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The New Merit Aid

Susan Dynarski

2.1 Introduction

Merit aid, a discount to college costs contingent upon academic performance, is nothing new. Colleges and private organizations have long rewarded high-achieving, college-bound high school students with scholarships. For example, the privately funded National Merit Scholarship program, established in 1955, annually awards grants to 8,000 entering college freshmen who perform exceptionally on a standardized test. Private colleges have long used merit scholarships to lure students with strong academic credentials.

While merit aid has a long history in the private sector, it has not played a major role in the public sector. Historically, government subsidies to college students have not been merit based. At the federal level, aid has been need based and strongly focused on low-income students. Eligibility for the two largest federal aid programs, the Pell Grant and Stafford Loan, is determined by a complex formula that defines financial need on the basis of income, assets, and family size. The formula is quite progressive: 90 percent of dependent students who receive federal grants grew up in families with incomes less than $40,000.[1]

At the state level, subsidies for college students have historically taken the form of low tuition at public college and universities. Most states have

1. Calculated from data in National Center for Education Statistics (1998a, table 314).

long had *some* form of merit aid, but these programs have traditionally been small and limited to the most elite students. For example, New York rewards each high school's top scorer on the Regents exam with a scholarship. While such small merit programs abound, the vast bulk of state spending on higher education takes the form of low tuition, made possible by the $50 billion in subsidies that states annually provide their postsecondary institutions. These institutional subsidies are highest at the flagship universities, which draw the highest-achieving students. In this sense, these institutional subsidies are, by far, the largest "merit aid" program in the United States. Access to this state subsidy has traditionally been controlled not by state governments but by the schools, who decide which students are sufficiently meritorious to gain admission.

Recently, however, state legislatures have gotten into the business of defining academic merit and awarding merit aid to hundreds of thousands of students. Since the early 1990s, more than a dozen states have established broad-based merit aid programs. The typical program awards tuition and fees to young residents who have maintained a modest grade point average in high school. Many require a high school grade point average (GPA) of 3.0 or above, not a particularly high threshold: In 1999, 40 percent of high school seniors met this standard.[2] Georgia, for example, gives a free ride at its public colleges and universities to residents who have a GPA of 3.0 in high school.[3] In Arkansas, the GPA cutoff is 2.5, exceeded by 60 percent of high school students.

This new breed of merit aid differs from the old style in both its breadth and, plausibly, its effect on students' decisions. The old style of merit aid was aimed at top students, whose decision to attend college is not likely to be contingent upon the receipt of a scholarship. By design, if not by intent, this elite form of merit aid goes to students whose operative decision is not whether to attend college, but which high-quality, four-year college to choose. By contrast, the new, broad-based merit aid programs are open to students with solid although not necessarily exemplary academic records. Such students may be uncertain about whether to go to college at all. When offered a well-publicized, generous scholarship, some of these students may decide to give college a try. Even among students who would have gone to college without the scholarship, the incentives of merit aid may have an effect on schooling decisions. For example, some may choose a four-year school over a two-year school, or a private school over a public

2. As I will discuss later in the paper, this figure varies quite dramatically by race and ethnicity. Source: Author's calculations from the 1997 National Longitudinal Survey of Youth (NLSY). This is the share of students with a *senior year* GPA of at least 3.0 and so is probably an upper bound on the share of students who achieve this GPA for their entire high school career. Unfortunately, NLSY does not contain GPA data for the entire high school career.

3. As the paper will discuss, the merit programs require that a high level of academic performance be maintained in college. In Georgia, a GPA of 3.0 must be maintained in college, a considerably higher hurdle than a 3.0 in high school.

school.[4] Those students planning to go to college out of state may instead decide to stay closer to home in order to take advantage of a merit scholarship.

This chapter will examine how merit aid affects this array of schooling decisions, using household survey data to measure the impact of the new state programs. I start with a case study of the Georgia Helping Outstanding Pupils Educationally (HOPE) Scholarship, the namesake and inspiration of many of the new state programs. I then extend the analysis to other states that now have broad-based, HOPE-like programs. In the empirical analysis, I pay particular attention to how the effect of merit aid has varied by race and ethnicity.

Merit aid might affect the decisions not only of students but also of institutions. Do colleges increase their tuition prices, in order to capture some of the subsidy? Do they reduce other forms of aid? Does the linkage of scholarships to grades lead to grade inflation at high schools and colleges? A number of studies have addressed these questions, and I will review the evidence on these topics. Finally, I will briefly discuss the political economy of merit aid. Why has it arisen where it has and when it has? What are the prospects for its continuation and growth, given the current, poor fiscal prospects of the states?

2.2   State Merit Aid: A Primer

Broad-based state merit aid became common in a very short span of time. In 1993, just two states, Arkansas and Georgia, had programs in place. By 2002, thirteen states had introduced large merit aid programs. Most of this growth has occurred quite recently, with seven programs starting up since 1999. As is clear from the map in figure 2.1, merit aid is heavily concentrated in the southern region of the United States. Of the thirteen states with broad-based merit aid programs, nine are in the South. Table 2.1 summarizes the characteristics of the thirteen broad-based merit programs. As was discussed earlier, dozens of states have some form of merit aid in place. The state programs detailed in table 2.1 were chosen because they have particularly lenient eligibility criteria, with at least 30 percent of high school students having grades and test scores high enough to qualify for a scholarship.[5]

4. Two-year colleges are generally cheaper than four-year colleges. Most merit aid programs make them both free.

5. The eligibility estimates are based on national data from the NLSY97. Many of the states listed in table 2.1 do not have enough observations in the NLSY97 to allow state-specific estimates of the share of students whose GPA qualifies them for their state's merit program. For all states, therefore, I use the national grade distribution to impute the share in a state that meets the eligibility criteria. When available, state-level data on the ACT and SAT are used to measure the share of students who meet these criteria. Note that these estimates are used only to choose the merit programs to be analyzed; they are not used in the paper's regression analyses.
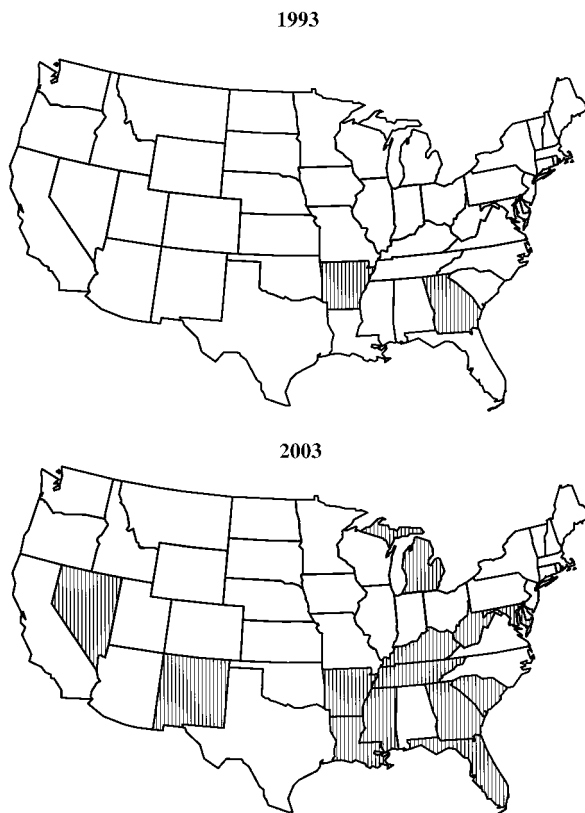
1993

2003

Fig. 2.1   States with broad-based merit aid programs

For example, the Arkansas award requires a GPA of 2.5, a standard met by 60 percent of high school students nationwide. The state also requires a minimum on the American College Test (ACT) of 19, a score exceeded by 60 percent of test takers nationwide and well below the Arkansas state average of 20.4. Five other states, like Arkansas, condition eligibility on a minimum GPA and test score. Six states use only GPA to determine eligibility. Of the states that require a minimum GPA, four require a GPA of 3.0, while two make awards to those with a GPA of 2.5.

Only one state—Michigan—bases eligibility solely on standardized test performance. For the class of 2000, 31 percent of Michigan students had test scores sufficiently high to merit an award. However, this overall eligibility rate masks substantial heterogeneity: Just 7.9 percent of African American students met the Michigan requirement. Civil rights groups have protested that this wide gap in eligibility indicates that Michigan's achievement test is an inappropriate instrument with which to determine

Table 2.1   **Merit Aid Program Characteristics, 2003**

State	Start	Eligibility	Award (in-state attendance only, exceptions noted)
Arkansas	1991	initial: 2.5 GPA in HS core and 19 ACT renew: 2.75 college GPA	public: $2,500 private: same
Florida	1997	initial: 3.0–3.5 HS GPA and 970–1270 SAT/20–28 ACT renew: 2.75–3.0 college GPA	public: 75–100% tuition/fees[a] private: 75–100% average public tuition/fees[a]
Georgia	1993	initial: 3.0 HS GPA renew: 3.0 college GPA	public: tuition/fees private: $3,000
Kentucky	1999	initial: 2.5 HS GPA renew: 2.5–3.0 college GPA	public: $500–3,000[a] private: same
Louisiana	1998	initial: 2.5–3.5 HS GPA and ACT > state mean renew: 2.3 college GPA	public: tuition/fees + $400–800[a] private: average public tuition/fees[a]
Maryland	2002	initial: 3.0 HS GPA in core renew: 3.0 college GPA	2-year school: $1,000 4-year school: $3,000
Michigan	2000	initial: level 2 of MEAP or 75th percentile of SAT/ACT renew: NA	in-state: $2,500 once out-of-state: $1,000 once
Mississippi	1996	initial: 2.5 GPA and 15 ACT renew: 2.5 college GPA	public freshman/sophomore: $500 public junior/senior: $1,000 private: same
Nevada	2000	initial: 3.0 GPA and pass Nevada HS exam renew: 2.0 college GPA	public 4-year: tuition/fees (max $2,500) public 2-year: tuition/fees (max $1,900) private: none
New Mexico	1997	initial: 2.5 GPA 1st semester of college renew: 2.5 college GPA	public: tuition/fees private: none
South Carolina	1998	initial: 3.0 GPA and 1100 SAT/24 ACT renew: 3.0 college GPA	2-year school: $1,000 4-year school: $2,000
Tennessee	2003	initial: 3.0–3.75 GPA and 890–1280 SAT/19–29 ACT renew: 3.0 college GPA	2-year school: tuition/fees ($1,500–2,500)[a] 4-year school: tuition/fees ($3,000–4,000)[a]
West Virginia	2002	initial: 3.0 HS GPA in core and 1000 SAT/21 ACT renew: 2.75–3.0 college GPA	public: tuition/fees private: average public tuition/fees

Note: HS = high school.

[a] Amount of award rises with GPA and/or test score.

eligibility for a state-funded scholarship. Similar objections were raised in Arkansas, which initially based eligibility for its program only on performance on standardized tests but later broadened the criteria to include academic performance in high school.

These controversies point to a shared characteristic of merit programs: their scholarships flow disproportionately to white, non-Hispanic, upper-income students. One reason is that blacks, Hispanics, and low-income youths are relatively unlikely to attend college, so *any* subsidy to college students will flow disproportionately to white, upper-income youth. But even among those nonwhite, Hispanic, and low-income youths who do attend college, academic performance is a barrier to merit aid eligibility.

For merit programs that are based on standardized tests, it is unsurprising to see (as in Michigan) a large gap in the eligibility rates of whites and African Americans, as the correlation between standardized test performance and race is well documented. However, even those programs with only a GPA cutoff will experience large racial differences in eligibility, since academic performance in the classroom varies considerably by race and ethnicity. Forty percent of high school seniors have a 3.0 GPA or higher, while only 15 percent of African Americans and Hispanics meet this standard. Further, blacks and Hispanics receive relatively low grades in college, which threatens their ability to keep any merit scholarship they are able to win with their high school grades.

Since nonwhite youths are less likely to qualify, it is plausible that merit aid programs will have little positive impact upon their college attendance. Further, if the new merit aid crowds out state spending on need-based aid or leads to higher tuition prices, the programs may actually *decrease* low-income, nonwhite college attendance, since these populations will face the resulting cost increases but will be disproportionately ineligible for the new merit scholarships. Merit aid would therefore tend to widen existing gaps in college attendance, as it flows to those who already attend college at the highest rates. A countervailing force is that blacks and Hispanics may be relatively sensitive to college costs. Among those blacks and Hispanics who are eligible, a merit program could have a relatively large impact on schooling decisions. It is therefore an empirical question, to be investigated by this chapter, whether merit programs narrow or widen existing racial and ethnic gaps in postsecondary schooling.

2.3   Case Study: The Georgia HOPE Scholarship

In 1991, Georgia Governor Zell Miller requested that the state's General Assembly consider the establishment of a state-run lottery, with the proceeds to be devoted to education. The Georgia General Assembly passed lottery-enabling legislation during its 1992 session and forwarded the issue to voters, who approved the required amendment to the state's constitution

in November of 1992. The first lottery tickets were sold in June of 1993. $2.5 billion in lottery revenue has flowed into Georgia's educational institutions since 1993. The legislation and amendment enabling the lottery specified that the new funds were not to crowd out spending from traditional sources. While it is not possible to establish conclusively that such crowdout has not occurred, spending on education has risen substantially since the lottery was initiated, both in absolute dollars and as a share of total state spending. Roughly equal shares of lottery funds have gone to four programs: the HOPE Scholarship, educational technology for primary and secondary schools, a new pre-kindergarten program, and school construction.

Residents who have graduated since 1993 from Georgia high schools with at least a 3.0 GPA are eligible for HOPE.[6] Public college students must maintain a GPA of 3.0 to keep the scholarship; a similar requirement was introduced for private school students in 1996. The HOPE Scholarship pays for tuition and required fees at Georgia's public colleges and universities. Those attending private colleges are eligible for an annual grant, which was $500 in 1993 and had increased to $3,000 by 1996. A $500 education voucher is available to those who complete a General Education Diploma (GED). The first scholarships were disbursed in the fall of 1993. Participation in HOPE during its first year was limited to those with family incomes below $66,000; the income cap was raised to $100,000 in 1994 and eliminated in 1995.

Two administrative aspects of HOPE differentially affected low- and upper-income youths. Since income is highly correlated with race and ethnicity, these administrative quirks may explain any racial and ethnic heterogeneity we observe in HOPE's effect. First, until 2001, HOPE awards were offset by other sources of aid. A student who received the maximum Pell Grant got no HOPE Scholarship except for a yearly book allowance of $400.[7] Insofar as blacks and Hispanics are disproportionately represented in the ranks of those who receive need-based aid, their HOPE awards would have been reduced more frequently than those of their white, non-Hispanic peers. Second, also until 2001, students from families with low incomes faced a more arduous application process for HOPE than did other students. Georgia education officials, concerned that students would forgo applying for federal aid once the HOPE Scholarship was available,

6. The high school GPA requirement is waived for those enrolled in certificate programs at technical institutes. For high school seniors graduating after 2000, only courses in English, math, social studies, science, and foreign languages count toward the GPA requirement.

7. As a result of this provision and the scaling back of the state's need-based State Student Incentive Grants (SSIGs), some low-income students have actually seen their state aid reduced since HOPE was introduced (Jaffe 1997). This contemporaneous shift in SSIG spending has the potential to contaminate the paper's estimates. However, SSIG spending was so miniscule—$5.2 million in 1995, before the program was scaled back—that the impact of its elimination on the estimates is likely to be inconsequential.

mandated that applicants from families with incomes lower than $50,000 complete the Free Application for Federal Student Aid (FAFSA). The rationale for the $50,000 income threshold was that few students above that cutoff were eligible for need-based, federal grant aid.[8] The four-page FAFSA requests detailed income, expense, asset, and tax data from the family. By contrast, those with family incomes above $50,000 filled out a simple, one-page form that required no information about finances other than a confirmation that family income was indeed above the cutoff. As a consequence of the two provisions just discussed, low-income students faced higher transaction costs and lower average scholarships than did upper-income students.

In 2000–2001, 75,000 students received $277 million in HOPE Scholarships. Georgia politicians have deemed HOPE a great success, pointing to the steady rise in the number of college students receiving HOPE. The key question is whether the program actually changes schooling decisions or simply subsidizes inframarginal students. In the next section, I discuss the data and empirical strategy I will use to answer this question.

2.4 Data

Any empirical analysis of state financial aid policy quickly comes face to face with frustrating data limitations. The data requirements appear minor, since eligibility for merit aid is determined by a very short list of characteristics: state of residence at the time of high school graduation, high school GPA, standardized test score, and, in some states, parental income. In order to use this information in an evaluation of the effect of merit aid, we would want these characteristics for repeated cohorts of high school students, both before and after merit aid is introduced in their state, so that schooling decisions of eligible and ineligible cohorts could be compared.[9] Finally, we need a data set with state-level samples large enough to allow for informative analysis.

No publicly available data set meets all of these requirements. Surveys that are limited to college students do not, by their nature, allow us to examine the college attendance margin. For example, the National Postsecondary Student Aid Survey (NPSAS) surveys college students about their aid packages and contains detailed information from students' aid appli-

8. In 1995, only 3.7 percent of dependent students from families with incomes over $40,000 received federal grant aid, while 57 percent of those from families with income under $20,000 did so (National Center for Education Statistics 1998a).

9. Alternatively, we could make use of the sharp discontinuities in the eligibility requirements to estimate the effect of merit aid from a single cohort. Kane (2003) uses this approach in an evaluation of California's CalGrant program, comparing the college attendance of those very slightly above and very slightly below the grade point cutoff. This approach requires very large samples; Kane uses an administrative data set that is a near-census of potential college entrants.

cations. By design, this data set cannot inform us about those students who decided *not* to go to college. Without making strong assumptions about how those who do not go to college differ from those who do, we cannot use the NPSAS to examine how aid affects the college attendance rate.

The NPSAS can be used to answer other questions of interest, however. For example, we might be interested in whether merit aid leads to higher tuition prices, or more or less government spending on other forms of aid. Or we might be interested in how the racial composition of a state's schools changes, if at all, after the introduction of a merit aid program. The NPSAS, as well as data that institutions gather about their students and report to the government through the Integrated Postsecondary Education Data System (IPEDS), can answer questions of this type.[10]

The National Longitudinal Surveys (NLSs) of Youth of 1979 and 1997 are particularly rich sources of data, containing information about academic performance on standardized tests, grades, parental income, and schooling decisions.[11] In a few years, the NLSY97 will be a useful resource for evaluating the newer merit aid programs, in particular those introduced in the late 1990s. The only weakness of the NLSY97 is that it is unlikely to interview enough youths in any one state to allow for detailed examination of a single merit aid program. Observations from multiple merit states could be pooled, however, as is done with the Current Population Survey in this paper.

Another potentially fruitful option for research in this area is data from administrative sources. Kane (2003) and Henry and Rubinstein (2002) take this approach in evaluations of programs in California and Georgia, respectively.[12] Kane matches enrollment data from California's public universities and colleges to federal aid applications and high school transcripts. He then uses regression-discontinuity methodology to estimate the effect of California's merit program on schooling decisions. Henry and Rubinstein use data from the College Board on high school grades and SAT scores in order to examine whether the Georgia HOPE Scholarship has led to grade inflation in high schools.

2.4.1   The Current Population Survey and the Analysis of State Aid Policy

The bulk of the analysis in this paper is based on a publicly available survey data set, the Current Population Survey (CPS). The CPS is a national

10. Papers that use college-based surveys in this way include Long (2002) and Cornwell, Mustard, and Sridhar (2003), both of which evaluate the Georgia HOPE Scholarship.

11. The U.S. Department of Education's longitudinal surveys of the high school cohorts of 1972, 1982, and 1992 contain similarly rich data. But because each survey contains a single cohort, we cannot use these data to observe schooling decisions in a given state both before and after merit aid is introduced.

12. California's program is not among the programs discussed in this chapter, as it is relatively narrow in its scope due to income provisions that exclude many middle- and upper-income youth.

household survey that each October gathers detailed information about schooling enrollment. Data on type of school attended, as well as basic demographics such as age, race, and ethnicity, are included in the CPS. While the CPS is the world's premier labor force survey, from the perspective of this chapter it has some key limitations.

First, the CPS lacks information about academic performance. We therefore cannot narrow the analysis to those whose academic performance makes them eligible for merit aid, and thereby measure the effect on schooling decisions of offering a merit scholarship among those who qualify (an effect I will denote π). From a policy perspective, the question we *can* answer is quite relevant: How does the existence of a merit aid program affect the schooling decisions of a state's youths? To answer this question, I will estimate a program effect (denoted β) that is the product of two interesting parameters: (1) π, the behavioral response to the offer of aid of youths eligible for the scholarship and (2) δ, the share of youths eligible for the scholarship:[13]

$$\beta = \pi\delta$$

When considering the effect of a financial aid program such as the Pell Grant, we generally are interested only in π. We assume that the parameters that determine Pell eligibility, such as family size and income, cannot easily be manipulated by those eager to obtain the grant. By contrast, merit aid is a program that *intends* to induce behavior that will increase the share that is aid-eligible. Policymakers consistently cite their desire to give students a financial incentive to work hard in high school and college as their motivation for establishing merit aid programs. Estimating π while ignoring δ would therefore miss half the story. Fortunately, data constraints prevent us from making this mistake!

A more serious weakness of the CPS is that it provides family background data for only a subset of youths. Highly relevant variables such as parental income, parental education, and other measures of socioeconomic status are available only for those youths who live with their families or who are temporarily away at college.[14] The probability that a youth has family background information available is therefore a function of his or her propensity to attend college. Under these circumstances, we cannot limit the analysis to those who have family background data without inducing bias in analyses in which college attendance is an outcome of interest.[15] In

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13. This formulation ignores any heterogeneity in π, the effect of the offer of aid on those who are eligible. It is almost certain that this effect is not homogeneous. For example, the offer of aid will probably have a different effect on those whose grades place them just on the margin of eligibility and those whose grades are so strong that they are well within this margin.

14. These youths appear on their parents' CPS record and so can be linked to parental data. Other youths will show up in the CPS as members of their own households.

15. Cameron and Heckman (1999) discuss this point.

the analysis, therefore, I will make use only of background variables that are available for all youths.

2.4.2 Is State of Residence of Youth Systematically Mismeasured in the CPS?

A final weakness of the CPS is that it explicitly identifies neither the state in which a person attended high school nor the state in which he or she attends college. In this paper, I proxy for the state in which a person attended high school with current state of residence. This is a reasonable proxy, for two reasons. First, among eighteen-to-nineteen-year-olds, the group studied in this chapter, migration across state lines for reasons other than college is minimal. Second, when youths *do* go out of state to college, according to CPS coding standards they are recorded as residents of the state of origin, rather than the state in which they attend college.

The key question is whether these standards are followed in practice. We are confident that this protocol has been followed for those youths (78 percent of the sample) who appear on their parents' record.[16] Whether the CPS correctly captures the state of residence for the other 22 percent is an important question, as error in the collection of these data will bias the chapter's estimates.

If state of residence is simply a noisy measure of state of origin for this 22 percent, then the paper's estimates will be biased toward zero. But consider the following scenario, in which we will be biased toward finding a *positive* effect of merit aid on the probability of college attendance. Say that HOPE has no effect on the college entry margin but does affect whether students go to college in state. If the CPS incorrectly codes the state of residence as the state in which one is attending college, then any drop in the outward migration of Georgia college students induced by HOPE will mechanically induce an increase in the observed share of Georgia youths attending college.

A few simple tabulations can give us a sense of whether this is a problem. If the scenario laid out in the previous paragraph holds, then we should observe relative growth in the *size* of the college-age population in Georgia after HOPE is introduced. To test this hypothesis, I predicted the size of Georgia's college-age population by aging forward the high school–age population. Specifically, I compared the population of eighteen-to-nineteen-year-olds in a given state to the population of sixteen-to-seventeen-year-olds in the same state two years earlier. This is an admittedly crude prediction of cohort size. It will be wrong for any number of reasons, among them immigration and incarceration of teenagers (prisons are not in the

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16. We cannot restrict the analytical sample to this subset because, as discussed earlier, whether a youth is on his or her parents' record is correlated with whether he or she is in college.

CPS sampling frame). However, the relevant issue is not how error-ridden this prediction is, but whether the sign and magnitude of its error *change systematically* when a merit program is introduced in a state. In particular, does the population of college-age youths expand unexpectedly when a state introduces a merit program?

Figure 2.2 plots the difference between the predicted and actual cohort sizes, with the difference normed by the predicted size. I plot the normed error for Georgia and the average normed error for the other states in the Southeast and the United States.[17] For measurement error to be inducing positive bias in the paper's estimates, the errors should grow relatively more negative in Georgia after HOPE is introduced. There is no such clear trend. The large negative errors in Georgia in 1993 through 1995 are somewhat disturbing, even though a muted version of this pattern also appears in the U.S. and Southeastern series. In figure 2.3, I show the same series for West Virginia, a southern state that had no merit program during this period. This state's pattern is almost identical to that of Georgia, suggesting that Georgia's shifts in cohort size are random noise and that the paper's estimates will not be contaminated by this source of bias.

2.5   Georgia HOPE Analysis

I begin by examining how the college attendance rate has changed in Georgia since HOPE was introduced, compared to how it has evolved in the other Southern states that have not introduced merit programs. The outcome of interest is whether an eighteen-to-nineteen-year-old is currently enrolled in college. I start with a parsimonious specification, in which an indicator variable for being enrolled in college is regressed against a set of state, year, and age effects, along with a variable, HOPE, that is set to 1 in survey years 1993 through 2000 for those who are from Georgia. In this equation, the HOPE variable therefore indicates that a young person of college age resides in Georgia after HOPE is in operation.

The estimating equation is as follows:

$$(1) \qquad y_{iast} = \beta_0 + \beta_1 HOPE_{st} + \delta_s + \delta_t + \delta_a + \varepsilon_{iast},$$

where y_{iast} is an indicator of whether person i of age a living in state s in year t is enrolled in college; δ_s, δ_t, and δ_a denote state, year, and age fixed effects, respectively; and ε_{iast} is an idiosyncratic error term. I use ordinary least

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17. That is, I calculate the prediction error for each state-year and divide it by the predicted value for that state-year. I take the average of these normed, state-year errors separately for the Southeastern United States and the entire United States, in both cases excluding Georgia. Each state-year is treated as a single observation; I have not weighted by population.

The Georgia series is substantially more volatile than those of the Southeast and United States; however, any state's error will look volatile compared to averages for the region and country. See figure 2.3 for an example of an equally volatile state.
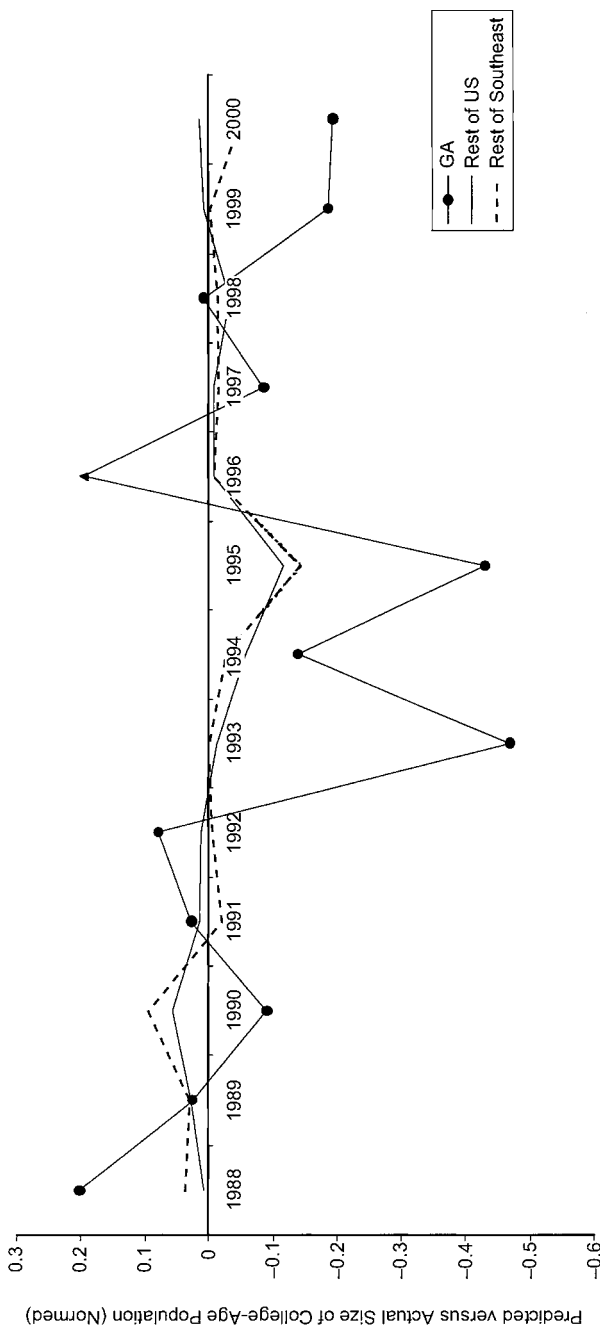
Fig. 2.2   Does measurement error in state of residence bias the estimates?

Note: The figure plots the difference between the predicted and actual population of college-age youth, with the difference normed by the predicted population. The predicted population of eighteen-to-nineteen-year-olds in a state is the number of sixteen-to-seventeen-year-olds in that state two years earlier. The data used are the October Current Population Surveys.

Fig. 2.3   Does measurement error in state of residence bias the estimates?
Note: The figure plots the difference between the predicted and actual population of college-age youth, with the difference normed by the predicted population. The predicted population of eighteen-to-nineteen-year-olds in a state is the number of sixteen-to-seventeen-year-olds in that state two years earlier. The data used are the October Current Population Surveys.
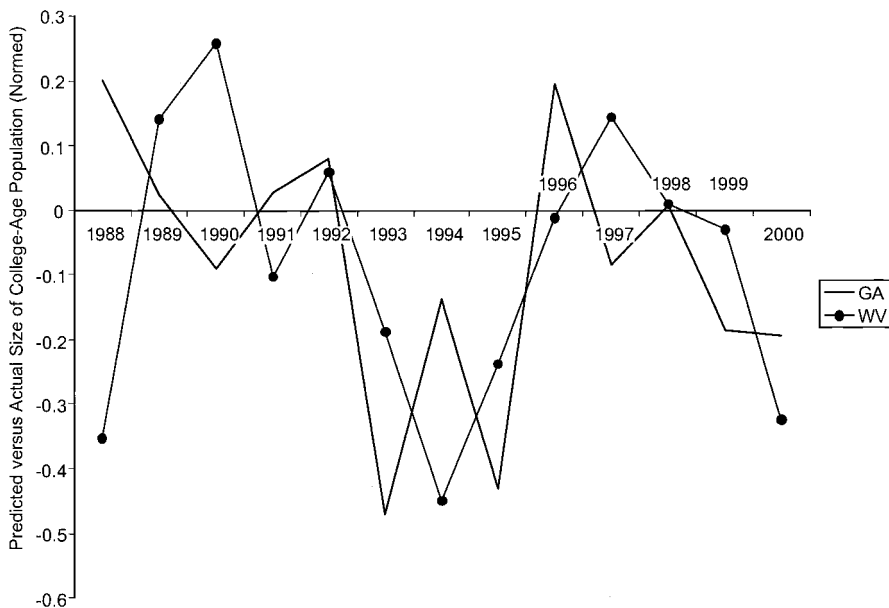
squares (OLS) to estimate this equation, correcting standard errors for heteroskedasticity and correlation of the error terms within state cells.

Recall that HOPE (1) decreases the price of college, (2) decreases the price of in-state colleges relative to out-of-state colleges, and (3) decreases the price of four-year colleges relative to two-year colleges. The corresponding predicted behaviors for Georgia residents are (1) increased probability of college attendance, (2) increased probability of in-state attendance relative to out-of-state attendance, and (3) increased probability of four-year attendance relative to two-year attendance.

Column (1) of table 2.2 shows the college attendance results. The estimates indicate that the college attendance rate in Georgia rose 8.6 percentage points relative to that in the other Southern, nonmerit states after HOPE was introduced. The estimate is highly significant, with a standard error of 0.8 percentage points. This estimate is quite close to the estimate in Dynarski (2000), which was based on CPS data for 1989 through 1997.[18]

18. The standard error is substantially smaller, however, than that in Dynarski (2000), which conservatively corrected standard errors for correlation at the state-year level. Bertrand, Duflo, and Mullainathan (2002) conclude that, in this type of application, the appropriate correction is for correlation at the state level.

Table 2.2   **Estimated Effect of Georgia HOPE Scholarship on College Attendance of Eighteen-to-Nineteen-Year-Olds (Southern Census region)**

	(1)	(2)	(3)	(4)
HOPE Scholarship	.086	.085	.085	.069
	(.008)	(.013)	(.013)	(.019)
Merit program in border state			−.005	−.006
			(.013)	(.013)
State and year effects	Y	Y	Y	Y
Median family income		Y	Y	Y
Unemployment rate		Y	Y	Y
Interactions of year effects with				
black, metro, Hispanic		Y	Y	Y
Time trends				Y
R^2	.020	.059	.059	.056
No. of observations	8,999	8,999	8,999	8,999

Notes: Regressions are weighted by CPS sample weights. Standard errors (in parentheses) are adjusted for heteroskedasticity and correlation within state cells. Sample consists of eighteen-to-nineteen-year-olds in Southern Census region, excluding states (other than Georgia) that introduce merit programs by 2000. See table 2.1 for a list of these states.

The result suggests that HOPE did, as predicted, increase the share of youths attending college.

I next probe the robustness of this result by adding a set of covariates to this regression. For reasons discussed earlier, I limit myself to covariates that are available for the entire sample and exclude any that require that a youth and his or her parents appear on the same survey record, such as parental education and income. Control variables indicate whether a youth lives in a metropolitan area, is African American, or is Hispanic. These three variables are each interacted with a full set of year effects, so that the effect of these attributes on schooling decisions is allowed to vary flexibly over time. I also include the state's unemployment rate and the median income of families with children who are near college age. These two variables are intended to capture any Georgia-specific economic shocks that may have affected college attendance decisions. Results are in column (2). The coefficient does not change, although the standard error increases to 1.3 percentage points.

I next examine whether the effect of merit aid extends across state borders. Since students travel across state lines for college, changes in postsecondary education policy in one state will reverberate in neighboring states. If more Georgians want to go to college, and the supply of colleges is inelastic, students from Florida, for example, will be pushed out of school when HOPE is introduced. The estimating equation is as follows:

$$(2) \quad y_{iast} = \beta_0 + \beta_1 HOPE_{st} + \beta_2 border\_merit_{st} + \beta_3 X_{st} + \beta_4 X_i + \delta_s + \delta_t$$
$$+ \delta_a + \varepsilon_{iast}$$

β_2 captures the effect of having a merit program in a neighboring state. X_{st} and X_i are the state-year and individual covariates discussed in the previous paragraph and used in column (2). Results are in column (3). The results weakly suggest that having a merit program on one's border has a small, negative effect on college attendance, indicating the presence of supply constraints. The point estimate is fairly imprecise, however: –0.5 percentage points, with a standard error of 1.3 percentage points.[19]

An identifying assumption of the preceding analysis is that Georgia and the control states were on similar trends in their college attendance rates before HOPE was introduced. If they were instead on divergent trends the estimates will be biased. In particular, if attendance was rising in Georgia relative to the other states before 1993, then we will falsely attribute to HOPE the continuation of this trend. The inclusion of these preexisting trends in the equation will eliminate this source of bias. In column (4), I add to the regression separate time trends for Georgia and the nonmerit states.[20] The point estimate drops moderately, to 6.9 percentage points, indicating that Georgia was trending away from the rest of the South before HOPE. However, there is still a substantial relative increase in attendance in Georgia that cannot be explained by this trend.

2.5.1   The Effect of HOPE on School Choice

I next examine whether HOPE has affected decisions other than college entry. In particular, I examine the type of college that a student chooses to attend. The October CPS contains information about whether a student attends a public or private college and whether it is a two- or four-year institution. I use this information to construct four variables that indicate whether a person attends a two-year private school, a two-year public school, a four-year private school, or a four-year public school. I then run a series of four regressions in which these are the outcomes, including the same covariates as in the richest specification of table 2.2. I show results that both do and do not include time trends. The results are shown in table 2.3. The attendance results of the previous table are included for ease of comparison.

The HOPE Scholarship appears to increase the probability of attendance at four-year public institutions substantially, by 4.5 percentage points (no time trends) to 8.4 percentage points (time trends included). Attendance at four-year private schools also rises, although the estimates are smaller than those (2.2 to 2.8 percentage points). There is a somewhat smaller rise in the probability of attendance at two-year private schools (about 1.5 percentage points) and a drop at two-year public schools (of 1.7

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19. I have also tested the inclusion of the interaction of having a merit program in one's own state and having a merit program in a neighboring state. The interaction is never large or significant, and its inclusion does not affect the paper's estimates.

20. The time trends are estimated using pre-1993 data.

Table 2.3   **Effect of Georgia HOPE Scholarship on Schooling Decisions (October CPS, 1988–2000; Southern Census region)**

	College Attendance (1)	2-Year Public (2)	2-Year Private (3)	4-Year Public (4)	4-Year Private (5)
No time trends					
Hope Scholarship	.085	−.018	.015	.045	.022
	(.013)	(.010)	(.002)	(.015)	(.007)
R^2	.059	.026	.010	.039	.026
Add time trends					
Hope Scholarship	.069	−.055	.014	.084	.028
	(.019)	(.013)	(.004)	(.023)	(.016)
R^2	.056	.026	.010	.029	.026
Mean of dependent variable	.407	.122	.008	.212	.061

Notes: Specification in "No time trends" is that of column (3) in table 2.2. Specification in "Add time trends" adds trends estimated on pretreatment data. In each column, two separate trends are included, one for Georgia and one for the rest of the states. Sample consists of eighteen-to-nineteen-year-olds in Southern Census region, excluding states (other than Georgia) that introduce a merit program by 2000. No. of observations = 8,999. Standard errors in parentheses.

to 5.5 percentage points). All but two of the eight estimates are significant at conventional levels.

These shifts in schooling decisions are in the expected direction. Any subsidy to college will both pull students *into* two-year public schools (from not going to college at all) and push them *out of* two-year public schools (into four-year colleges). The HOPE Scholarship appears to push more students out of two-year, public institutions than it pulls in, producing a net drop at these schools. Most of these students appear to shift toward four-year public institutions, although some also shift into the private sector.[21]

2.5.2   The Effect of HOPE on Migration to College

We might expect that HOPE would also affect whether students choose to attend college in their home state. Data from both the University System of Georgia (USG) and the Department of Education's Residence and Migration Survey suggest that HOPE has had the effect of encouraging Georgia residents who would have attended a four-year college out of state to stay in Georgia instead. Data from the Residence and Migration Survey indicate that in 1992 about 5,000 Georgians were freshmen at two- and four-year colleges in the states that border Georgia. This represented an average of 3.4 percent of the border states' freshman enrollment. By 1998, just 4,500

21. Note that the coefficients for the four schooling options do not sum to the overall attendance effect. This is because the type of school is unknown for some students, who appear as college attenders but not as attending a specific type of school.

Georgians crossed state lines to enter college in the border states, accounting for an average of 2.9 percent of freshman enrollment in those states. This drop in migration was concentrated in a group of border schools that have traditionally drawn large numbers of Georgians. At the ten border schools drawing the most Georgia freshmen in 1992, students from Georgia numbered 1,900 and averaged 17 percent of the freshman class. By 1998, the ten top destinations enrolled 1,700 Georgians, who represented 9 percent of freshman enrollment. Jacksonville State College in Florida, for example, drew 189 Georgian freshmen in 1992 and only 89 in 1998; the share of the freshman class from Georgia dropped from 17 to 11 percent.

Further supporting the conclusion that Georgia's four-year college students are now more likely to attend college in state is a shift in the composition of Georgia's four-year colleges. Figure 2.4 shows data from the USG on the share of freshman enrollees that are Georgia residents at Georgia's two- and four-year public colleges. The data are separately plotted for the two-year, four-year, and elite four-year colleges in the state. Here we see a definite shift toward Georgia residents since HOPE was introduced, with the effect most pronounced at four-year colleges (especially the top schools) and least evident at the two-year schools. This pattern fits with our understanding that four-year students are most mobile when making college attendance decisions.
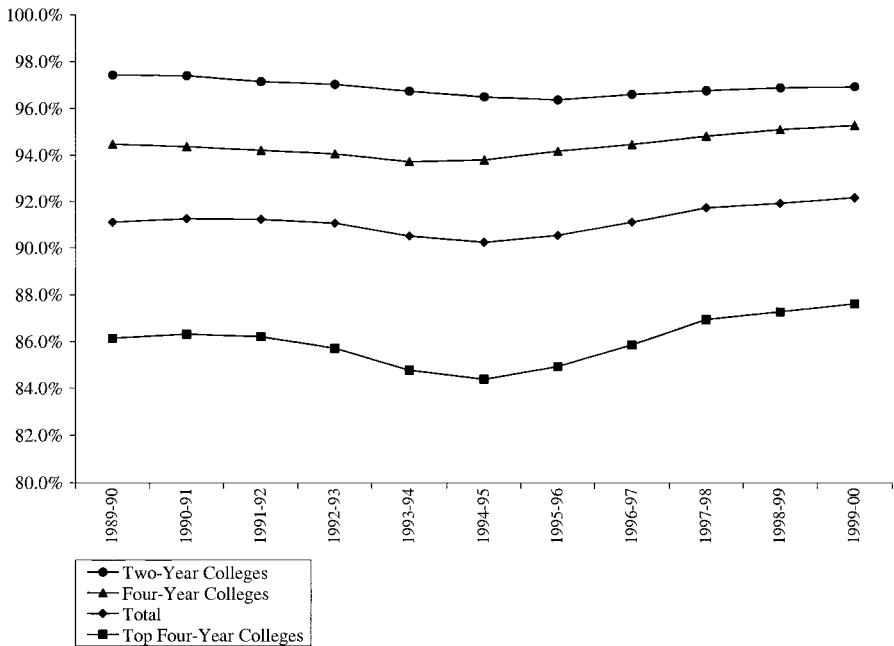

Fig. 2.4   University System of Georgia students, Georgia residents as share of total enrollment

2.5.3   The Differential Impact of HOPE by Race and Ethnicity

The effect of merit programs may vary across racial and ethnic groups for a number of reasons. First, as was discussed earlier, academic performance in high school is strongly correlated with race and ethnicity. Second, the rules of the programs are sometimes such that they are likely to have a lesser impact on low-income youths. Until recently, Georgia did not offer the grant to those youths who had substantial Pell Grants and low college costs. Mechanically, then, the program would have had a lower impact on African Americans and Hispanics, who tend to have lower incomes: in Georgia, 94 percent of African American but just 62 percent of white sixteen-to-seventeen-year-olds live in families with incomes less than $50,000.[22] The numbers for the rest of the United States are similar.[23] Third, states that have merit programs may reduce need-based aid or appropriations to colleges. Both of these effects would tend to make college more expensive for those who don't qualify for the merit programs to which the money is being channeled. Finally, the elasticity of schooling with respect to a given grant may differ across demographic groups. A priori, it is not clear whether blacks and Hispanics would be more or less elastic than other students in their schooling decisions.[24]

To explore how the effect of merit aid programs varies by race and ethnicity, I repeat the analysis of the preceding section but allow the effect of HOPE to differ across racial and ethnic groups. I divide the population into two mutually exclusive categories: (1) white non-Hispanics and (2) Hispanics of any race plus blacks.[25] I then estimate the effect of merit aid separately for each group. The estimating equation is

$$(3) \quad y_{iast} = \beta_0 + \beta_1 \text{Merit}_{st} + \beta_2 \text{Merit}_{st} \times \text{black\_hisp}_i + \beta_3 \text{border\_Merit}_{st}$$
$$+ \beta_4 X_{st} + \beta_5 X_i + \delta_s + \delta_t + \delta_a + \varepsilon_{iast}.$$

Results for Georgia are in table 2.4, for specifications that do and do not include preexisting time trends.[26] The point estimates are somewhat un-

Table 2.4   **Effect of Georgia HOPE Scholarship on College Attendance Analysis by Race and Ethnicity (October CPS, 1988–2000; Southern Census region)**

	No Time Trends	Time Trends
Merit Program	.096	.140
	(.014)	(.013)
Merit · black/Hispanic	–.030	–.147
	(.023)	(.039)
R^2	.059	.056

Notes: Specification in first column is that of column (3) in table 2.2. Specification in second column adds trends estimated on pretreatment data. Separate trends are included for four groups: white-control, white-treat, nonwhite-control and nonwhite-treat. Sample consists of eighteen-to-nineteen-year-olds in Southern Census region, excluding states other than Georgia that introduce a merit program by 2000. Standard errors in parentheses.

stable, changing substantially when time trends are included. But the two sets of estimates agree that HOPE had a substantially greater effect on white attendance than black and Hispanic attendance. The estimated effect of HOPE on the white attendance rate is 9.6 to 14.0 percentage points, while that on blacks and Hispanics is –0.7 to 6.6 percentage points. The results indicate that HOPE has increased racial and ethnic gaps in college attendance in Georgia.

2.6   The Effect of Broad-Based Merit Aid in Other States

The Georgia program was one of the first, largest, and best-publicized merit aid programs. It has also been, by far, the best-studied program; at this point, dozens of papers have analyzed its impact. In the absence of sound empirical research on the effect of the other merit programs, the Georgia experience has been extrapolated in predicting their effects.[27] However, as is shown in table 2.1, there is heterogeneity in program rules, which may well lead to heterogeneity in the programs' effects. Further, initial college attendance rates and the supply of postsecondary schools vary across the merit aid states, which may affect the impact of the merit programs on schooling decisions. For all these reasons, results from one state may not provide a good prediction of the effect of another state's merit program.

Fortunately, many of the merit aid programs in table 2.1 have now been in existence sufficiently long to allow us to separately estimate program effects for each state. I will limit my analysis to the South, where all but three of the programs in table 2.1 are located. A benefit of focusing on the Southern merit states is that they have a natural control group: the non-

27. An exception is the study by Binder and Ganderton (2002), which examined the effect of New Mexico's merit program. They conclude that New Mexico Success has not affected the college attendance rate but, like HOPE, has shifted students toward four-year schools.

merit Southern states. The programs of three Southern states (Maryland, Tennessee, and West Virginia) are excluded, as they were introduced after 2000, the last year of the sample. That leaves seven merit programs, located in Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, and South Carolina.

I follow the approach used in the analysis of HOPE, creating a variable that indicates a year and state in which a merit program is in place. I estimate the following equation:

(4) $y_{iast} = \beta_0 + \beta_1 merit_{st} + \beta_2 border\_merit_{st} + \beta_3 X_{st} + \beta_4 X_i + \delta_s + \delta_t$
$+ \delta_a + \varepsilon_{iast}$

Results are in table 2.5. The estimated overall effect of the seven merit programs is 4.7 percentage points. The estimate is highly significant, with a

Table 2.5   **Effect of All Southern Merit Programs on College Attendance of Eighteen-to-Nineteen-Year-Olds**

	All Southern States (N = 13,965)			Southern Merit States Only (N = 5,640)		
	(1)	(2)	(3)	(4)	(5)	(6)
Merit program	.047			.052		
	(.011)			(.018)		
Merit program, Arkansas		.048			.016	
		(.015)			(.014)	
Merit program, Florida		.030			.063	
		(.014)			(.031)	
Merit program, Georgia		.074			.068	
		(.010)			(.014)	
Merit program, Kentucky		.073			.063	
		(.025)			(.047)	
Merit program, Louisiana		.060			.058	
		(.012)			(.022)	
Merit program, Mississippi		.049			.022	
		(.014)			(.018)	
Merit program, South Carolina		.044			.014	
		(.013)			(.023)	
Merit program, year 1			.024			.051
			(.019)			(.027)
Merit program, year 2			.010			.043
			(.032)			(.024)
Merit program, year 3 and after			.060			.098
			(.030)			(.039)
State time trends			Y			Y
R^2	.046	.046	.047	.035	.036	.036

Notes: Specification is that of column (3) in table 2.2, with the addition of state time trends where noted. Sample consists of eighteen-to-nineteen-year-olds in Southern Census region, with the last three columns excluding states that have not introduced a merit program by 2000. Standard errors in parentheses.

standard error of 1.1 percentage points. In column (2), I allow this affect to vary across the seven states, by replacing the single merit dummy with a set of seven dummies, one for each state's program. The specification of column (2) is otherwise identical to that of column (1), and so the appropriately weighted average of the seven coefficients is the 4.7 percentage points of column (1). Six of the estimates are highly significant. Five are clustered between 4.9 (Mississippi) and 7.4 (Georgia). Well below Mississippi are Florida and South Carolina, with estimated effects of 3.0 and 0.2 percentage points, respectively.

We might suspect that the merit states are somehow different from the nonmerit states and that the nonmerit states therefore form a poor control group for these purposes. We can test the sensitivity of our results to the choice of control group by dropping the nonmerit states from the sample and estimating the effect of merit aid purely from the staggered timing of its rollout across the states. In this approach, the merit states form their own control group. Figure 2.5 graphically illustrates the identification strategy. During the first years of the sample (1988–1990), before the first merit program is introduced, all of the states are in the control group. In 1991, Arkansas moves into the treatment group, followed in 1993 by Georgia. By 2000, all of the states are in the treatment group. This approach assumes that the states that eventually have a merit program are on similar trends in the schooling outcomes of young people. The assumption is that the year in which a state's merit program begins is quasi-random, uncorrelated with any state-specific trends in or shocks to schooling decisions.

Results are in columns (4) and (5) of table 2.5. The estimated overall effect is insensitive to the choice of control group, with the estimate rising only slightly from 4.7 to 5.2 percentage points. The state-specific coefficients are somewhat more sensitive to the choice of control group. For five of the states, the two approaches yield similar results. The two exceptions are Arkansas and Florida, for whom the estimates vary substantially between column (2) and column (4). Arkansas's estimate drops from 4.8 to 1.6, while Florida's rises from 3.0 to 6.3.

Only South Carolina has a consistently small and insignificant effect,
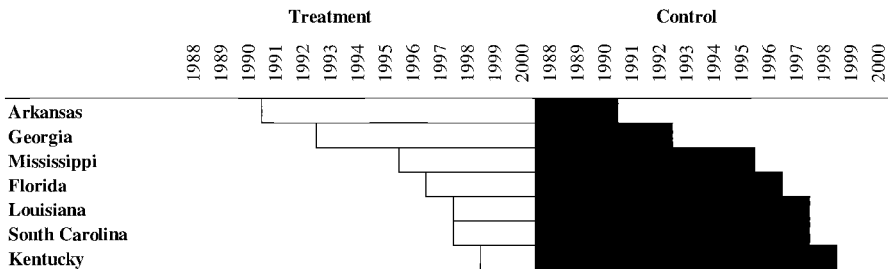

Fig. 2.5   Timing of introduction of state merit programs

which may be explained by its requirement that students score at least 24 on the ACT. Nationally, just 30 percent of test takers scored above South Carolina's ACT cutoff in 2000, while 88 percent met Missouri's requirement and about 60 percent met the requirements of Arkansas (19), Florida (20), and Louisiana (19.6).[28] The South Carolina legislature has come under pressure to loosen the scholarship requirements and has responded by adding another route to eligibility. As of 2002, students can qualify for a scholarship by meeting two of three criteria: 24 on the ACT, a GPA of 3.0, and graduating in the top 30 percent of one's high school class (Bichevia Green of the South Carolina Commission on Higher Education, personal communication, June 14, 2002). Further, the ACT requirement has been dropped completely for those attending two-year institutions. It will be of interest to see if the effect of South Carolina's program on college attendance rises with this shift in policy.

Next, I examine whether the inclusion of preexisting time trends affects the results. Preexisting trends could contaminate the results for both control groups. The merit states may be on different time trends from the nonmerit states, and they may be on different trends from each other. I estimate a trend for the entire 1988–2000 period for each state. Deviations from these trends after a state introduces merit aid are then attributed to the new program.[29]

Results are in columns (3) and (6). As was true in the specification without time trends, the merit-only control group produces somewhat larger estimates. Both approaches indicate that the effect of merit aid evolves over time, with the effect rising from 2.4 percentage points in the first year a program is in effect to 6.0 percentage points in year three and beyond. When the merit states are used as their own control group, the effect rises from 5.1 percentage points in year one to 9.8 percentage points in the third year. Note that these are not cumulative effects but period-specific program effects.

The effect of merit aid may rise during the first years of a program for several reasons. It may take time for information about the new programs to diffuse. It also takes time for high school students who are inspired to work harder to increase their overall GPAs. Those who are high school seniors when a program is first introduced can do little to increase their cu-

28. These figures refer to the national ACT distribution, which has a mean of 21. The black and Hispanic distributions have lower means, of 16.9 and 19, respectively. Fewer members of these groups will meet the state ACT cutoffs.

29. In this specification, a simple merit dummy will not properly identify the effect of the merit aid program, as such an approach would inappropriately attribute part of the aid-induced change to the trend. We can solve this problem by replacing the merit dummy with either a separate time trend or year effects after merit aid is introduced in a state. Wolfers and Stevenson (forthcoming) use this approach to estimate the effect of divorce law reform, which occurred in different states in different years.

mulative GPAs, while those who are freshmen have four years to increase their effort. The pool of eligible youths may thereby expand over time.

The effect could also *diminish* over time, if many college students fail to qualify for scholarship renewals and their younger peers are discouraged from taking up the scholarship. Further, in the presence of supply constraints, the effect of latecomer programs would be smaller than that of earlier programs, as attendance grows and the supply grows tighter. The results in table 2.5 indicate that, across the merit states, the incentive and information effects dominate the discouragement effect.

2.6.1   The Effect of Merit Aid on College Choice

The analysis of the previous section indicates that the state merit aid programs have increased college attendance. I next examine whether these programs have also affected the choice of college, as was true in Georgia. I use the analytical framework of the previous section, although I will only show results that pool the merit states in order to gain precision. All of the Southern states are included in the sample; results are similar, but less precise, when the sample is limited to the Southern merit states. I show results that do and do not include time trends.

Table 2.6 indicates that, overall, the Southern merit programs have had a strong effect on the choice of college, with a considerable shift toward four-year public schools of 4.4 percentage points, which is about the same

Table 2.6	**Effect of All Southern Merit Programs on Schooling Decisions of Eighteen-to-Nineteen-Year-Olds (all Southern states; $N = 13{,}965$)**				
	College Attendance (1)	2-Year Public (2)	2-Year Private (3)	4-Year Public (4)	4-Year Private (5)
No time trends					
Merit program	.047	−.010	.004	.044	.005
	(.011)	(.008)	(.004)	(.014)	(.009)
R^2	.046	.030	.007	.030	.020
State time trends					
Merit program, year 1	.024	−.025	.009	.034	.010
	(.019)	(.012)	(.005)	(.012)	(.007)
Merit program, year 2	.010	−.015	.002	.028	−.001
	(.032)	(.018)	(.003)	(.035)	(.011)
Merit program, year 3 and after	.060	−.037	.005	.065	.022
	(.030)	(.013)	(.003)	(.024)	(.010)
R^2	.047	.031	.009	.032	.022

Notes: Specification is that of column (3) in table 2.2, with the addition of state time trends where noted. Sample consists of eighteen-to-nineteen-year-olds in Southern Census region. Estimates are similar but less precise when sample is limited to Southern merit states. Standard errors in parentheses.

as the overall attendance effect. There are no effects on other choices of college. As was discussed earlier, this is probably the result of equal-sized shifts toward and away from two-year public schools, by students on the margin of college entry and four-year-college attendance, respectively. The time trend specification gives similar results, although here there is more indication of a net drop in the probability of attendance at two-year public colleges.

2.6.2 Do All Merit Aid Programs Have the Distributional Impact of HOPE?

Many of the merit programs are quite new. Of the seven programs examined in table 2.5, three had been operative for fewer than four years by 2000. In this section, I examine the four more mature programs—those of Georgia, Florida, Arkansas, and Mississippi—in greater depth. An advantage of focusing on the older programs is that these states have sufficient postprogram observations to allow for the finer cuts of the data needed to examine heterogeneity in the effect of aid across demographic groups. Given the strong impact of HOPE on the racial/ethnic gap in schooling, it is of interest to examine whether the other programs have had a similar impact.

In table 2.7, I examine how the effect of the four programs varies by race

Table 2.7   **Effect of Merit Aid on College Attendance Analysis by Race and Ethnicity (October CPS, 1988–2000; Southern Census region)**

	Georgia ($N = 8,999$) (1)	Florida ($N = 10,213$) (2)	Arkansas ($N = 8,949$) (3)	Mississippi ($N = 8,969$) (4)
No time trends				
Merit program	.096	.001	.054	.002
	(.014)	(.022)	(.023)	(.011)
Merit · black/Hispanic	−.030	.077	.045	.120
	(.020)	(.021)	(.026)	(.032)
R^2	.059	.055	.061	.058
Time trends				
Merit program	.140	.030	.060	.016
	(.013)	(.021)	(.024)	(.015)
Merit · black/Hispanic	−.147	.000	.043	.083
	(.039)	(.030)	(.043)	(.033)
R^2	.056	.052	.059	.055

Notes: Specification in "No time trends" is that of column (3) in table 2.2. Specification in "Time trends" adds trends estimated on pretreatment data. In each column, separate trends are included for four groups: white-control, white-treat, nonwhite-control and nonwhite-treat. In each column, sample consists of eighteen-to-nineteen-year-olds in Southern Census region, excluding states (other than the treatment state) that introduce a merit program by 2000. Standard errors in parentheses.

and ethnicity. The control group is the nonmerit states. I show the results of specifications that do and do not include preprogram time trends.[30] While the estimates do change when time trends are included, and some are quite imprecisely estimated, a consistent story emerges from the table. The estimates are in concord with those of table 2.5, which showed that each of these four programs had a strong impact on the college attendance rate. However, table 2.7 shows that the *relative* effects on blacks and Hispanics differ substantially across programs. In particular, Georgia is an outlier in its relatively low effect on blacks and Hispanics, as compared to its effect on whites.

Georgia's HOPE has had the largest impact of all the state programs on the college attendance of whites, with the estimated effect ranging from 9.6 to 14.0 percentage points (without and with time trends, respectively). Analogous effects in the other states are substantially smaller, with no state's estimates for white non-Hispanics larger than 6 percentage points. Further, the effect of Georgia HOPE on blacks and Hispanics is 3.0 to 14.7 points *lower* than the effect on whites. In the other three states, the estimated effect of merit aid on blacks and Hispanics is consistently *more positive* than its effect on white non-Hispanics.

This is an important finding, as Georgia's is the only program whose distributional effect has been examined in depth, and the assumption has been that, in other states, merit aid would similarly widen the racial gap in college attendance (see, e.g., Cornwell, Mustard, and Sridhar 2003 and Dynarski 2000). The results in table 2.7 indicate that the other mature merit aid programs have not had this effect, with nearly all of the estimates suggesting that merit aid has actually *narrowed* the gap.

Why is Georgia different? Its HOPE Scholarship diverges from the other three programs in two key dimensions. First, of the four programs analyzed in table 2.7, Georgia's has the most stringent GPA requirements. Georgia requires a high school GPA of 3.0, while Arkansas and Mississippi require a GPA of only 2.5. Florida's high school GPA requirement is similar to Georgia's, but its renewal requirements are less stringent. While Georgia requires that a HOPE scholar maintain a GPA of 3.0 in college, in Florida a college GPA of 2.75 allows a student to keep the scholarship. A college GPA of 2.75 also qualifies a student for renewal in Arkansas, and only a 2.5 is required in Mississippi.

Scholarship renewal rates for blacks are substantially lower than those of whites in Georgia, indicating that the college GPA requirement hits them particularly hard. Blacks at the University of Georgia are twice as likely as whites to lose their scholarship after the freshman year (Healy

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30. In the analysis of each program, four preprogram trends are estimated: two for white non-Hispanics (one for the treatment state and one for the control states) and two for blacks/Hispanics (one for the treatment state and one for the control states).

1997). A study at the Georgia Institute of Technology also found that blacks were substantially more likely than whites to lose their scholarships. This differential disappeared after accounting for differences in ability (as measured by SAT scores; Dee and Jackson 1999). More generally, since blacks and Hispanics have relatively low high school and college grades, less stringent GPA requirements will disproportionately benefit this group.

A second key difference between HOPE and the other state programs is its treatment of other sources of aid and associated paperwork requirements for students potentially eligible for aid. During the period under analysis, HOPE was reduced dollar for dollar by a student's other aid, and low-income students were required to fill out extensive paperwork in order to establish their eligibility for other aid. The net impact of these requirements was that lower-income students had to work harder for less aid than their well-off counterparts.[31] In stark contrast, Arkansas gives *larger* awards to low-income students, by allowing students who receive the Pell to keep their Academic Challenge Scholarships and by excluding from eligibility students from families with incomes above $55,000.[32]

2.7 Additional Effects of Merit Aid on Individuals and Institutions

The analysis in this paper has focused on the effect of merit aid on two critical margins: the decision to attend college and the type of college chosen. I have touched on another outcome that is quite important, at least to legislators: the decision to attend college within one's home state. I have found that merit aid moderately increases college attendance and shifts students from two-year schools toward four-year schools. The data also suggest that Georgia's merit aid program has increased the probability that a student will attend college in his or her home state. It remains to be determined whether merit aid keeps those students in state after they have completed their education, which is the ultimate goal of legislators who hope to use merit aid to staunch a perceived "brain drain." It also remains to be settled whether the merit programs have increased completed schooling, as opposed to attempted schooling.[33]

There are many other margins of behavior that merit aid may affect.

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31. Georgia recently eliminated this aspect of its program. As more data become available, it will be of interest to examine whether this change has altered the distributional impact of HOPE.

32. This is the income cutoff for a family of four. Median income for a family of four in Arkansas is $45,000, so a large share of students falls under these income guidelines.

33. Data limitations, rather than conceptual difficulties, hamper the analysis of this particular margin of behavior. At a minimum, we require data on the completed schooling of adults, along with information about the state in which they graduated high school. As of 2002, these data are not available in any survey large enough for informative analysis of the existing merit programs. The 2000 Census microdata may prove useful in this context, and I am currently examining this question using these data.

Thoroughly addressing all of these potential effects would expand this lengthy chapter into a book. Here I will provide a necessarily brief discussion of these issues.

2.7.1   Additional Effects of Merit Aid on Individuals

A goal of merit aid is to increase academic effort in high school and college. The carrot of merit aid may cause students to work harder in high school and college in order to qualify for and then maintain their scholarships. This increased effort would be reflected in higher grades, test scores, and college attendance rates. However, observed academic performance may also improve for unintended reasons, in that pressure from students and parents on teachers may lead to grade inflation at both the high school and college level.

A small literature has examined the effect of merit aid on academic effort. Henry and Rubenstein (2002) show that the average high school GPA of freshmen entering the Georgia public universities rose from 2.73 in 1992 to 2.98 in 1999. In order to test whether this increase reflects greater effort or grade inflation, they examine SAT scores of entering freshmen, which are not subject to the same parental and student pressures as high school grades. The authors find that the average SAT score of entering freshmen in Georgia rose along with grades after HOPE was introduced, from 968 to 1010. While these results are suggestive, they are not conclusive, since this study examines only students in Georgia. It is quite possible that the increases in grades and SAT scores in Georgia are part of a broader secular trend rather than a consequence of HOPE.

Grades at the college level may also be affected by merit aid. First, students may work harder in their courses in order to keep their scholarships. This is an intended effect of the merit programs. Two unintended effects may also increase college grades. Professors may feel pressured to give higher grades so as not to threaten their students' continued eligibility for HOPE, and students may choose less demanding course loads for the same reason. Note that determining whether merit aid increases effort in college is inherently difficult. While the SAT is a well-accepted metric of the preparation of high school students, there is no equivalent instrument used to measure the achievement of college students.

Whether due to increased effort, less demanding course loads, or grade inflation, college grades at the University of Georgia are on the rise, with the proportion of freshman grades below a B dropping from 40 percent in 1993 to 27 percent in 1996 (Healy 1997). In New Mexico, Binder and Ganderton (2002) found support for the hypothesis that this is due, in part, to students taking fewer courses per semester, and therefore concentrating more effort on each course. They found support for the hypothesis that students respond to a merit program by taking on less-demanding course loads. They found that credit hours per semester dropped after the Success

program was introduced. This work on New Mexico is the only conclusive empirical research regarding the question of effect of merit programs on academic effort in college.

Even the largest estimates of the effect of merit aid on schooling decisions suggest that the great majority of aid goes to inframarginal families—that is, to families whose schooling decisions are unaffected by their receipt of aid.[34] For these families, of interest is which margins of behavior *are* affected by the windfall receipt of scholarship funds. Do students use these funds to reduce the number of hours they work while in school? Do they increase their spending on leisure activities? Do families save the money, for retirement or later bequests to their children? One study suggests that at least part of the money is used for increased current consumption. Cornwell and Mustard (2002) examine new car registrations in Georgia and comparison states and find that car purchases rose faster in Georgia after the introduction of HOPE than before. They reach similar conclusions by examining the correlation between car registrations and the number of HOPE recipients at the county level within Georgia, finding an elasticity of new car registrations with respect to HOPE recipients of about 2 percent.

2.7.2   Impact of Merit Aid on Institutions

Dynarski (2000) compares the cost of attendance (room, board, tuition, and fees) at four-year schools in Georgia to that in the rest of the Southeast. She concludes that prices rose faster at public schools in Georgia than in comparable states after HOPE was introduced. Long (2002) subjects this question to a more thorough analysis, controlling for college selectivity and state characteristics. She separately examines the various components of the cost of attendance: tuition, room and board, and institutional financial aid. She finds that the increase in posted schooling prices in Georgia is fully explained by increases in room and board, which are not covered by HOPE. Further, she finds that institutional financial aid dropped as a result of the introduction of HOPE. Long hypothesizes that schools may have been under pressure from the state not to raise tuition, since any increases here would have to be met by increased HOPE outlays. Increases in room and board and drops in aid, however, could slip by with less attention. Private schools faced no such incentives to manipulate the form taken by their price increases, and accordingly their price increases are more evenly divided between tuition and room and board after HOPE.

Cornwell, Mustard, and Sridhar (2003) provide insight into how a merit

34. It is important to note that merit aid is not unique in this way. Estimates of the effects of other forms of student aid also indicate that aid largely goes to those whose observable schooling decisions are unaffected by the receipt of aid. Targeting of subsidies is a classic topic of public economics; there is no transfer program that is 100 percent effective in limiting its subsidy to those whose decisions are contingent on the receipt of the subsidy.

aid program affects the composition of institutions of higher education. They examine enrollment data for two- and four-year colleges in Georgia and the rest of the Southeast. Their empirical results show how the changing schooling choices of Georgia's young people translated into major shifts in the demographic composition of Georgia's schools. They find that enrollment expanded after the introduction of HOPE, relative to enrollment in comparable states. They also find a sharp rise in the enrollment of black students at Georgia's four-year colleges. Given the relatively small increase in the college attendance rate of blacks found in the present analysis, their increased presence at Georgia's four-year colleges probably reflects a shifting of black students from out-of-state colleges to Georgia schools.

2.8   The Politics and Finance of Merit Aid

State merit aid programs grew during the 1990s, a period characterized by strong economic growth and overflowing state coffers. Recently, merit programs have begun to feel the pinch of the recent economic downturn. As state legislators struggle to balance their budgets, merit aid programs dependent upon legislative appropriations (Arkansas, California, Louisiana, Maryland, and Mississippi) find themselves in direct competition with other state priorities such as elementary and secondary education and health care. Arkansas, the first state to introduce a broad-based merit aid program, has temporarily closed the program to new enrollees. Although current scholarship recipients can renew their awards, no new students are being admitted to the program. Funding for Louisiana's program barely avoided the chopping block during the state's last legislative session.

Those merit programs with committed revenue streams have been relatively buffered from the economic and political effects of the recession. Six states (Florida, Georgia, New Mexico, West Virginia, South Carolina, and Kentucky) fund their programs with revenues from a state lottery, while two (Nevada and Michigan) use funds from the tobacco litigation settlement. With their dedicated funding sources, merit aid in these states is not vulnerable to legislators seeking to cut spending in the face of sinking tax revenues. This puts merit aid in a unique position, since other sources of funding for higher education at the state level are not protected in the same way. For example, public universities are experiencing leaner times this fiscal year as their state appropriations are reduced. Aid for low-income students is also vulnerable. West Virginia's need-based aid program could not deliver scholarships to all those low-income students who were eligible during the 2002–2003 academic year. The same year, the state's new merit program, which has no income cap, was launched with full funding.

A similar dynamic has emerged at the federal level. The fastest-growing subsidies for college students—tax credits, savings tax incentives, and loans—are programs whose funding is not contingent upon legislative ap-

propriation. By contrast, spending on the Pell Grant program, which funds the most needy students, is determined by annual legislative appropriation.

While lottery funding protects merit aid funding from downturns in tax revenue and associated drops in appropriations, using lotteries to fund merit scholarship is a particularly regressive form of redistribution. The high-achieving college students who receive merit funds are relatively likely to be white and from upper-income families. Lottery spenders, by contrast, tend to be disproportionately concentrated in the bottom of the income distribution. Through both the revenue and spending channels, then, lottery-funded merit programs are regressive in their impact.

Why have merit aid programs spread so rapidly and maintained such strong political support? One possibility is that merit aid is a politically astute way to build support for spending on postsecondary education. Consider three alternatives for subsidizing college: merit aid, subsidized public tuition, and need-based aid. Merit aid has a political advantage over low tuition in that it has a high profile. Parents (voters) generally do not understand that the public university tuition they pay is kept artificially low by state appropriations to the schools. As a result, they may be unsympathetic to legislative efforts to increase funding through this route. If, instead, their child receives a "scholarship" that pays for tuition, the perceived benefit is personal and immediate, inducing political support for the spending. This gives merit- and need-based aid a political edge over tuition subsidies as politically viable methods of subsidizing college costs.

A second dynamic gives merit aid an edge over the other subsidy options. Since students "earn" merit aid, families may feel a more personal connection to the program and fight for its continuation. In this way, a merit program is akin to Social Security: In both cases, voters are fiercely supportive of transfers that they perceive as earned rewards rather than unconditional entitlements.

A third political advantage of merit aid, again held in common with Social Security, is that it is broad based in its constituency. In most states, students of any income level qualify for a merit scholarship as long as they earn the required grades. All families are therefore potential recipients of, and political supporters of, merit aid scholarships. By contrast, the bulk of need-based aid flows to a relatively narrow slice of the population. The price of this highly progressive spending on need-based aid is that many voters do not perceive themselves as its potential beneficiaries. William Julius Wilson (1987) and Theda Skocpol (1991) have argued that robust welfare states are characterized by benefits that are widely available and, therefore, widely supported. They argue that means-tested antipoverty programs are politically weak because their scope is narrow. A similar dynamic could explain strong political support for merit-based aid paired with weak political support for need-based aid.

Do these political realities indicate that a progressive aid system is po-

litically unviable? Skocpol and Wilson point out that politically popular "universal" programs can provide political cover for redistributive transfers. As Social Security shows, a universal program can be layered with transfers that channel extra dollars toward those with greater need. This does not necessarily require new spending, as existing need-based programs could simply be relabeled in a way that enhances their political viability. For example, federal need-based grants could be delivered to needy students through the tax system by making the Hope and Lifetime Learning tax credits refundable.[35] This would eliminate one layer of paperwork (the FAFSA) yet allow aid eligibility to still be determined with the detailed financial information that is provided in tax filings. More important, funding for low-income students would be shifted into a program with broad-based political appeal and a guaranteed funding stream.

2.9   Conclusion

This paper has examined how merit aid programs in seven states have affected an array of schooling decisions, with particular attention to how the effects have varied by race and ethnicity. I find that merit aid programs typically increase the attendance probability of college-age youths by 5 to 7 percentage points. The programs are therefore effective at getting more students into college. In fact, as I discuss presently, the merit programs appear to be *more* effective than need-based aid at achieving this goal.

The merit programs also shift students toward four-year schools and away from two-year schools. Why? Four-year colleges are far more expensive than two-year colleges, but merit aid programs generally reduce the direct cost (tuition and required fees) of each option to zero. It is therefore expected that a greater proportion of students would choose the four-year option than they would in the absence of merit aid. An open question is whether this shift toward four-year colleges is socially beneficial. Four-year colleges are more expensive to run than two-year colleges, so a shift toward these schools will increase the total cost of educating college students. Further, marginal students who cannot handle the rigors of a four-year college may drop out of school altogether, whereas at a two-year institution they may have received the support they needed to persist. A countervailing factor is that some students who would not have considered going on for a BA will do so once they are enrolled in a four-year school.[36]

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35. Those who assail the need-based aid system for its complex application process will probably be horrified by this suggestion, as the federal tax system is also notoriously complex. But the Earned Income Tax Credit has proved to be an effective mechanism for transferring money to low-income families, and a refundable education tax credit has the potential to do the same for low-income students.

36. Rouse (1995) addresses the effect of community colleges on college entry and completion.

The current analysis does not allow us to address which of these effects dominates.

The merit programs also appear to close racial and ethnic gaps in schooling, at least in three of the four states whose programs are old enough to allow analysis by race. Merit aid programs in Arkansas, Florida, and Mississippi have closed gaps, with Georgia's the only program to widen them. I attribute the Georgia program's unique distributional effect to its relatively stringent academic requirements and a recently eliminated provision that channeled the most generous scholarships to higher-income students. This leaves open the question, however, of why merit aid does not simply have a race-*neutral* effect on schooling in states that do not have Georgia's unusual provisions.

One possible explanation for the role of merit aid in closing gaps in schooling is the simplicity and transparency of these programs. First, these programs are well publicized, and knowledge among potential recipients is unusually high; one survey found that 70 percent of Georgia high school freshmen could name the HOPE program without prompting, while 59 percent could identify its eligibility requirements (Henry, Harkreader, Hutcheson, and Gordon 1998). Second, unlike need-based aid, merit aid programs have minimal application procedures, and the applicant knows at the time of application both whether he is eligible and the amount of the award. By contrast, need-based aid requires that the applicant complete a complicated set of forms and wait for months to find out the actual award amount, which is a complicated function of family finances.

Collecting information about college costs and completing application forms may be particularly challenging to parents for whom English is a second language or who have not gone to college themselves. A program with low transaction and information costs may therefore find a particularly large response among nonwhite, low-income populations. This strong response among the eligible may more than compensate for the fact that a smaller proportion of nonwhites meet the academic requirements of merit aid.

This interpretation of the present results is consistent with a set of studies that have shown little effect of the need-based Pell Grant on schooling decisions (e.g., Kane 1995; Hansen 1983) but a large effect of simpler, more transparent subsidy programs (e.g., Dynarski [2003] on the Social Security student benefit program and Kane [1994] on tuition prices). Kane and Hansen both find no impact of the need-based Pell Grant on college attendance. By contrast, Kane, in his 1994 study, finds that tuition prices have a substantial impact on college attendance. Dynarski finds that the Social Security student benefit program, which had minimal application requirements, had a large impact on college attendance and completed schooling.

Whereas a benefit of a program with few paperwork requirements is that it may move more youths into school, a cost is the loss of targeting. Unlike

the Pell Grant, a merit aid program subsidizes many middle- and upper-income students. A merit aid program is therefore relatively more costly to run than need-based aid. However, a merit aid program is no more costly than subsidized public tuition prices, which also benefit students regardless of income. Further, as was discussed earlier in this chapter, merit aid has a substantial advantage over both need-based aid and subsidized tuition in that it has a broad and loyal base of political support in states that have introduced the programs.

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Comment   Charles Clotfelter

Susan Dynarski has written a well-crafted analysis of the effect of state merit aid programs on college attendance. She has employed variation over time and across states in the utilization of merit aid programs to provide very credible estimates of their enrollment effects. Rather than pursue points already well developed in her paper, I will note two aspects of the general topic that I suspect were not really part of Dynarski's charge in writing her chapter but that nonetheless warrant further reflection by policy analysts and researchers. One is the distributional impact of these programs, and the other concerns the wider array of effects emanating from them and programs like them.

Before the introduction of the "new breed of merit aid," states offered fi-

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nancial aid largely in the form of low tuition levels and easy geographic accessibility. Rather than devise means-tested financial aid of the form used in federal aid programs or by private institutions using the federally endorsed "uniform methodology," most states have eschewed individually tailored aid in favor of low tuitions across the board. As Hansen and Weisbrod (1969) showed, however, this seemingly populist policy—combined with a pattern of subsidies that favored elite public universities and admissions standards that caused eligibility to be correlated with parental income—has the effect of aiding the affluent rather than the poor. This was the dominant policy of states until Arkansas, and then, most prominently, Georgia, introduced a new form of state financial aid, the Helping Outstanding Pupils Educationally (HOPE) Scholarship. Fully consistent with the nation's (and especially the South's) infatuation with using tangible rewards for spurring educational achievement and enabled by the revenues produced by its new state lottery, Georgia offered a striking new carrot to its high school students: achieve a B average and receive in return a full-tuition scholarship to any state college or university. (Those enrolling in private institutions in the state received a stipend to cover a limited amount of tuition.) As Dynarski makes clear, the program's required grade point average meant that a higher percentage of whites than blacks were eligible to receive support. Though data on the incomes of students were not available, it was clear that this program also had a pro-middle-income impact reminiscent of the California low-tuition policy studied by Hansen and Weisbrod (1969). Two other features increased this tendency: the Pell Grant recipients had their state awards reduced by the amount of these grants, and the income ceiling on eligibility for the HOPE Scholarship Program was eliminated in the program's second year. Add to these pro-affluent aspects of the expenditure side of the program the regressivity of the implicit tax on the revenue side, and you have a rather stunning distributional impact. Putting aside whatever pro-poor effect there might be in legalizing the lottery, the policy choice to finance this merit aid program with a heavy implicit tax paid disproportionately out of lower incomes is quite remarkable. To be sure, Georgia appears to be an outlier in the way it financed and designed its merit aid program. But it is probably safe to say that one effect of the new breed of merit aid is a small but real redistribution of income.

A second point that Dynarski's paper moves me to mention is the rather uncontroversial assertion that enrollment effects, as important as they may be, are only one of a number of effects likely to emanate from these new state aid programs. In fact, Dynarski mentions several types of effects. She notes, for example, that the programs are likely to affect not only the propensity to enroll in college (and, more specifically, the propensity to go to four-year institutions) but also students' choices among institutions.

With several thousand new dollars in pocket (dollars that cannot not be spent just anywhere, however), aspiring college students might well be expected to make different choices than they would have in the absence of the program. She notes as well that merit programs, as a result of new patterns of enrollment, might influence the racial composition of institutions. And, she notes, merit aid programs could impact other forms of aid provided by states or, indeed, other state policies. A final effect that she mentions, one in line with the so-called Bennett hypothesis that "greedy colleges" would respond to increases in aid simply by raising tuition, is the possibility that a generous new merit aid program might inspire institutions, both public and private, to raise their tuitions.

This said, I would argue that there are yet other effects that might result from the introduction of this new breed of merit program. Because eligibility for these scholarships is contingent on a high level of academic performance in high school, one might surely expect such a program to influence the effort expended by students in high school. In light of the financial rewards available, we might also expect parents to offer encouragement to their high school children beyond the normal level of parental hectoring. Once in college, successful scholarship holders must confront the prospect of further academic performance requirements for them to retain their scholarships. Thus one would reasonably expect another set of effects, including those on the amount of effort devoted to study and on the choice of a major. Average grades awarded in various departments can differ significantly within a single college, and it should not be surprising that undergraduates pay attention to such differences, especially when financial repercussions are added to the other consequences of making low grades. For their part, institutions might respond to these pressures by allowing grades to inflate. Effects on the choice of major and on grades suggest another effect—the likelihood that students will stay on to graduate.

Another set of effects would arise out of the altered composition of student bodies. If, as appears to be the case, these programs raise the average academic qualification of students at some state schools, the learning environments there could be altered, depending on what kinds of peer effects are at work. The changes in composition might also affect the institutions' ability to recruit and retain talented faculty. One might also imagine that the surge in demand by qualified students might cause some institutions to confront questions such as whether to establish new or revise existing enrollment caps.

Susan Dynarski's chapter represents a useful and insightful contribution to a volume focusing on decisions about college. She shows that one new form of state aid program, one based on measured achievement rather than financial need, has affected the decisions of many college applicants about whether and where to attend college. My comments have touched on two

related sets of questions that I view as interesting extensions, not important omissions.

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