have had low levels of education, are entering (Ellwood 2001). Slow growth of the stock of educated labor has broad implications for economic and social policy. During the 1980s, rapid growth in the demand for educated labor combined with sluggish growth in its supply to generate a surge in earnings inequality (Katz and Murphy 1992; Autor, Katz, and Kearney 2005).

Significant growth in the college-educated work force requires an increase in college completion, as very large gains in college entry are now behind us. Seventy percent of young high school graduates have attended college (author's calculations from Census 2000); among children from high-income families, the rate is 90 percent (Ellwood and Kane 2000). There are several channels through which public policy might affect the completion margin. Sociologists have focused on strengthening connections between college students, their peers, and their teachers (Tinto 1994). Another strategy is to deepen academic preparation in primary and secondary school.

This paper focuses on a third channel: reducing college costs. A small literature has shown that decreasing college costs substantially increases college entry. Dynarski (2002) reviews this literature. By comparison, we know little about how costs affect college completion.[3] Theory does not unambiguously predict the impact of schooling costs on college dropout rates. In Manski's framework (1989), students learn about their academic skills when they enter college and, should they find them lacking, drop out. Marginal decreases to college costs may induce students with low

---

2. Author's tabulations from the Public Use Microdata One Percent Sample of the 2000 Census.
3. For examples of the analysis of the causal impact of college costs on attendance see Kane (1994), Dynarski (2000, 2003, 2004) and Seftor and Turner (2002).

skills and high expectations of dropping out to undertake this experiment. An alternative (but not incompatible) story is that the marginal college student may be credit-constrained. Relaxing credit constraints may then induce into college individuals with better academic skills than the typical college student.

Determining how schooling costs affect human capital investment is not straightforward, because the costs faced by a potential student may well be correlated with unobserved determinants of her educational attainment. This paper exploits the introduction of large scholarship programs in two Southern states to identify the effect of college costs on college completion. During the 1990s, a dozen states introduced large-scale merit aid programs. These programs waive tuition and fees for students who achieve a minimum grade point average (GPA) in high school (typically 3.0), and maintain a minimum GPA in college (typically 2.5 to 3.0). Arkansas started the trend in 1991, with Georgia following suit in 1993. These are not scholarships for the academic elite: nearly 60 percent of students graduating high school in Georgia qualify for its merit scholarship.

Previous work has shown that these programs have had a positive impact on college attendance (Dynarski 2000 and 2004). In those papers, data limitations prevented the estimation of the effect of merit aid on college completion. These data limitations have been relaxed by the release of the 2000 decennial census microdata. As of 2000, several cohorts who were exposed to the Arkansas and Georgia programs were in their early twenties, the traditional age of college graduation. I use a treatment-comparison research design to evaluate the effect of these programs on college completion rates. Cross-cohort differences in college completion within the states provide the identifying variation in the analysis. The identifying assumption is that any cross-cohort change in college completion in the treatment states, relative to the control states, is due to the scholarship programs.

To preview the results, I find a large and significant impact of college costs on degree receipt. The scholarship programs appear to increase the share of young people with a college degree by three percentage points, from a base of 27 percent—a substantial effect. Correcting for measurement error in treatment assignment boosts the estimate to about four percentage points.

The effects are strongest among women, with white, non-Hispanic women showing increases of 3.2 percentage points and the share of nonwhite and Hispanic women attempting or completing any years of college increasing by six percentage points. Differential performance in high school explains some of the gender differences in the effect of the merit scholarships. In course grades and standardized tests, girls outperform boys in high school and are substantially more likely to go on to college (Goldin, Katz, and Kuziemko 2005). More girls than boys meet the eligibility requirements for merit scholarships, with 49 percent of female college freshmen and 36 percent of males having a high school GPA of at least 3.0.[4]

The identifying assumption, while ultimately untestable, is subjected to a series of plausibility checks. A key threat to the internal validity of the estimates is any pre-program trend in college completion in the treatment states relative to the comparison states (Meyer 1995). A plausible scenario is that Arkansas and Georgia began to build their human capital years before introducing the scholarship programs, through

---

4. Author's calculations from the 1992 wave of the National Education Longitudinal Study of 1988.

increased investment in children's education or by attracting skilled adults from other states. In this scenario, these preexisting trends could even *cause* the scholarship programs, with well-educated parents in the treatment states demanding scholarships so as to reduce tuition costs for their college-bound children. Both preexisting trends and reverse causality would produce a spurious, positive correlation between the scholarship programs and educational attainment.

I address this critical set of concerns with several methods. First, throughout the analysis I assign program eligibility based on state of *birth* rather than state of *residence*. Recent migration into the treatment states by well-educated young workers and their families cannot, therefore, explain the results. Second, the data show a distinct break from trend for the affected cohorts. Third, I include in the regressions parametric and nonparametric controls for trends in education at the level of the state of birth and state of residence. Fourth, I show that results are robust to limiting the comparison group to the South or to states that introduce merit scholarships in the late 1990s. Fifth, including controls for demographic composition, cohort size, the college premium, unemployment rate, median family income, and spending on higher education in the state do not alter the results.[5] Finally, I show that the program effects occur only at educational margins plausibly affected by eligibility for the merit scholarships. Together, this set of results provides strong support for the identifying assumption of the paper.

My reduced-form estimation strategy cannot separately identify the effect of aid on entry and persistence conditional on entry. I can, however, place fairly narrow bounds on the persistence effect. The scholarship programs appear to increase by 5-11 percent the probability of persistence to degree of those who would have entered college even in the absence of the programs. A cost-benefit analysis indicates that tuition reduction is a socially efficient method for increasing college completion. Even at very low rates of return to schooling (40-60 percent below returns observed in the cross-section) the benefits of the scholarships outweigh their costs.

However, the paper's results clearly indicate scholarships *alone* will not keep the bulk of dropouts from leaving college. The programs studied in this paper drove to zero the direct costs of schooling for many entering college students, yet even with this offer of free tuition a large share of students continued to drop out of college.[6] Note that this finding does not rule out liquidity constraints as an explanation for low college completion rates, because the direct cost of college is a fraction of its total cost. Further, a conclusion that current tuition prices are not the main impediment to college completion does not imply that substantially raising the price of college would not increase the dropout rate, since there are likely nonlinearities in the response to price. But the results do suggest that the direct costs of college are not the only (or even the central) impediment to degree completion. Policymakers and researchers will need to explore additional avenues in order to substantially increase the stock of college-educated labor.

---

5. All of these variables are measured in the individual's the state of birth, in the year in which she was 18.
6. This conclusion accords with Stinebrickner and Stinebrickner (2003), who show that the dropout rate approaches 50 percent at Berea College, which pays all costs (tuition, fees, books, room and board) for all its students.

II. Background

A. *College Costs and College Completion*

Financial aid plausibly affects several margins of behavior: college attendance, college entry, and degree completion. College attendance is a state variable, indicating that at a given point in time a person is enrolled in college. Entry and completion are stock variables, indicating that a person has ever attended college or has completed college, respectively. Dozens of studies have examined the relationship between college costs and these outcomes (Leslie and Brinkman 1988). Almost all are plagued by identification problems, with the analyses failing to control for correlation between college costs and unobserved determinants of schooling outcomes.

A handful of well-identified studies, however, has established a strong casual link between schooling costs and college attendance. Most relevant to the current paper, Dynarski (2000, 2004) and Kane (2003) find that large-scale state merit scholarship programs substantially increase the share of young people attending college. Dynarski (2003) finds that the elimination of the Social Security student benefit program, which paid the college costs of the children of deceased parents, substantially reduced the college attendance of the affected population. Studies that examine the Pell Grant, currently the largest source of federal grant aid, produce somewhat mixed results: Hansen (1983) and Kane (1995) found no effect of the introduction of the Pell on the college attendance rate of low-income recent high school graduates, but recent work by Seftor and Turner (2002) has found a positive effect on the attendance of prime-age workers.

The evidence on the effect of aid on college persistence and completion is comparatively thin, especially for recent cohorts. Angrist (1993) and Bound and Turner (2002) show that the Korea and World-War II GI Bills increased the completed schooling of veterans. Both estimates are based on those exiting the military who had graduated high school in first few decades of the Twentieth Century; extrapolating their behavior to young high school graduates in 2005 is a stretch. Dynarski (2003) finds that the elimination of the Social Security student benefit program in 1981 reduced the schooling of the affected population of two-thirds of a year, but this result is imprecisely estimated. Bettinger (2004) examines the effect of grants on the persistence rate of college students, relying on discontinuities in the aid formula that affect those already enrolled in college. He cautiously concludes there is a positive effect, but notes that his point estimates are extremely sensitive to functional form.[7]

B. *State Merit Aid Programs*

Since the early 1990s, more than a dozen states have established broad-based merit aid scholarships. While states have long awarded college scholarships based on academic performance in high school, these programs have traditionally subsidized

---

7. Extrapolation of Bettinger's estimates requires assuming that the effect of a scholarship on the hazard of dropping out is linear in its value and constant across years of college. With these assumptions, his estimates suggest the Arkansas and Georgia programs will have effects two to three times larger than those found in the paper.

only the highest-performing students. For example, New York gives a scholarship to each high school's top scorer on the state Regents exam. The academically elite students who receive these scholarships are not on the margin of dropping out of college. By contrast, the new merit scholarships are open to students with solid but not exemplary academic records. A dozen state merit aid programs have eligibility criteria that are lenient enough that more than 30 percent of high school seniors qualify. Some of these scholarship recipients will be on the margin of dropping out, making these programs are a fruitful source of variation for understanding the relationship between college costs and persistence.

In 1993, just two states, Arkansas and Georgia, had large-scale merit scholarship programs in place. By 2003, 13 states had introduced large merit aid programs.[8] Most of the programs are too new to detect effects on college completion in currently available data. The oldest programs, established in Arkansas in 1991 and in Georgia in 1993, provide the identifying variation in the paper.[9]

The Arkansas program was proposed in January 1991 by then-Governor Bill Clinton. The program was quickly approved by the legislature and was in place in time for the high school class of 1991 to be offered scholarships for their fall enrollment. Eligibility for the Arkansas scholarship requires a 19 or above on the ACT, a score exceeded by 60 percent of test-takers nationwide and below the Arkansas state average of 20.4. Eligibility also requires a cumulative high school GPA of 2.5 in a set of core classes, a standard met by up to 60 percent of high school students nationwide.[10] Continued receipt of the scholarship requires a GPA of 2.5 in college and progress at the rate of at least 24 semester hours a year. The scholarship is available for a maximum of four years of undergraduate study.

At its inception the Arkansas program paid tuition and required fees up to $1,000 at public and private colleges in Arkansas; this was raised to $1,500 in 1994 and $2,500 in 1997. Starting in 1994, students received a bonus of $500 if their previous year's college grades were 3.0 or above. In light of prevailing tuition prices in Arkansas, these were generous subsidies. In 1994, tuition and required fees were $2,200 at University of Arkansas at Fayetteville (the state's flagship institution) and $1,110 at Arkansas State University at Beebe.

When the scholarship was introduced, Arkansas limited eligibility to those with family incomes below $35,000; this was raised to $40,000 in 1993 and to $75,000 in 1999.[11] These caps fall relatively high in the income distribution of Arkansas, where incomes are among the lowest in the nation. Median household income in the state was $27,000 and $32,000 in 1989 and 1999, respectively.

Georgia's Helping Outstanding Pupils Educationally (HOPE) program was proposed in early 1993 by then-Governor Zell Miller. This program, too, moved quickly from proposal to legislation, with the high school class of 1993 eligible for the first

8. Programs were introduced in Mississippi in 1996, Florida and New Mexico in 1997, Louisiana and South Carolina in 1998, Kentucky in 1999, Michigan and Nevada in 2000, Maryland and West Virginia in 2002, and Tennessee in 2003. Dynarski (2004) examines these programs.

9. Despite the differences between the two programs that I detail below, I find that their effects on college completion are virtually identical.

10. Author's calculations from the 1997 National Longitudinal Survey of Youth.

11. These shifts in the income caps, in principle, provide useful identifying variation. Unfortunately, in the census we cannot identify family income at the time an individual was graduating high school.

scholarships. The program is funded by a lottery, also established in 1993. The scholarship requires a 3.0 GPA in high school. Renewal of the scholarship requires a 3.0 GPA in college, with GPAs calculated when students accumulate 30, 60, 90, and 120 credit hours. Unlike the Arkansas program, there is no time limit on scholarship receipt, but funded credit hours are limited to 150.[12] While Arkansas' scholarship is dollar-denominated, the Georgia scholarship covers full tuition and fees at any Georgia public college or university. For Georgia private colleges and universities, the scholarship pays a lump sum that was gradually raised from $1,000 in 1993 to $3,000 in 1995. In 1993, HOPE was limited to children from families with income below $66,000. The cap was raised to $100,000 in 1994 and completely eliminated in 1995.[13]

The structure of the public university scholarship in Georgia provides incentives for schools to raise their prices in order to capture more scholarship funds. Countervailing pressure against price increases is provided by the legislature and students ineligible for the scholarships. The paper's estimates capture the reduced-form effect of the merit scholarships, which includes any drop in educational attainment among those ineligible for the scholarship but exposed to any price increases. Given the generosity of the scholarships and the small increases in price documented in the literature, any such offsetting effect is likely quite small.[14]

Both the Georgia and Arkansas scholarship programs were introduced in the context of broad-based education reforms at the state level. These are unlikely to contaminate the paper's results, as I now briefly discuss. In Georgia, the lottery that funded HOPE also paid for an expansion of prekindergarten programs and purchased technology upgrades for elementary and secondary school classrooms. These two programs were introduced contemporaneously with HOPE in 1993. The children affected by the expansion of prekindergarten are too young to appear in my data so cannot affect the results. While expanded access to technology in high school could theoretically increase completed education of the affected cohorts by improving their schooling conditions, rigorous evaluations of the impact of computer technology on academic achievement show zero to negligible effects (Guryan and Goolsbee 2006; Angrist and Lavy 2002).

The Arkansas scholarship program was the capstone of an effort by Governor Bill Clinton to reform education in his state. During his two terms as governor, the state's system of school finance was overhauled and standardized testing was expanded. The cohorts I examine in the paper were 5-17 years old at the start of Clinton's tenure in

---

12. Cornwell, Lee, and Mustard (2004) conclude that HOPE has led students to take light course loads in order to keep a high GPA. Students bear most of the cost of such a strategy, since it increases the opportunity cost of a college degree. If HOPE does slow progress through college, I will underestimate its lifetime impact on college completion, since the exposed cohorts are still relatively young. In principle, I can estimate the effect of HOPE on time-to-degree by calculating age-specific program effects. In practice, these age-specific effects cannot be disentangled from year-specific effects induced by the aging of the program and multiple changes in the program rules.

13. Until 2001, HOPE scholarships were offset by other sources of aid. This will tend to reduce the Georgia program's impact among low-income students, who receive a disproportionate share of aid.

14. Dynarski (2000) shows that public tuition and fees rose slightly faster in Georgia than in the rest of the United States after HOPE once introduced, after lagging U.S. tuition growth in the preprogram period. In a more detailed analysis, Long (2004) also concludes that the HOPE program had a small positive effect on prices.

1983; the first cohort eligible for the merit scholarship was 10. All of the Arkansas birth cohorts used in the analysis were therefore potentially exposed to the Clinton K-12 reforms, though the length of their exposure varied. If these reforms positively affected educational attainment, they would manifest themselves as a smooth upward time trend in the completed education of those born in Arkansas. As I discuss in the next section, the paper's estimates are identified by sharp changes in the completed education of adjacent age cohorts. The inclusion of state-specific trends does not substantively affect the estimates.

III. Empirical Methodology

I use a treatment-comparison research design. States that never introduce merit programs, or introduce them after the period under analysis, constitute the comparison group. I will test the sensitivity of the results to the choice of comparison states. Relative changes in educational attainment in the states that introduce merit aid identify the effect of merit aid.

The key variable of interest is an indicator variable, *merit*, that is set to one for an individual who would have been exposed to a merit aid program at high school graduation. For example, those who graduated from high school in 1991 and after in the state of Arkansas were exposed to that state's scholarship program. Those who graduated before 1991 were not eligible; the program was not grandfathered. Similarly, those who graduated in 1993 and after in Georgia were exposed to that state's scholarship program.

The census provides neither state nor year of high school graduation. I use age at the time of the census survey to assign the year of high school graduation, which I assume to occur at age 18. For example, an individual who was 27 during the Census survey in April 2000 was 18 in April 1991, and so would be assigned to be a high school senior in 1991. This is an imperfect proxy, because in the spring of their senior year many high school students are older or younger than 18. Information on quarter of birth would allow a more accurate assignment, but this variable is unavailable in the 2000 Census. The main estimates of the paper do not account for this source of measurement error; as I show later in the paper, it appears to bias estimates downward by roughly 10 percent.

I use state of birth to assign state of high school attendance. For this purpose, state of birth is preferred over state of residence because the latter may be endogenously determined, with individuals migrating to college and then settling in the state in which they go to school. A drawback of using state of birth as a proxy for state of high school attendance is that about 20 percent of high-school-age youth live outside their state of birth. This classification error will tend to bias the estimates downward. Again, the main estimates of the paper do not account for this source of measurement error; as I show later in the paper, it appears to bias estimates downward by roughly 25 percent.

A. Sample Definition

My analytical sample consists of 22- to 34-year-olds in the 2000 census. The lower cutoff is chosen because it is a traditional age of college-leaving, and so is a

reasonable age at which to begin measuring degree completion. The upper cutoff is more arbitrary, and is chosen to provide roughly equal numbers of age cohorts graduating high school in the years preceding and following the introduction of the programs. These age cohorts correspond to the high school classes of 1984 through 1996.

I limit the sample to those who currently live in, and were born in, the United States.[15] Observations for which age, state of birth, or completed education is imputed are dropped from the analysis. Analyses that include the imputed values produce similar results. I do not use the census sample weights, which again does not substantively affect the results.

B. Variable Definitions

The Census does not collect data on years of schooling at the college level, instead capturing information about degree completion. Due to these data constraints, I cannot measure the impact of the programs on years of completed schooling, and I will instead focus on whether an individual has earned any college degree, including a two-year associate's degree, or AA. Additional results will show the impact of the merit aid programs at every level of education by estimating treatment effects for all 16 categories of the census education variable.

Measures of race and ethnicity are included in some specifications. Mutually exclusive indicator variables define individuals as Hispanics of any race, Black non-Hispanics, white non-Hispanics, and other non-Hispanics. Table 2 contains the means of the key variables, listed separately by the individual's state of birth—Arkansas, Georgia, the rest of the South, and the rest of the United States.[16]

C. Specification

In the most parsimonious specification, I regress educational attainment against the treatment dummy and a set of state of birth and age effects. I estimate the following equation using Ordinary Least Squares:

$$(1) \quad y_{iab} = \beta merit_{ab} + \delta_a + \delta_b + \varepsilon_{iab}$$

Here, y_{iab} is a measure of the completed education of person i of age a born in state b. δ_b and δ_a denote state of birth and age fixed effects, respectively, and ε_{iab} is an idiosyncratic error term. The identifying assumption of this equation is that any relative increase across age cohorts in the schooling of those born in merit aid states is attributable to the merit program itself. If the identifying assumption is correct, β is the increase in education associated with exposure to a merit aid program.

---

15. Mississippi's program, introduced in 1996, is old enough to affect two-year degree completion in 2000 but too young to affect four-year degree completion. To simplify the interpretation of the results I have removed Mississippi from the sample.
16. The South consists of the South Atlantic states (Delaware, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia, West Virginia, and the District of Columbia); the East South Central states (Alabama, Kentucky, Mississippi, and Tennessee); and the West South Central states (Arkansas, Louisiana, Oklahoma, and Texas).

Table 2

Sample Means, by State of Birth, 22- to 34-year olds, Census 2000,
One Percent Sample

	Arkansas	Georgia	Rest of South	Rest of U.S.
Any college experience	0.495	0.517	0.551	0.606
Any college degree	0.223	0.260	0.282	0.337
Associate's degree only	0.058	0.060	0.068	0.083
Bachelor's degree or above	0.165	0.200	0.214	0.254
Non-Hispanic white or Asian	0.786	0.682	0.707	0.783
Unemployment rate in state of birth, at age 18	0.072	0.056	0.066	0.064
N	3,467	9,225	97,321	332,347

Notes: Means are unweighted. Observations with imputed values of education, age, or state of birth are dropped. Unemployment rate is that of young people, from Bureau of Labor Statistics. Mississippi, which introduced a merit program in the middle of the period under analysis, is dropped from the sample.

I collapse the data into state-age means before running the estimating equation:

$$(2) \quad \bar{y}_{ab} = \beta merit_{ab} + \delta_a + \delta_b + \varepsilon_{ab}$$

With the means weighted by their cell size in the regression, Equation 2 produces coefficient estimates mathematically identical to those obtained in the individual-level regression of Equation 1. I adjust the standard errors for serial correlation within state. Monte Carlo simulations in Bertrand, Duflo, and Mullainathan (2004) show that this approach produces hypothesis tests of the correct power.[17]

IV. Results and Robustness Checks

I first graph the data that provide the identifying variation in the paper. Figure 1A separately plots the probability of degree completion in Arkansas and the rest of the United States (excluding Georgia). Because this is a single cross-section, the graph reflects both time and age variation in education, with age effects dominating among the youngest cohorts. Education rises steadily among those in their twenties. In the United States, the share holding a college degree rises from 17.5 percent among 22-year-olds to 37.7 percent among 29-year-olds. College completion is considerably lower in Arkansas than in the United States; among pre-program cohorts, the gap averages 13 percentage points.

The shape of the U.S. age-education profile provides the counterfactual for Arkansas. Among the preprogram age cohorts, Arkansas roughly tracks the United States. The series is noisy but essentially flat for these cohorts, though the Arkansas series is

17. In an individual-level analysis of a state-level treatment, standard errors can be misleading for two reasons: (1) the treatment does not vary across individuals, but across states and time, and (2) there may be serial correlation in the error term within states. See Bertrand, Duflo, and Mullainathan 2004.
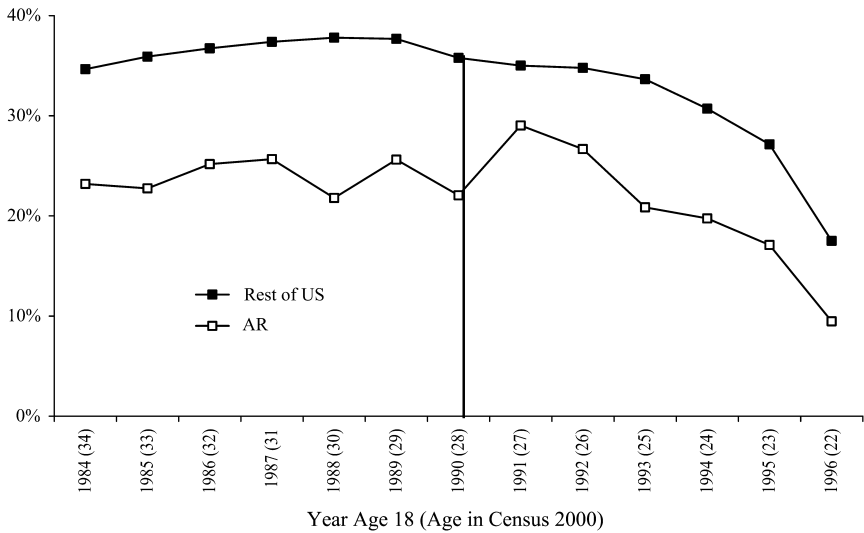
Figure 1A

Proportion Holding a College Degree, by Age, Born in Arkansas vs. Born in Rest of United States. Vertical line indicates last preprogram year (1990).

(unsurprisingly) noisier than that of the United States. With the first post-program cohort, there is a marked convergence in the two series. The gap between Arkansas and the United States narrows by more than seven percentage points between the two cohorts that straddle the introduction of the scholarship. After this sharp convergence, Arkansas cohorts again roughly track their United States counterparts. During the post-program era, the gap between the United States and Arkansas averages nine percentage points. We see a similar dynamic at work in Georgia, shown in Figure 1B. The gap between Georgia and the United States is roughly nine percentage points among the preprogram cohorts. Starting with the first post-program cohort, we see a convergence between the two series, with the gap closing to about five percentage points.

In both of these figures, the sharp divergence from trend in the first program year supports the identifying assumption of the paper. In the regression analysis, I will explicitly test for this break from trend by controlling for linear and quadratic trends in age at the state level.

Table 3 presents the results of estimating Equation 2. The coefficient of 0.0298 in Column 1 suggests that the merit programs increased the share of the population earning a college degree by 2.98 percentage points. To give a sense of the magnitude of this estimate, note that the preprogram level of degree completion was about 24 percent in Georgia and Arkansas. The coefficient is precisely estimated, with a standard error of 0.40 percentage points.
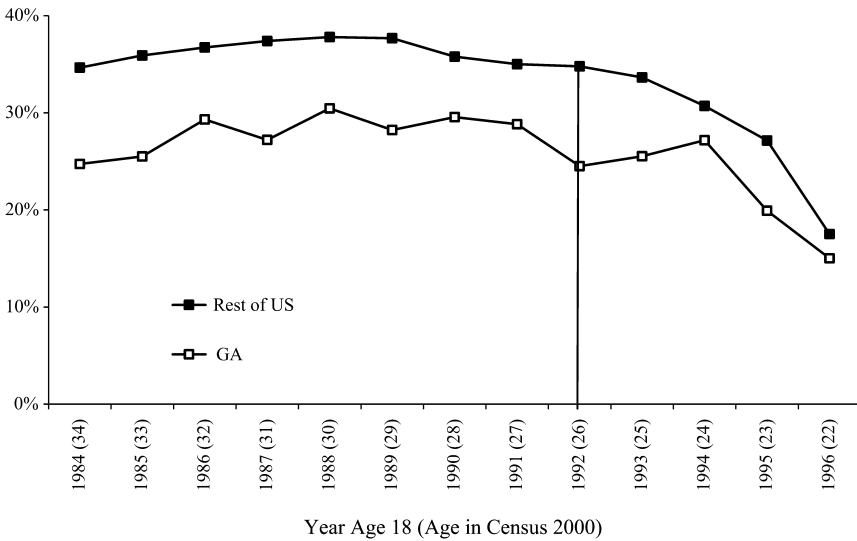
Figure 1B
Proportion Holding a College Degree, by Age, Born in Georgia vs. Born in Rest of United States. Line indicates last preprogram year (1992).

A. Controlling for State-Specific Labor Market Shocks and Other Covariates

In the classic human capital model (Becker 1994), educational attainment is a function not only of direct costs but also of opportunity costs and returns to schooling. A tight labor market will increase the opportunity costs of college, which will tend to reduce the share of young people completing a degree. A booming labor market also may boost the income of parents of college-age children, which in the presence of liquidity constraints will tend to increase the share of young people completing a degree. The same labor market shocks will also boost tax revenues and render a state more likely to introduce a merit aid program. High returns to schooling will also tend to keep people in school, and also may induce parents to pressure politicians to fund scholarships. These correlations will tend to bias my estimates of the causal impact of the merit scholarships.

   To test whether labor market conditions are biasing the estimates, I add a set of control variables to the basic specification.[18] First, I control for the state unemployment rate, measured in the respondent's state of birth in the year in which he was 18 years old. This variable is intended to capture both opportunity costs and the financial situation of families. Second, I control for the college wage premium among prime-wage workers, again measured in the respondent's state of birth in the year

---

18. Mechanically, I control for these covariates as follows. I regress the merit dummy and outcome variable against the covariates and form residuals. I collapse these residuals into cell means at the level of state of birth and age and reestimate Equation 2 with these cell means as the unit of observation. This produces the same point estimates as running Equation 1 in the microdata with covariates.

Table 3
OLS Estimates of Effect of Merit Aid Programs on College Degree Attainment

	(1) Baseline	(2) Labor Market Characteristics	(3) Other Post- secondary Education Spending	(4) Family Income And Demographics
Merit aid program	0.0298	0.0309	0.0276	0.0264
	(0.0040)	(0.0042)	(0.0037)	(0.0038)
Age fixed effects	Y	Y	Y	Y
State of birth fixed effects	Y	Y	Y	Y
Cohort size, unemployment rate, and college premium		Y	Y	Y
Public spending on higher education			Y	Y
Median family income, sex, race, ethnicity				Y
N	650	650	611	611

Notes: All regressions are at the level of state-of-birth by age cell means. Regressions are weighted by cell size and standard errors adjusted for serial correlation within states. In Columns 3-4, cell means are of residuals from regressions that include the listed covariates. See text for covariate definitions.

in which he was 18 years old.[19] This is intended to capture the young person's expected returns at the time he is making the decision to enroll in college. Third, I control for cohort size in an individual's state and year of birth. Cohort size may affect educational attainment through capacity constraints in education (Bound and Turner 2004). Results are in Column 2 of Table 3. After controlling for these measures of labor market conditions the estimate rises imperceptibly, from 2.98 to 3.01, with little loss in precision.[20]

Next, I examine whether other schooling inputs are changing systematically in states that introduce merit scholarship programs. If states that introduce these programs are increasing other spending on higher education, we may overestimate the effect of the merit programs. I therefore control for a state's per-student appropriations to its public colleges and universities.[21] The estimates are quite stable and precise, at 2.76 (0.37) percentage points.

Income may be rising in states with merit programs, with parents investing more in their children throughout their lives. I therefore add to the regression median household income in the state (at the time a person was 18) as a proxy for parents' ability to invest in their children's human capital.[22] Finally, I include controls for race and ethnicity, and the interaction of these variables with a sex dummy. The estimated program effect (Column 4) is quite stable, at 2.64 percentage points, with a standard error of 0.38.

B. Robustness to Definition of Comparison States

Labor market conditions and population characteristics may be shifting in unobserved ways in the South relative to the rest of the country. Over the past few decades, relative income and education have been rising in the South. The program effects estimated so far may simply reflect a secular convergence of the South with the rest of the country. If this is the case, then the South is a better counterfactual than the United States for Arkansas and Georgia. I therefore limit the sample to those with a Southern state of birth and reestimate Equation 2. The result is in Column 2 of Table 4. The estimate is quite stable at 2.78 percentage points, though the standard error doubles to 0.79 percentage points.

In the next column, I define the comparison group as any state that had introduced a merit program by 2003; this includes the non-Southern states of Michigan, Nevada, and New Mexico. The resulting estimate is 2.64, with a standard error of 0.48 percentage points. In the last column, I again limit the sample to states that ever

---

19. I use 35- to 54-year-olds in the1984-96 March CPS to estimate these college premia. The premium is defined as the difference between the mean log wages of year-round, full-time workers with a high school degree and a BA.

20. I have also interacted all of the covariates with age, which flexibly captures any changes over time in the effect of variables on schooling decisions. The results are quite similar to those shown in the table.

21. Appropriations to public colleges and universities are measured when an individual is 18. I am grateful to Sarah Turner for the appropriations and enrollment data (which she obtained from Grapevine and IPEDS, respectively). Alaska, Hawaii, and the District of Columbia are excluded from the specification that includes appropriations data.

22. Statistics on state median household income are Census Bureau estimates based on Current Population Survey data; they were downloaded from http://www.census.gov/hhes/income/histinc/h08.html on November 8, 2005.

Table 4
Sensitivity of Results to Choice of Comparison States

	United States	Southern States	States that Ever Introduce Merit Aid	Southern States that Ever Introduce Merit Aid
	(1)	(2)	(3)	(4)
Merit aid program	0.0298	0.0278	0.0264	0.0229
	(0.0040)	(0.0079)	(0.0048)	(0.0060)
Age fixed effects	Y	Y	Y	Y
State of birth fixed effects	Y	Y	Y	Y
N	650	208	156	117
Mean of Y	0.33	0.28	0.28	0.27

Notes: Cell-mean regressions, weighted by cell size. Standard errors adjusted for correlation within state of birth.

introduce a merit scholarship but drop the non-Southern merit states from this analysis. The estimate is still positive and significant but drops to 2.29 percentage points, with a standard error of 0.60 percentage points.

C. Falsification Exercise

State of birth and state of residence are highly correlated. To make clear that the identification of the program effect is driven by state of birth, rather than current state of residence, Table 5 shows the results of reestimating the equations with state of *residence* determining treatment status. That is, current state of residence, rather than state of birth, is assumed to be the state in which a person graduated from high school. These estimates are very small and statistically indistinguishable from zero: 0.39 and zero percentage points when the entire United States and the South, respectively, are used as the comparison groups.

This result shows that the states with scholarship programs are not simply states to which the well-educated are migrating. This strongly supports the identification strategy of the paper. In fact, because the estimates of the paper indicate that the merit programs are inducing more young people to complete college, the null results of Table 5 necessarily imply that either highly educated workers are migrating out of merit states or relatively uneducated workers are migrating into the merit states. The scenario of an inflow of uneducated workers is consistent with Moretti (2004). He shows that a college-educated work force generates positive wage externalities for both skilled and unskilled workers; through this channel, a state with a growing share of college-educated workers can be a magnet for unskilled labor.

Whether those induced to complete college remain in the state in which they are educated is important from the state's perspective: It is quite different to educate a

Table 5

Falsification Exercise: Assign Treatment Status Based of State of Residence

	United States		South	
	(1)	(2)	(3)	(4)
	State of Birth	State of Residence	State of Birth	State of Residence
Merit aid program	0.0298	0.0039	0.0278	0.0000
	(0.0040)	(0.0149)	(0.0079)	(0.0176)
Age fixed effects	Y	Y	Y	Y
State of birth fixed effects	Y		Y	
State of residence fixed Effects		Y		Y
N	650	650	208	208

Note: Regressions are at the level of cell means. Regressions weighted by cell size. Cell means are defined by age and state of birth in Columns 1 and 3 and by age and state of residence in Columns 2 and 4. Standard errors adjusted for correlation within state of birth or residence.

college graduate and have her leave than to have her stay and produce positive externalities. I will explore endogenous migration in future work; here I focus on the efficacy of higher education policy in getting more people to complete college, however mobile those workers may be after graduation.

D. Underlying Trends in Educational Attainment

The analysis so far has controlled for observable differences between the treatment and comparison states. Any unobserved differences between the treatment and comparison states that are fixed over time will be absorbed by the state-of-birth fixed effects and will not bias the estimates. But any *changes* in the populations and economies of Arkansas and Georgia that do not also occur in the comparison states are a threat to the internal validity of the estimates. Because the industrial composition of the South has been shifting substantially over the last several decades, and there has been a steady migration of the U.S. population southward, the assumption of fixed differences between the treatment and comparison states may well be invalid.

I informally evaluated this threat to validity with Figures 1A and 1B, which indicate a sharp break for the affected cohorts. A more formal approach is to control parametrically for state-specific trends in college completion. A typical method is to include linear time trends in the regression and identify the program effect with deviations from those trends. Figures 1A and 1B make clear, however, that the counterfactual trend is not linear, but quadratic. I test both the linear and quadratic functional forms, as well as a nonparametric approach. As discussed below, all of these strategies produce similar point estimates but large loss of precision.

Table 6

Robustness Check: Linear, Quadratic, and Nonparametric Controls for Trends

	Baseline	State of Birth X Age Trends		Census Division X Age Effects	State of Residence X Age Effects
		Linear	Quadratic	Nonparametric	Nonparametric
	(1)	(2)	(3)	(4)	(5)
United States	0.0298	0.0221	0.0216	0.0235	0.0416
	(0.0040)	(0.0136)	(0.0209)	(0.0145)	(0.0120)
South	0.0278	0.0245	0.0344	0.0235	0.0318
	(0.0079)	(0.0183)	(0.0235)	(0.0149)	(0.0140)

Notes: Regressions are at the level of cell means. Regressions are weighted by cell size and standard errors are adjusted for correlation at the state level. All regressions include state-of-birth and age fixed effects. Specification in Column 2 includes a separate linear trend in age for each state of birth. Specification in Column 3 includes a separate quadratic trend in age for each state of birth. Column 4 includes a full set of age-effect X division-of-birth interactions. Column 5 includes a full set of age-effect X state-of-residence interactions.

I first add to the baseline regression a linear term in age, interacted with the state of birth dummies:

$$(3) \quad \bar{y}_{ab} = \beta merit_{ab} + \lambda_b age_{ab} + \delta_a + \delta_b + \varepsilon_{ab}$$

This specification allows each state a linear age trend in degree completion; the program effect is identified by deviations from these trends. Results are in Column 2 of Table 6. For the U.S. sample, the point estimate drops slightly (from 2.98 to 2.21 percentage points) but the standard error more than triples. A similar pattern holds for the Southern census region, where the estimate drops to 2.45 percentage points and the standard error rises to 1.83.

I next add the square of age, again interacted with state of birth:

$$(4) \quad \bar{y}_{ab} = \beta merit_{ab} + \lambda_b age_{ab} + \eta_b age^2_{ab} + \delta_a + \delta_b + \varepsilon_{ab}$$

As shown in Column 3, the estimate based on the U.S. sample is essentially unchanged, while the Southern sample estimate rises to 3.44 percentage points. The standard errors rise yet higher and the estimates are not statistically significant.

The approaches just discussed impose a functional form on the underlying trends. Including a separate set of age effects for each geographic area, by contrast, allows the age-education profiles to take whatever forms the data suggest. In Column 4, I add to the regression the interactions of age with the nine census divisions of birth. Arkansas and Georgia are in separate divisions, so separate counterfactual age profiles are estimated for each of these treatment states. Using either the U.S. or Southern samples, the resulting estimate is 2.35 percentage points, with a standard error of about 1.5 percentage points, representing a small change in the point estimate but a large loss in precision.

While in a single cross-section I cannot control for the interaction of *state of birth* with age effects and still identify the program effect, I can control for the interaction of *state of residence* with age. With this approach, those who reside in their state of birth identify the state-specific age-education profiles, while those who live outside of their state of birth identify the program effect. Results are in Column 5. The estimates rise slightly and are statistically significant: 4.16 percentage points for the United States and 3.18 for the Southern sample.

With data from two points in time, we can include the interaction of state of birth with age while still identifying the program effect. I therefore pool the 1990 and 2000 censuses and add to Equation 2 the interactions of state of birth with age effects, which controls for any state-of-birth-specific idiosyncrasies in the shape of the age-education profile that are persistent over time. I also include the interaction of census year and age, thereby controlling for changes over time in the shape of the age-education profile that are common across the states. For the United States, the point estimate (standard error) is 2.2 (0.39) percentage points, while for the South alone it is 2.7 (0.74) percentage points.[23] I also have tested adding the interactions of census year and state of birth to the regression. In this specification, the program effect is identified by the triple interaction of state of birth, age effects, and year. The resulting estimates are larger but substantially less precise: 3.50 (2.05) for the United States and 3.32 (2.18) for the South. That the estimates rise with the addition of these variables indicates that degree completion grew more slowly in Arkansas and Georgia between 1990 and 2000 than in the comparison states, with only the cohorts exposed to the merit programs gaining on their counterparts in the comparison states.

E. Accounting for Classification Error in Treatment Status

The analysis so far has used state of birth and age to assign eligibility for a merit aid program, with each individual imputed to be eligible with probability one or probability zero. There are two sources of error in this method of assignment to treatment status. First, in the 2000 census, 24 percent of high school students lived outside their state of birth. Assuming this rate has not changed substantially over time, my assignment of eligibility for merit aid is incorrect for about 24 percent of the analytical sample. Second, many students are younger or older than 18 in the spring of their senior year, typically ranging in age from 17 to 19. For those high school seniors who were younger or older than 18 at the time a merit program was introduced in their state, the paper has incorrectly imputed eligibility. In this section of the paper, I attempt to correct for both of these sources of error in the assignment of treatment status.

I first address misclassification due to interstate migration. By using state of birth as a proxy for state of high school attendance, I have so far ignored the information provided by current state of residence. I now predict state of high school attendance with state of birth and use this predicted value to assign treatment status. That is, I allow treatment status to be a probabilistic (rather than deterministic) function of state of birth. To implement this strategy, I use high-school-age youth (15- to 17-years-old) in the 2000 census to estimate a matrix of transition probabilities between state of birth and state of high school attendance. In principle, this matrix could

---

23. These standard errors are clustered at state of birth and census year, which is the unit of observation at which we would be concerned about autocorrelation.

have 51 X 51 cells. However, because attendance in only two states (Arkansas and Georgia) produces a treatment, it is more efficient to estimate a matrix with dimension 51 X 2, corresponding to 51 states of birth and high school attendance in Georgia or Arkansas. Specifically, I run Ordinary Least Squares (OLS) regressions of the following form, where i indexes individuals, b indexes state of birth and I is an indicator variable:

$$I(state\ of\ residence = AR)_{ib} = \delta_b + \varepsilon_{ib}$$

$$I(state\ of\ residence = GA)_{ib} = \mu_b + v_{ib}$$

I apply the resulting predicted probabilities to the older sample (22- to 34-year-olds) to yield predicted state of high school attendance. The resulting predicted probabilities are used to define the treatment variable, which now ranges from zero to one. I then rerun Equation 2. The treatment variable is now defined as the sum of the interactions of two predicted probabilities with age dummies, for example, for person of age a born in state b:

$$merit_{ab} = \Pr_b(high\ school\ in\ AR) \times I(a < = 18\ in\ 1991)$$
$$+ \Pr_b(high\ school\ in\ GA) \times I(a < = 18\ in\ 1993)$$

The point estimate (standard error) rises from 2.98 (0.40) to 3.74 (0.54) percentage points. This is quite close to the error-corrected estimate that would be appropriate if migration to and from the merit states were random. Aigner (1973) and Freeman (1984) show that the relationship between the true coefficient and the estimate in the presence of classification error is:

$$\beta^* = \frac{\hat{\beta}}{1-\delta}$$

where $\hat{\beta}$ is the coefficient estimated in the presence of measurement error and δ is the degree of classification error.[24] Nationwide, 24 percent of high school seniors live outside their state of birth, so the baseline estimate of 2.98 corresponds to a error-corrected estimate of 3.92 (=2.98/0.76). Measurement error in state of high school graduation has a substantial impact on the magnitude of the estimate, biasing it downward by about 25 percent.

I next account for measurement error in the year of high school graduation. I do so by allowing treatment status to be a probabilistic function of age, using the 1989-91 School Enrollment Supplements of the October CPS to estimate age-specific probabilities of being a high school senior for those age 16 through 24.[25] I use these

24. This result may seem obvious, since classical measurement error is known to produce attenuation in regression coefficients. However, measurement error in binary variables is nonclassical: If a zero is observed, the measurement error can only be nonnegative and if a one is observed the measurement error can only be nonpositive.
25. The questions needed to determine whether a person is enrolled for a high school senior are available for these ages only. I constrain the sum of these probabilities to equal one, so that the estimate is corrected only for the timing of when an age cohort was a high school senior. Allowing the probabilities to sum than less than one would inflate the estimates by the inverse of the share of a birth cohort that attains the senior year of high school.

probabilities to impute for the analytical sample the probability of being a senior in each of the years from 1984 through 2000. The resulting predicted probabilities are used to define the treatment variable, which now ranges from zero to one, and the key estimating equation is rerun. The treatment dummy is now defined as the sum of the interactions of two predicted probabilities with state of birth, for example, for person of age a born in state b:

$$merit_{ab} = I_b(\text{born in Arkansas}) \times Pr_a(\text{HS senior in 1991+})$$
$$+ I_b(\text{born in Georgia}) \times Pr_a(\text{HS senior in 1993+})$$

The resulting estimate is 3.33 (0.57) percentage points; measurement error in year of high school graduation has relatively little impact on the estimates, biasing them downward by about 10 percent.

When I allow treatment status to be a probabilistic function of *both* state of birth and age, the estimated program effect is 4.21 (0.57) percentage points. The combined impact of measurement error in state of birth and year of high school graduation is to bias estimates downward by 1.23 percentage points (=4.21-2.98).

V. Heterogeneity in Program Effects

The robustness checks of the previous section establish a strong case for causal interpretation of the paper's estimates. The estimates indicate that the merit aid programs increased the share of the young, working age population receiving a college degree by about three percentage points. Having laid out the case for causality in the estimated effects, in this section I identify the margins of behavior and populations that respond most strongly to the scholarship programs.

A. Levels of Schooling Affected by the Program

I start by examining how margins other than college degree completion react to the programs. This exercise is of interest for two reasons. First, it is a check on the identification strategy, in that it allows us to confirm that there is no significant change in education at levels unaffected by the policy. Second, it allows us to hypothesize about the marginal student whose behavior is affected by the program.

I pinpoint changes in the full distribution of schooling using the methodology of the preceding section. I create a set of indicator variables, each indicating that an individual's level of education is greater than or equal to schooling category j and create means of these variables for each state-of-birth by age cohort:

$$(5) \quad \bar{y}_{ab}^j = E(educ \geq j)_{ab}$$

I then estimate program effects (β^j) for each of these j outcomes.

$$(6) \quad \bar{y}_{ab}^j = \beta^j merit_{ab} + \delta_a^j + \delta_b^j + \varepsilon_{ab}^j$$

These coefficients, along with their point-wise 95 percent confidence intervals, are plotted in Figure 2. Each point represents the estimated program effect on the
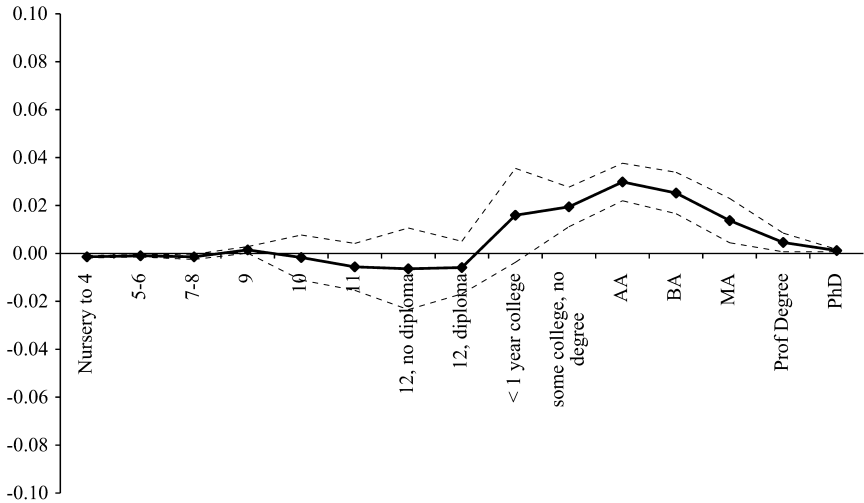
Figure 2
*Estimated Effect of Merit Aid of Full Distribution of Education. Plotted is
estimated effect of program on Pr(Educ>=X)*

probability of being *equal to or above* that level of education. The point above AA,
for example, is 2.98. This estimate, seen throughout in the paper, is the impact of the
program on the probability of receiving an AA or above (that is, a college degree).

For all precollege outcomes, the estimates are close to zero. Ex ante, we might have
expected an effect of the programs upon high school graduation, because they both re-
ward academic performance in high school and increase the option value of graduat-
ing.[26] However, the results suggest that those whose behavior is affected by these
incentives (those close to having a B average in high school) are not at the margin of
dropping out of high school and therefore are unresponsive to the programs.

The first positive and large estimate appears at the college entry margin, with a sta-
tistically insignificant estimate of 1.59 percentage points.[27] There is a yet larger (and
significant) impact on persistence through college, with the share completing at least
some years of college rising by 1.94 percentage points. Relative to baseline, this is a
large effect: in the 2000 census, 9 percent of this age group had entered college without

26. High school grades have risen in Georgia since its scholarship was introduced, which is consistent with
either increased effort in high school or grade inflation. Henry and Rubenstein (2002) document a steady
correlation between SAT scores and high school grades among entering Georgia college freshmen. They
argue that this unchanging relationship is evidence against grade inflation.
27. In previous work with the CPS, I have estimated a five to seven percentage point impact of the merit
programs on the contemporaneous college *attendance* rate of 18- to 19-year-olds (Dynarski 2000 and
2004). This result is not directly comparable to any of the present estimates, since the attendance rate con-
flates two outcomes: entry and persistence conditional upon entry. The contemporaneous CPS attendance
questions may capture short college spells forgotten by those answering retrospective Census questions.
Card and Lemieux (2001) note divergence between education of cohorts as measured by Census and
the CPS.

completing a single year. As discussed in detail in the previous section, there is a statistically significant effect of 2.98 percentage points upon degree completion.

There also is a statistically significant impact upon completing any education *beyond* the Bachelor of Arts (BA) (1.37 percentage points). There are several plausible explanations for a program effect beyond the BA. First, courses taken at the baccalaureate level often count toward a graduate degree, so completing a BA moves one closer to a Master of Arts (MA). In fact, accounting and nursing students at Georgia Southern University concurrently earn bachelor's and master's degrees, with HOPE paying for 150 credit hours of the combined course-load; the BA requires just 120 credit hours.[28] More generally, a simple model of human capital accumulation suggests that post-baccalaureate schooling decisions are a function of past schooling costs, predicting that a college graduate who paid less for her bachelor's degree will be more willing to borrow for a master's degree.[29] Finally, in the presence of liquidity constraints, a scholarship may cause students to work less and complete their education more quickly. In my data, I cannot rule out that the treated cohorts are simply completing their planned degrees at a quicker pace. If this is the case, the programs' effects on completed education will fade as the treated cohorts age.[30] However, even if the program effect completely dissipates as cohorts age, there will still be a positive welfare impact of the scholarships, because education completed earlier in life yields more years of private and social returns.

B. Implied Increase in Years of Schooling Produced by the Program

Translating these multiple effects into years of schooling requires assumptions about the years of college represented by each census category. If we assume that that those who enter college but earn no degree have one year of college; that those with an Associate in Arts (AA) have two years of college; and that those who earn a BA have four years of college, the implied impact of the program is an increase of 0.12 years of college. This calculation does not include the estimated increase in the share of the cohort earning a master's degree.

C. Heterogeneity Across Demographic Groups in Program Impact

I next turn to exploring heterogeneity across populations in the programs' effects. Treatment heterogeneity could be driven by a variety of factors that vary systematically across the population, such as preparation in high school, labor market opportunities, returns to schooling, parental education, and liquidity constraints. I capture the reduced-form impact of all of these channels. I estimate Equation 2 separately for four mutually exclusive groups: non-Hispanic white/Asian men, non-Hispanic white/Asian women, Hispanic and nonwhite men, and Hispanic and nonwhite women.

---

28. Thanks to Christopher Cornwell for drawing this to my attention. State legislators, arguing that HOPE was not intended to pay for graduate school, have voted to limit to 127 the credit hours paid by the program (Salzer 2005). A second HOPE provision, introduced in 1996, encourages graduate study: the state forgives graduate student loans of those who teach in Georgia elementary and secondary schools.

29. See Dynarski (2000) for the development of this model, which shows that future human capital investment will depend on the cost of past investment if the price of debt rises with its level.

30. In theory, I can test for fadeout of the program effect; in practice, it is difficult to discern such patterns from random noise and year-specific changes in program generosity.

Table 7

Heterogeneity in Treatment Effects By Race, Ethnicity and Sex

	(1) Any College Degree	(2) BA or above	(3) AA Only
Full sample	0.0298	0.0252	0.0046
	(0.0040)	(0.0044)	(0.0025)
White Non-Hispanic women	0.0316	0.0229	0.0087
	(0.0048)	(0.0050)	(0.0020)
Nonwhite and Hispanic women	0.0346	0.0082	0.0263
	(0.0214)	(0.0138)	(0.0082)
White Non-Hispanic men	0.0158	0.0193	-0.0035
	(0.0092)	(0.0050)	(0.0093)
Nonwhite and Hispanic men	0.0160	0.0279	-0.0120
	(0.0034)	(0.0047)	(0.0028)

Notes: Each coefficient represents a separate regression. Regressions are at the level of cell means. Regressions are weighted by cell size and standard errors are adjusted for correlation at the state level. All regressions include state-of-birth and age fixed effects.

I summarize the effects on degree completion in Table 7. For the entire sample, the effect is concentrated on the BA margin: 2.52 percentage points, as compared to 0.46 for the AA margin. Among nonwhite and Hispanic women, however, the AA margin dominates: the estimated effect is 2.63 percentage points for the AA as compared to 0.82 for BA or above. Among men, the estimated AA effect is negative, significantly so for nonwhites and Hispanics, indicating that the subsidies are shifting this group from AA receipt toward BA completion.

In Figures 3A through 3D, I provide greater detail by plotting the impact of the scholarships on each of the 16 census education categories, for each subgroup. This shows responses on margins not shown in Table 7. Hispanic and nonwhite women are most responsive (Figure 3B), with their college entry rate rising by six percentage points and their probability of completing at least some college rising by seven percentage points. They also exhibit large increases in their probability of receiving any college degree (3.46 percentage points). White, non-Hispanic women (Figure 3A) respond quite strongly, with their shifts concentrated at higher levels of education: the probabilities of completing any college degree and completing a BA increase by three and two percentage points, respectively. All of these estimates are highly significant.

Program effects are relatively muted among white, non-Hispanic men (Figure 3C), whose probability of receiving a BA rises by 1.93 percentage points. The results for Hispanic and nonwhite men are mixed and noisy (Figure 3D). There are precisely estimated increases in the probability of completing any college degree and at least
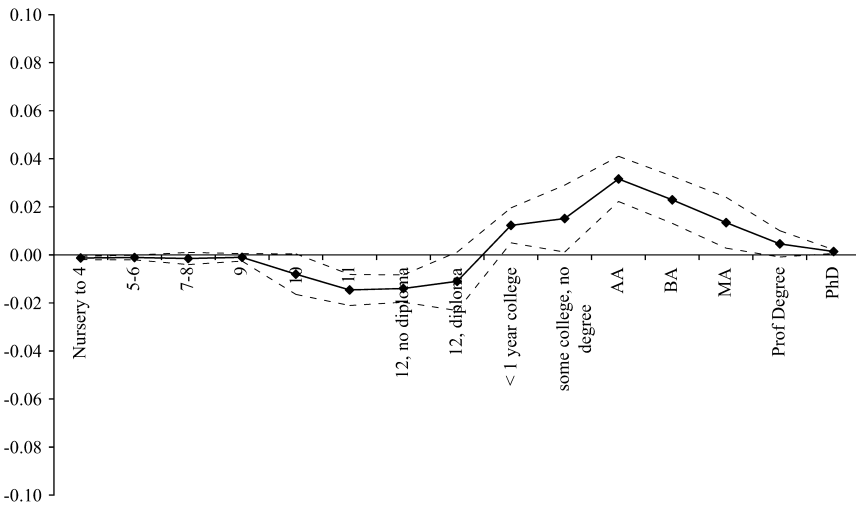
Figure 3A

Non-Hispanic White Women. Estimated Effect of Merit Aid of Full Distribution of Education. Plotted is estimated effect of program on Pr(Educ>=X).

a BA (1.60 and 2.79 percentage points, respectively), but there is also a large, insignificant drop in the probability that this group will complete high school. This could indicate that instructional resources are being shifted away from students on the margin of dropping out of high school, but this estimate is so imprecise we cannot draw firm conclusions.

The stronger results for women accord with previous evidence on the relative elasticity of male and female college attendance (Card and Lemieux 2001). As the return to college has risen over time, women have made far greater gains than men have in college completion rates. Between the late 1980s and the late 1990s, young women shot past their male peers in their college completion rate, with the share of recent high school graduates with a BA rising from 21 to 31 percent for women and from 24 to 26 percent for men. The female advantage in college-going is particularly pronounced among nonwhites.[31] Nonwhite and Hispanic men are more likely than others to drop out of high school, be incarcerated, or join the military, all of which will blunt the effect of any scholarship on this group's schooling decisions. Differential performance in high school explains gender differences in the effect of a merit scholarship, in particular. In course grades and standardized tests, girls outperform boys in high school and are substantially more likely to go on to college (Goldin, Katz, and Kuziemko 2005). Among members of the high school class of 1992 that

31. These statistics are for the high school classes of 1982 and 1992 and are drawn from High School and Beyond and NELS88, respectively. See National Center for Education Statistics (2005). For discussion of the gender gap in college, see Jacob (2002).
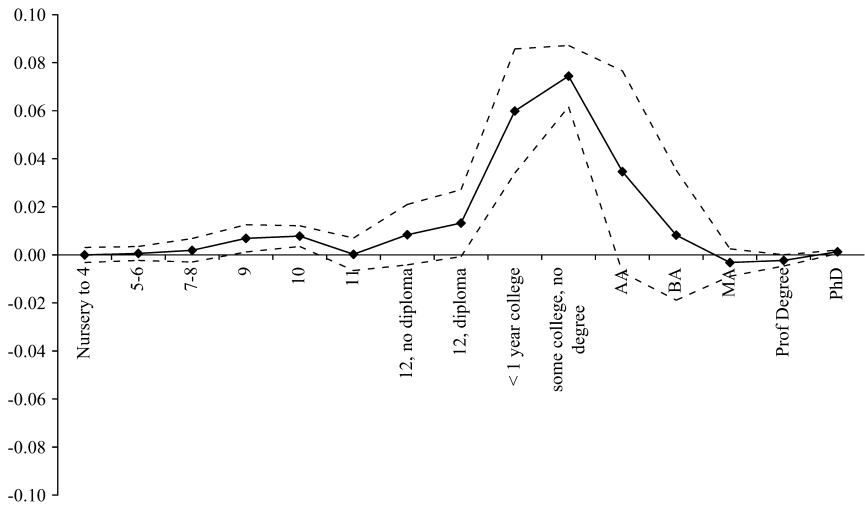
Figure 3B

Hispanic and Nonwhite Women. Estimated Effect of Merit Aid of Full Distribution of Education. Plotted is estimated effect of program on Pr(Educ>=X).

went to college, 49 percent of women had a high school GPA of at least 3.0, while just 36 percent of their male peers performed as well. As a result, fewer male than female high school graduates would have been eligible for the merit scholarships.

VI. Discussion

Together, these tables and figures provide strong evidence that the merit aid programs increased the completed schooling of eligible youth. Merit aid is estimated to increase the college entry rate by 1.6 percentage points, the share who complete any years of college by 1.94 percentage points, the share who complete any college degree by 2.98 percentage points, and the share who complete a BA or above by 2.52 percentage points. All but the first of these estimates are highly significant. All of these margins are plausibly affected by the merit aid programs, which decrease the cost of both entering and persisting through college.

A. How Does Merit Aid Affect Persistence in College?

The paper has measured the reduced-form impact of merit aid on completed schooling, which is the product of effects upon entry and persistence. I cannot separately identify the effect of merit aid on college entry and persistence conditional on entry, because I cannot identify the marginal entrant. We can place informative bounds on the size of the persistence effect, however. An upper bound is formed by assuming that *none* of those induced into college by the scholarships completes a degree.
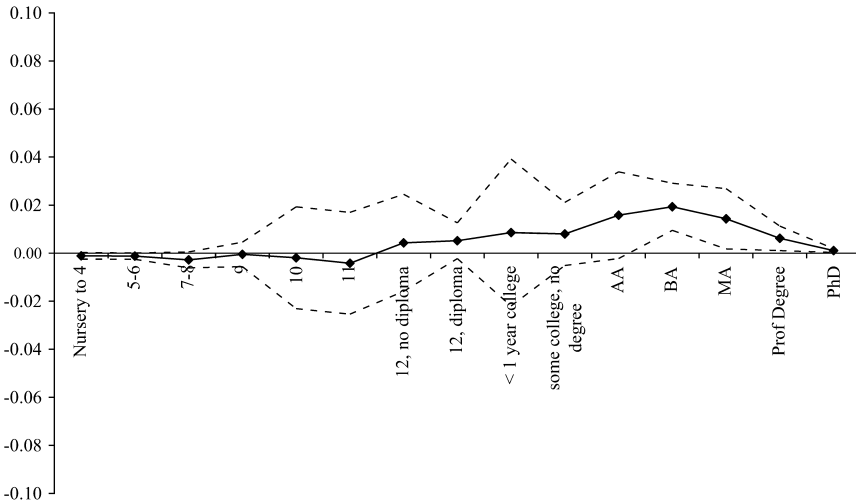
Figure 3C

Non-Hispanic White Men. Estimated Effect of Merit Aid of Full Distribution of Education. Plotted is estimated effect of program on Pr(Educ>=X).

The other bound is formed by assuming that *all* of those induced into college by the scholarships complete a degree. I calculate these bounds below, using the estimated effects from the previous section and data for preprogram cohorts in the treatment states, among whom 51.5 percent entered college and 26.7 percent completed a degree, leading to a baseline persistence rate of 51.8 percent (=26.7/51.5).

1. Scenario A: No student induced into college by the scholarship program completes a degree.

In this case, all of the 2.98 percentage point increase in degree completion must be explained by increased persistence among those who would have entered college even in the absence of the scholarship. The merit programs are then estimated to have increased the degree completion rate to 29.7 percent (=26.7+2.98). The persistence rate is therefore calculated to rise by 5.2 percentage points to 57.0 percent (=29.7/51.5), or by about 10 percent (=5.2/51.8).

2. Scenario B: Every student induced into college by the scholarship program completes a degree.

The programs are estimated to increase college entry by 1.6 percentage points. Thus, in this scenario, 1.38 percentage points (=2.98-1.6) of the increase in degree completion must be attributable to increased persistence of those who would have gone to college in the absence of the program. The program is estimated to increase their completion rate to 28.08 (=26.7+1.38) percent, which in turn implies an increase of
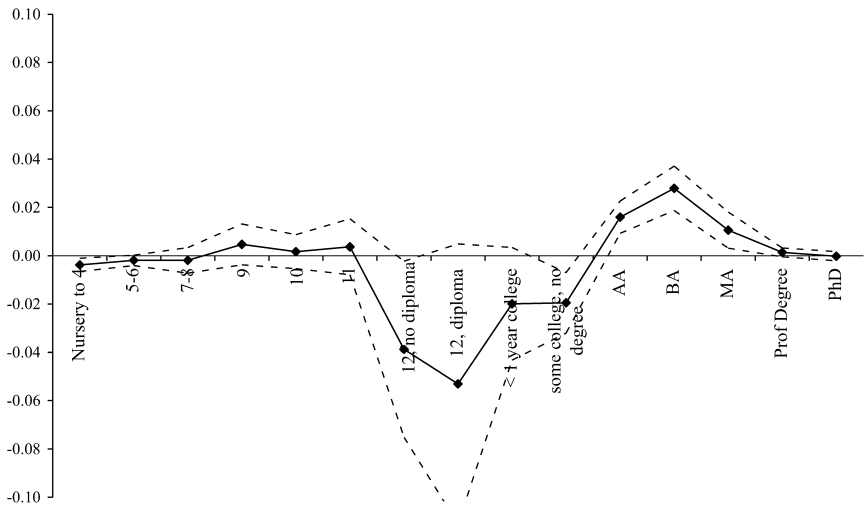
Figure 3D
Hispanic and Nonwhite Men. Estimated Effect of Merit Aid of Full Distribution of Education. Plotted is estimated effect of program on Pr(Educ>=X).

2.7 percentage point in persistence (from 51.8 percent to 54.5 percent [=28.08/ 51.5]), or about 5 percent (=2.7/51.8).

The scholarship programs are therefore estimated to increase persistence to degree, conditional on college entry, by 2.7 to 5.2 percentage points. Given a baseline persistence rate of 51.8 percent, this is equivalent to a proportional increase of 5–10 percent (or, equivalently, corresponds to a decrease in the college dropout rate of 6– 12 percent). If we assume that marginal entrants persist at the same rate as preprogram college students, the implied increase in the persistence rate for inframarginal college entrants is 4.3 percentage points.

B. External Validity of Estimates

Note that the paper's estimates reflect any incentive effect of the scholarships on academic effort in high school and college. They are therefore not directly comparable to estimates yielded from variation in price driven by, for example, Pell Grant eligibility. It is not clear whether the academic requirements of the programs will produce larger or smaller college completion effects than a nonmerit subsidy. The merit programs' academic requirements may push students to work harder in college, and thereby make them more likely to succeed. This may make these programs particularly effective at increasing degree receipt. Conversely, though, they may deny subsidies to many students who are on the margin of completing a college degree but whose grades are too low to maintain the scholarship. The programs require a 2.75 to 3.0 GPA in college, well above the GPA required to graduate. This may make the programs *less* effective in encouraging degree completion than one that is targeted at a lower point in the distribution of academic achievement.

VII. Cost-Benefit Analysis

While the Arkansas and Georgia programs appear to increase college entry and degree completion, most of the scholarship funds go to students who would have entered or completed college in the absence of the subsidy. The social welfare consequences of the program hinge on whether the benefits of the human capital created by the merit scholarships exceed their cost. Throughout the following cost-benefit analysis, when assumptions are necessary I err on the side of overestimating costs and underestimating benefits.

A. Assumptions

I assume a real discount rate of 4 percent. I assume $2,500 is the scholarship paid out to an enrolled eligible student.[32] Scholarships flowing to students whose schooling is unaffected by the scholarship can be treated as a transfer or a cost. I will calculate the cost-benefit ratio under both assumptions. I normalize the population to size one. All dollar amounts can therefore be interpreted as the expected, per-person cost of offering the scholarship to an entire birth cohort.

1. Scholarships paid to infra-marginal students

The expected scholarship cost is the present-discounted sum of four years of scholarship payments, weighted by the probability of receiving the scholarship in each year. The Beginning Postsecondary Students Survey collected transcript data on students entering college in 2001. These data suggest that 30 percent of a birth cohort would qualify for a first-year scholarship, 12 percent for the second year, 9 percent for the third, and 6 percent for the fourth year of college.[33] These decreasing rates of eligibility reflect both failure to meet to the GPA requirements and failure to persist through college. Each member of the cohort is therefore expected to receive a merit scholarship for 0.57 years (=0.3+0.12+0.09+0.06), even if the scholarship has no impact on schooling decisions. These scholarships are expected to cost $1,425 (=0.57 year*$2,500/year); with discounting, the figure is $1,403.

2. Scholarships paid for induced years of schooling

I calculated earlier in the paper that the programs increased schooling by roughly 0.12 years. Without discounting, this increase in schooling produces an expected scholarship cost of $300 (=0.12 year*$2,500/year). With discounting, the expected cost is $274.

---

32. The scholarships pay tuition and fees at public colleges in Georgia and up to $2,500 a year toward tuition and fees in Arkansas. Tuition and fees averaged roughly $2,500 in the two states at this time.
33. Half of entering freshmen have high school GPAs of 3.0 or above. In Census 2000, 60 percent of the sample goes to college. This implies that 30 percent of the sample (=0.5*0.6) would qualify for a merit scholarship for the first year of college. Based on college GPA data in BPS:2001, 40 percent of those who qualify in year one are expected to qualify for an award the second year, 30 percent for a third year, and 20 percent for a fourth year. These data are consistent with administrative data from Georgia. Dee and Jackson (1999) show that at Georgia Tech just 43 percent of students who won the scholarship the first year also received it the second year.

3. Additional costs to public for induced years of schooling

Most colleges subsidize the cost of educating their students. To meet these costs, public colleges receive substantial subsidies from the states, while private colleges are subsidized by endowments. Winston (1999) estimates that tuition and fees cover just 30 percent of the cost of educating a college student.[34] This implies that if tuition and fees are $2,500, the total cost of educating a college student for a year is roughly $8,333. Once tuition and fees are paid, the cost to the public of educating a college student for a year is therefore $5,833. For inframarginal schooling, this $5,833 is a sunk cost and does not count toward program costs. Schooling induced by the program is expected to increase these costs by $700 (=0.12 year*$5,833/year); with discounting, the figure is $640.

4. Excess burden induced by the taxes needed to pay for scholarships and subsidies

Taxes pay for the costs of (1), (2), and (3), and taxes induce deadweight loss. Based on estimates in Gruber and Saez (2002), the marginal deadweight loss of taxation of 0.245 (assuming an assuming an average state plus federal income tax rate of 0.33). Deadweight loss adds $568 to program costs.

5. Opportunity costs of those whose schooling is increased by the program

The program induces an expected loss of 0.12 years of labor. Earnings for those in their late teens and early twenties range from $16,400 for a high school graduate to about $20,000 for those with an AA (calculated from Census 2000). A back-of-the-envelope calculation yields expected opportunity costs of $2,160 (=0.12*$18,000). A more careful calculation weighs each increment of schooling by the appropriate opportunity cost and discounts the sum. Assuming that the annual opportunity cost for those induced to increase their completed education to S is the annual earnings of young people with completed education S-1 yields estimated opportunity costs of $1,969.

B. Total costs

These costs are summarized in Table 8. The total expected cost of the program is $4,854 per person exposed to the program. Eighty-four percent of the scholarships pay for schooling that would have occurred in the absence of the scholarship. But because opportunity costs are higher than scholarship costs, schooling induced by the scholarships accounts for about 60 percent of the *total* expected costs of the program. Earnings sacrificed by those induced into school by the scholarships account for $1,969 of these costs, and $274 takes the form of scholarships.

In order to pass a cost-benefit test, expected benefits of the program must equal or exceed $4,854 per person exposed to the scholarship program. Because the program is expected to increase schooling by 0.12 years, this translates into a cost of $40,450 (=$4,854/0.12) for each year of school induced by the program. If a single year of college increases (the present-discounted value of) lifetime earnings by at least

---

34. This average figure likely overstates the subsidy costs for marginal college graduates and entrants, who do not attend the selective institutions at which the subsidy is highest.

Table 8
Estimated Cost of Scholarship Programs. Present Discounted Values, 4 Percent Real Discount Rate

	Inframarginal Schooling	Induced Schooling	Total
Years of scholarship receipt, per person exposed to program	0.57	0.12	0.69
	Costs Weighted by Years of Scholarship Receipt		
Scholarship payments ($2,500/year)	$1,403	$274	$1,677
Additional public costs of college ($5,833/year)	—	$640	$640
Marginal deadweight loss of raising revenue (24.5 percent)	$344	$224	$568
Opportunity costs	—	$1,969	$1,969
Total expected cost	$1,747	$3,107	$4,854

Notes: Table shows the expected costs of offering the merit scholarship program to a birth cohort whose size is normed to one. Column 1 shows costs that would be incurred if the impact of the program on completed schooling were zero. Column 2 shows the additional costs incurred assuming the program effects are those estimated in the paper. See text for additional explanation.

$40,450, then the programs pass the cost-benefit test. Table 9 shows the breakeven rates of return implied by these costs and benefits. Expected lifetime earnings for someone with a high school degree are $414,000.[35] A wage return of $40,450 for this person implies a rate of return to schooling of 9.8 percent (=$40,450/$414,000). When we count scholarships paid to inframarginal students as a transfer, the breakeven rate of return for this additional year of schooling drops to 6.9 percent.

Breakeven rates of return are lower for higher levels of schooling, because baseline earnings are higher. A person with some college but no degree is expected to earn $490,000 from age 19 through 65. Given lifetime earnings of $490,000, an additional year of college would need to increase lifetime earnings by 5.9 percent (=$28,758/$490,000) if scholarships to inframarginals are counted as a transfer and 8.3 percent (=$40,450/$490,000) if they are counted as a cost. Analogous breakeven rates of return for those being shifted from an AA degree to a BA (which requires an additional two years of schooling, and so incurs higher costs) are 5.4 percent and 7.5 percent.

---

35. This is the discounted sum of average earnings for each single-year age group in Census 2000, and includes those who do not work or work part-time. Using this static age-earnings profile to calculate lifetime earnings implicitly assumes that there will be no productivity-related increase in earnings over the lifecycle for cohorts now entering the labor market. Since the better-educated typically have steeper age-earnings profiles, any such increase would tend to increase the return to education.

Table 9

Efficiency Analysis: Breakeven Rates of Return (ROR) to Schooling

Change in Schooling Induced by Scholarship	Added Years of Schooling	Lifetime Earnings, Lower Level of Schooling	Scholarships to Inframarginals Counted as **Transfer**		Scholarships to Inframarginals Counted as **Cost**	
			Program Cost	Breakeven ROR	Program Cost	Breakeven ROR
High school grad → some college	1	$414,000	$28,758	6.9%	$40,450	9.8%
Some college → AA	1	$490,000	$28,758	5.3%	$40,450	8.3%
AA → BA	2	$537,000	$57,517	5.2%	$80,900	7.3%

Notes: Costs are based on those calculated in Table 8. The present-discounted values of lifetime earnings are based on empirical age-earnings profiles in Census 2000 and are discounted using a real rate of 4 percent. See text for further explanation.

Because the program moved students along all three of these margins—from high school to some college, from some college to an AA, and from an AA to a BA, the breakeven rate of return to schooling for the entire program is a weighted average of the rates calculated above. Treating the scholarships to inframarginal students as a cost implies breakeven rates of return to schooling of 7.5 to 9.8 percent, while treating these scholarships as a transfer implies breakeven rates of return of 5.4 to 6.9 percent.

These breakeven rates are at the low end of instrumental-variable estimates of the rate of return to schooling. The rate of return to years of high school education has been estimated at 6–10 percent among those exiting high school in the mid-twentieth century (Angrist and Krueger 1991; Staiger and Stock 1997) and 13-15 percent for more recent cohorts (Oreopoulos 2005). Kane and Rouse (1995) and Card (1995) estimate the rate of return to a year of college at 9 and 13 percent, respectively. The breakeven rates are also below OLS estimates of the rate of return to schooling.

The calculations suggest that the merit scholarships increase social welfare. Benefits outweigh costs even if the rate of return to schooling for marginal students is relatively low. The balance tilts further in favor of the programs' benefits when we consider that nonmarket returns to schooling have been left out of the calculation. Recent empirical research shows that the private and social nonwage returns to schooling are substantial. Currie and Moretti (2003) conclude that college improves the health of offspring, while Moretti (2004) finds that college graduates produce positive wage externalities. Other authors have found that additional years of high school extend life (Lleras-Muney 2005), increase civic participation (Dee 2004 and Milligan, Moretti and Oreopoulos 2004), and reduce crime (Lochner and Moretti 2004).

VIII. Conclusion

While the college attendance rate has risen sharply over time, the share of the population that has completed college has stayed relatively flat. Given that a very high proportion of high school graduates currently attempt college, large increases in the stock of college-educated labor will have to operate through the intensive rather than the extensive margin, by adding more years to the schooling of those who enter college rather than drawing more into postsecondary education. This paper has provided strong evidence that subsidies to the direct costs of college are an effective tool for increasing college completion and persistence.

I find a large and significant impact of these subsidies on both degree receipt and college entry. The results are robust to the inclusion of covariates, including measures of labor market shocks. The inclusion of flexibly specified state-specific trends in education do not alter the conclusions. The results suggest that merit programs increase college degree attainment by three to four percentage points. This is a substantial effect, given that the baseline share of the affected population with a college degree was just 27 percent. The effects on schooling are strongest among women, with white, non-Hispanic women increasing degree receipt by 3.8 percentage points and the share of Hispanic and nonwhite women attempting or completing any years of college increasing by six and seven percentage points, respectively.

While my reduced-form estimation strategy cannot separately identify the effect of aid on entry and persistence, I estimate fairly narrow bounds on the persistence

effect. The merit aid programs appear to increase by 5–11 percent the probability of persistence to degree of those who would have gone to college in the absence of a merit aid program—that is, of inframarginal college entrants. A simple cost-benefit analysis concludes that the private benefits of the scholarship programs substantially outweigh their costs.

These results indicate that tuition policy can play a welfare-enhancing role in increasing the stock of college-educated labor. But it should be emphasized that, for the bulk of college students, the offer a scholarship with very low transaction costs is not sufficient to get them to complete a degree. Even with the offer of free tuition, a large share of students continue to drop out of college, suggesting that the *direct* costs of school are not the only impediment to college completion. The results indicate that more than tuition reduction is necessary to substantially increase the stock of college-educated labor. Candidate mechanisms are better preparation in elementary and secondary school, more intensive institutional supports in college, and funding that extends beyond direct costs to opportunity costs.

References

Angrist, Joshua. 1993. ''The Effect of Veterans Benefits on Education and Earnings.'' *Industrial and Labor Relations Review* 46(4):637–52.

Angrist, Joshua, and Alan Krueger. 1991. ''Does Compulsory School Attendance Affect Schooling and Earnings?'' *Quarterly Journal of Economics* 106(4):979–1014.

Angrist, Joshua, and Victor Lavy. 2002. ''New Evidence on Classroom Computers and Pupil Learning.'' *Economic Journal* 112 Issue 482 (October):735–65.

Autor, David, Lawrence Katz, and Melissa Kearney. 2005. ''Trends in U.S. Wage Inequality: Re-Assessing the Revisionists.'' NBER Working Paper 11627.

Becker, Gary. 1994. *Human Capital*. Chicago: University of Chicago Press.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. ''How Much Should We Trust Differences-in-Differences Estimates?'' *Quarterly Journal of Economics* 119(1):249–75.

Bettinger, Eric. 2004. ''How Financial Aid Affects Persistence,'' In *College Choices: The Economics of Where to Go, When to Go, and How To Pay for It*, ed. Caroline Hoxby, Chicago: University of Chicago Press.

Blumenstyk, Goldie. 1992. ''An Education Governor? Bill Clinton's Education Record Is Examined.'' *Chronicle of Higher Education* 38:34 April 29, p. A23.

Bound, John, and Sarah Turner. 2002. ''Going to War and Going to College: Did World War II and the G.I. Bill Increase Educational Attainment for Returning Veterans?'' *Journal of Labor Economics* 20(4):784–815.

———. 2004. *Cohort Crowding: How Resources Affect Collegiate Attainment*. University of Michigan Population Studies Center, Research Report No. 04–557.

Card, David. 1995. ''Using Geographic Variation in College Proximity to Estimate the Return to Schooling.'' In *Aspects of Labour Market Behaviour: Essays in Honour of John Vanderkamp*, ed Louis N. Christofides, E. Kenneth Grant, and Robert Swidinsky, 201–22. Toronto: University of Toronto Press.

———. 2001. ''Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems.'' *Econometrica* 69(5):1127–60.

Card, David, and Alan B. Krueger. 1992. ''Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States.'' *Journal of Political Economy* 100(1):1–40.

Card, David, and Thomas Lemieux. 2001. ''Dropout and Enrollment Trends in the Postwar Period: What Went Wrong in the 1970s?'' In *Risky Behavior among Youths: An Economic Analysis*. ed. Jonathan Gruber, Chicago: University of Chicago Press.

Cornwell, Christopher, and David Mustard. 2005. ''Merit-Based Scholarships and Car Sales.'' University of Georgia. Unpublished.

Cornwell, Christopher, David Mustard, and Deepa Sridhar. 2006. ''The Enrollment Effects of Merit-Based Financial Aid: Evidence from Georgia's HOPE Program.'' *Journal of Labor Economics* 24(4):761–86.

Cornwell, Christopher, Kyung Hee Lee, and David Mustard. 2004. ''Student Responses to Merit Scholarship Retention Rules.'' *Journal of Human Resources* 40(4):895–917.

Currie, Janet, and Enrico Moretti. 2003. ''Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings.'' *Quarterly Journal of Economics* 118(4):1495–1532.

Dee, Thomas. 2004. ''Are There Civic Returns to Education?'' *Journal of Public Economics* 88(9):1697–1720.

Dee, Thomas, and Linda Jackson. 1999. ''Who Loses Hope? Attrition from Georgia's College Scholarship Program.'' *Southern Economic Journal* 66(2):379–90.

Dynarski, Susan. 2000. ''Hope for Whom? Financial Aid for the Middle Class and Its Impact on College Attendance.'' *National Tax Journal* 53(3):629–61.

———. 2002. ''The Behavioral and Distributional Implications of Aid for College.'' *American Economic Review* 92(2):279–85.

———. 2003. ''Does Aid Matter? Measuring the Effect of Student Aid on College Attendance and Completion.'' *American Economic Review* 93(1):279–88.

———. 2004. ''The New Merit Aid,'' In *College Choices: The Economics of Where to Go, When to Go, and How To Pay for It*. ed. Caroline Hoxby.Chicago: University of Chicago Press.

Ellwood, David. 2001. ''The Sputtering Labor Force of the 21st Century: Can Social Policy Help?'' In *The Roaring Nineties: Can Full Employment Be Sustained?* ed. Krueger, Alan and Robert Solow. New York: Russell Sage.

Ellwood, David, and Thomas Kane. 2000. ''Who is Getting a College Education? Family Background and the Growing Gaps in Enrollment.'' In *Securing the Future*, ed. Sheldon Danziger and Jane Waldfogel. New York: Russell Sage.

FPG Child Development Institute, University of North Carolina: 2005. ''Early Learning, Later Success: The Abecedarian Study, Early Childhood Educational Intervention for Poor Children: Executive Summary.'' http://www.fpg.unc.edu/~abc/summary.cfm Accessed July 14, 2005.

Goldin, Claudia, Lawrence Katz, and Ilyana Kuziemko. 2006. ''The Homecoming of American College Women: The Reversal of the College Gender Gap.'' *Journal of Economic Perspectives* 20(4):133–56.

Goolsbee, Austan, and Jonathan Guryan. 2006. ''The Impact of Internet Subsidies in Public Schools.'' *Review of Economics and Statistics*. 88(2) May: 336–47.

Gruber, Jonathan, and Emmanuel Saez. 2001. ''The Elasticity of Taxable Income: Evidence and Implications.'' *Journal of Public Economics* 84(1):1–32.

Henry, Gary, and Ross Rubenstein. 2002. ''Paying for Grades: Impact of Merit-based Financial Aid on Educational Quality.'' *Journal of Policy Analysis and Management* 21(1):93–109.

Healy, Patrick. 1997. ''HOPE Scholarships Transform the University of Georgia.'' *The Chronicle of Higher Education*, November 7, p. A32.

Jacob, Brian. 2002. ''Where the Boys Aren't: Non-cognitive Skills, Returns to School and the Gender Gap in Higher Education.'' *Economics of Education Review* 21:589–98.

Kane, Thomas J. 1994. ''College Entry by Blacks since 1970: The Role of College Costs, Family Background, and the Returns to Education.'' *Journal of Political Economy* 102(5):878–911.

———. 2003. "A Quasi-Experimental Estimate of the Impact of Financial Aid on College-Going." National Bureau of Economic Research Working Paper 9703.

Kane, Thomas J. and Cecilia Rouse. 1995."Labor Market Returns to Two- and Four-year College." *American Economic Review* 85(3):600–14.

Katz, Lawrence F., and Kevin M. Murphy. 1992. "Changes in Relative Wages, 1963-87: Supply and Demand Factors." *Quarterly Journal of Economics* 107 February, 35–78.

Krueger, Alan, and Diane Whitmore. 2001. "The Effect of Attending a Small Class in the Early Grades on College-test Taking and Middle School Test Results: Evidence from Project Star." *Economic Journal* 111 January, 1–28.

Leslie, Larry, and Paul Brinkman. 1988. *The Economic Value of Higher Education*. New York: Macmillan.

Lleras-Muney, Adriana. 2005 "The Relationship between Education and Adult Mortality in the United States." *Review of Economic Studies* 72(1).

Lochner, Lance, and Enrico Moretti. 2004, "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports," *American Economic Review* 94(1):155–89.

Long, Bridget. 2004. "How do Financial Aid Policies affect Colleges? The Institutional Impact of the Georgia HOPE Scholarship." *Journal of Human Resources* 39(3).

Meyer, Bruce. 1995. "Natural and Quasi-Natural Experiments in Economics." *Journal of Business and Economic Statistics* 12:151–62.

Milligan, Kevin, Enrico Moretti, and Philip Oreopoulos. 2004. "Does Education Improve Citizenship? Evidence from the U.S. and the U.K." *Journal of Public Economics* 88:9–10.

Moretti, Enrico. 2004. "Estimating the Social Return to Higher Education: Evidence from Longitudinal and Repeated Cross-Sectional Data." *Journal of Econometrics* 121(1–2): 175–212.

National Center for Education Statistics, U.S. Department of Education. 2005. "Gender Differences in Participation and Completion of Undergraduate Education and How They Have Changed Over Time." Washington, D.C.: GPO.

Oreopoulos, Philip, Marianne Page and Ann Stevens. 2006. "Does Human Capital Transfer from Parent to Child? The Intergenerational Effects of Compulsory Schooling." *Journal of Labor Economics* 24(4):726–60.

Organization for Economic Cooperation and Development. 2004. *Education at Glance*: *OECD Indicators 2004*. Paris: OECD.

Salzer, Patrick.2005. "House Votes to Limit HOPE to 127 Credits." *The Atlanta Journal Constitution*, February 22.

Seftor, Neil, and Sarah Turner. 2002. "Back to School: Federal Student Aid Policy and Adult College Enrollment." *Journal of Human Resources* 37(2):336–52.

Stinebrickner, Ralph, and Todd Stinebricker. 2003. "Understanding Educational Outcomes of Students from Low-Income Families." *Journal of Human Resources* 38(3):591–617.

Tinto, Vincent. 1994. *Leaving College: Rethinking the Causes and Cures of Student Attrition*. Chicago: University of Chicago Press.

Turner, Sarah E. 2004 "Going to College and Finishing College: Explaining Different Educational Outcomes," in Caroline Hoxby, ed., *College Choices: The Economics of Where to Go, When to Go, and How To Pay for It*. Chicago: University of Chicago Press.

Winston, Gordon. 1999. "Subsidies, Hierarchy and Peers: The Awkward Economics of Higher Education." *Journal of Economic Perspectives* 13(1):13–36.