Finishing College:

The Role of Schooling Costs in Degree Attainment

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Half of college students drop out before completing a degree. While there is strong evidence that financial aid can increase college *attendance,* there is little evidence that it increases degree completion or even years of completed schooling. This is an important evidentiary gap, since it is completed schooling that is rewarded by the labor market. I exploit the introduction of two large state financial aid programs to estimate the impact of aid on completed schooling. I find that the aid programs increase the share of the population that completes a college degree by three percentage points. The effects are strongest among women, with white, non-Hispanic women increasing degree receipt by 3.8 percentage points and the share of Hispanic and nonwhite women attempting or completing any years of college increasing by six and eight percentage points, respectively. While my estimation strategy cannot separately identify the effect of aid on entry and persistence, I establish fairly tight bounds on the persistence effect, concluding that the aid programs increase the college persistence rate by 6 to 11 percent.

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I. Introduction

This paper examines whether subsidies to college costs can induce more people to obtain a college degree. Careful empirical studies of the impact of tuition, merit aid, Pell Grants and other grant programs have all concluded that decreasing college costs substantially increases the college attendance rate of young people.[1] However, we know comparatively little about how financial aid affects completed schooling.[2] There is enormous heterogeneity in college outcomes. Only half of those who every attend college complete a degree: in the 2000 Census, just 57 percent of young people with any college experience have completed an associate's degree or bachelor's degree.[3] Thirteen percent of those who attempted college did not complete even a year of post-secondary schooling. Understanding the impact of aid policy on completion is therefore critical to understanding its welfare consequences, since it is completed schooling that is rewarded by the labor market.

Theory does not unambiguously predict the impact of financial aid on completed schooling, even once we know the impact of aid on college attendance. In Manski's framework (1989), students learn about their academic skills when they attend college and, should they find them lacking, drop out. Marginal decreases to college costs may induce students with low expectations of success to undertake this experiment. An alternative (but not incompatible) story is that the marginal college student may be credit-constrained. Relaxing credit constraints may then induce into college individuals with better

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[1] See Kane (1994) on tuition, Dynarski (2000, 2004) on merit aid, Seftor and Turner (2002) on Pell Grants and Dynarski (2003) on the Social Security student grant program. Dynarski (2002) reviews this literature.

[2] Dynarski (2003) and Bettinger (2004) present suggestive evidence that aid increases completed schooling. Angrist (1993) and Bound and Turner (2002) show that veterans' educational benefits increase completed schooling.

[3] Among 22- to 34-year-olds with a high school diploma in the 2000 Census, 69 percent had taken some college courses. Of these college entrants, 13 percent had not completed *any* years of college, while 30 percent finished a year or more without receiving a degree. Only 57 percent completed an AA or a BA. The focus on degree completion, rather than years of completed schooling, reflects data constraints imposed by the Census data.

academic skills than the typical college student. These two stories predict very different impacts of aid on education of the population: the first suggests it will lead to large increases in college attendance but little change in degree completion, while the latter suggests that aid may have substantial effects on both margins of behavior.

This paper exploits the fortuitous timing of two events to identify the effect of student aid on completed schooling. First, a dozen states introduced large-scale merit aid programs during the 1990s. These programs waive tuition and fees for students who achieve a minimum GPA in high school, typically 3.0, and maintain a minimum GPA in college, typically 2.5 to 3.0. Arkansas started the trend with the first broad-based merit program, with Georgia following suit in 1993. In previous work, I have shown that these programs have had a positive impact on college attendance (Dynarski, 2000 and 2004). In those papers, data limitations prevented the estimation of the effect of merit aid on college completion. These data limitations have been relaxed by a second fortuitous event, the release of the 2000 decennial census microdata. As of 2000, several cohorts who were exposed to the merit aid programs were sufficiently old (at least 22, the traditional college-leaving age) to allow the comparison of their completed education to those of earlier, ineligible cohorts.

To preview the results, I find a large and significant impact of these subsidies on degree receipt. I find that the aid programs increase the share of the population that completes a college degree by three percentage points. The effects are strongest among women, with white, non-Hispanic women increasing degree receipt by 3.8 percentage points and the share of Hispanic and nonwhite women attempting or completing any years of college increasing by six and eight percentage points, respectively. Among men, BA receipt is the most responsive margin.

Estimates that control for observable characteristics and underlying trends in unobservable determinants of degree completion return quite similar results. So do estimates that use as the control group the entire US, the south alone, and states that ever introduce a merit aid program. In every case, the estimates indicate that the merit aid programs increased the share of the young, working age population receiving a college degree by about three percentage points. Correcting for mis-classification in treatment status increases this point estimate to 3.3 to 5.1. Alternative methods of computing standard errors on the key parameters all produce statistically significant results.

These results indicate that aid policy can play a substantial role in increasing the labor-market share of those with a college degree. While my reduced-form estimation strategy cannot separately identify the effect of aid on entry and persistence, I can estimate fairly narrow bounds on the persistence effect. The merit aid programs appear to increase by five to eleven percent the probability of persistence to degree of those who would have gone to college in the absence of a merit aid program – that is, of inframarginal college entrants. Future research will explore the effects of these exogenous shifts in completed schooling on wages, health, family formation and other outcomes that are theoretically affected by completed schooling.

The paper is organized as follows. Section II provides a literature review and background on the state subsidy programs that will be the subject of the analysis. Section III lays out the identification strategy and describes the data. Section IV provides results and robustness checks. Section V explores heterogeneity in the program's impact. Section VI discusses the results and Section VII concludes.

II. Background

This section discusses the relevant economic literature and provides background on the programs that will be the subject of the empirical analysis.

College Costs and College Completion

Financial aid plausibly affects several margins of behavior: college attendance, college entry, and degree completion.[4] Dozens of studies have examined the relationship between college costs and these

[4] College attendance is a state variable, indicating that at a given point in time a person is enrolled in college. Entry and completion are stock variables, indicating that a person has ever attended college or has completed college, respectively.

outcomes.[5] Almost all are plagued by identification problems, with the analyses failing to control for correlation between college costs and unobserved determinants of schooling outcomes.[6]

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

The evidence on the effect of aid on college persistence and completion is comparatively thin. Angrist (1993) and Bound and Turner (2002) show that veterans' educational benefits increase completed schooling. In her examination of the elimination of the Social Security student benefit program, Dynarski (2003) finds a drop in the schooling of the affected population of two-thirds of a year, but this result is imprecisely estimated. Bettinger (2004) uses regression-discontinuity methodology to examine the effect of the Pell Grant on persistence rate of college entrants and finds a positive effect, but notes that his

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[5] Leslie and Brinkman (1988) review these studies.

[6] Dynarski (2002) discusses issues of identification in this literature.

results are quite sensitive to specification.[7] The contribution of the present paper is to provide estimates of the effect of aid on persistence and degree completion that are precisely estimated and quite robust to specification and functional form.

State Merit Aid Programs

Since the early Nineties, more than a dozen states have established broad-based merit aid programs. While most states have some form of merit aid, these programs have traditionally subsidized only the highest-performing students. For example, New York rewards each high school's top scorer on the Regents exam with a scholarship. The elite students who receive these scholarships are unlikely to alter their decision to complete a degree based on whether they receive the subsidy.

The newer merit programs are open to larger numbers of students who are of modest academic ability. The typical program awards tuition and fees to young residents who have maintained a modest grade point average in high school. 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.[8] The Arkansas program requires a high

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[7] Bettinger finds that a $1,000 increase in Pell Grant eligibility increases persistence between the first and second years of college by two to four percentage points. Translating his results into the current context requires several very strong assumptions: 1) the effect of aid is linear in the amount of aid 2) the effect of aid on the hazard of dropping out is constant across years of college and 3) the population whose behavior is affected by the merit programs is as elastic in price as the population whose behavior is affected by the Pell Grant.

With the implausibility of these assumptions firmly in mind: the value of the merit scholarships examined in the paper is roughly $3,000. Bettinger's estimates would therefore predict that the merit aid programs would increase persistence in a given year of college by six to twelve percentage points. Assume a baseline hazard of dropping out in a given year of college of 29 percent, which corresponds to the empirical four-year survival rate of 36 percent (=Pr(BA)/Pr(any college)=0.25 /0.69). Decreasing this hazard by six percentage points (from 29 to 23 percent) corresponds to an increase in the four-year survival rate of college entrants from 36 percent to 46 percent. Since 69 percent of the population enters college, this is equivalent to an increase in the share with a BA of about seven percent, more than twice the magnitude of the paper's estimates.

[8] The merit programs require continued academic performance 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 GPA of 2.5, a standard met by 60 percent of high school students nationwide.[9] The state also requires a minimum on the ACT of 19, a score exceeded by 60 percent of test-takers nationwide and well below the Arkansas state average of 20.4. In short, the new, broad-based merit aid programs are open to students with solid though not necessarily exemplary academic records. Many of these students may be on the margin of completing a college degree, and so these programs are a fruitful source of variation for examining this particular outcome.

Table 1 describes the dozen state merit aid programs whose eligibility criteria are sufficiently lenient that at least thirty percent of high school seniors have grades and test scores that qualify them for an award. As the table shows, merit aid is heavily concentrated in the South. Of the thirteen states with broad-based merit aid programs, nine are in the South. 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. Many of the merit programs are too new to detect effects in currently available data. Merit programs introduced by 1993 (those in Arkansas and Georgia) provide the identifying variation in the analysis.

The Arkansas program was proposed in January 1991 by then-Governor Bill Clinton. The program was quickly approved by the legislature and was in place in time for the high school class of 1991 to receive scholarships upon college enrollment in the fall. Arkansas high school graduates for the classes of 1991 forward with 19 on the ACT and a cumulative GPA of 2.5 in a set of core classes were eligible for the scholarship. Continued receipt required a GPA of 2.5 in college and at least 24 semester hours a year. At its inception the program paid tuition and required fees up to $1,000 at public and private colleges in Arkansas; this was raised to $1,500 in 1994 and $2,500 in 1997. As a basis of comparison, tuition and required fees were $2,200 in 1994 for University of Arkansas at Fayetteville, the state's flagship institution, and $1,110 for Arkansas State University at Beebe. Starting in 1994, students could

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[9] Author's calculations from the 1997 National Longitudinal Survey of Youth. This is the share of students with a *senior year* GPA of at least 3.0, and so is likely an upper bound on the share of students who achieve this GPA for their entire high school career.

get an annual bonus of $500 if their previous year's college grades were 3.0 or above. The program had a family income cap on participation of $35,000 for a family with two children; this was raised to $40,000 in 1993 and $75,000 in 1999.

Georgia's program was proposed in early 1993 by then-Governor Zell Miller. This program, too, moved quickly from proposal to legislation, with the high school class of 1993 eligible for the first scholarships, which covered tuition and fees at Georgia public colleges or $1,000 at a Georgia private school. The private school scholarship was raised gradually to $3,000 as of 1995. Initial eligibility requires a 3.0 GPA in high-school core curriculum classes, while renewal requires maintaining a 3.0 GPA in college. The scholarship is available for a maximum of four years.

III. Empirical Methodology

The empirical strategy is straightforward. I use a treatment-control approach, comparing educational outcomes in states that do and do not introduce merit aid programs. States that never introduce merit programs, or introduce them after the period under analysis, constitute the control group. Relative changes in educational attainment in the states that introduce merit aid identify the effect of merit aid. I use the 2000 census for the bulk of the analysis, using the 1990 census for some robustness checks.

Treatment status is assigned based on year and state of high school graduation. The key variable of interest is a dummy variable, *merit*, which indicates whether a person would have been eligible for a merit aid program upon high school graduation. For example, those who graduated from high school in 1991 and after in the state of Arkansas were exposed to that state's scholarship program. Those who graduated before 1991 were not eligible; the program was not grandfathered. Similarly, those who graduated in 1993 and after in Georgia were exposed to merit scholarship programs.

The census provides neither state nor year of high school graduation. In the main analysis, I use age at the time of the census survey to assign the year of high school graduation, which is assumed to occur at age 18. For example, an individual who was 27 in spring 2000 was 18 in spring 1991, and so is assigned to be a high school senior in 1991. This is an imperfect proxy, since in the spring of their senior year many high school students are 19.[10] In one specification, I use CPS data on the age distribution of high school seniors to probabilistically assign the year of high school graduation and, thereby, treatment status.

In the main analysis, I use state of birth to assign state of high school attendance. For this purpose, state of birth is preferred over state of residence because the latter may be endogenously determined by the treatment: individuals may migrate to college and then settle in the state in which they go to school. However, a drawback of using state of birth as a proxy for state of high school attendance is that about twenty percent of high-school-age youth live outside their state of birth. This classification error will tend to bias the estimates downward. In one specification, I use high school students in the Census to estimate a matrix of transition probabilities between state of birth and state of residence. I apply this matrix to the analytical sample in order to probabilistically assign the state of high school graduation and, thereby, treatment status.

Sample Definition

My main analytical sample consists of 22- to 34-year-olds in the 2000 census. The lower cutoff is chosen because it is a traditional age of college-leaving, and so is a reasonable age at which to begin measuring degree completion. The upper cutoff is more arbitrary, and was chosen to provide seven years of data for the years preceding the introduction of the oldest program in 1991. Using the year a person was 18 to assign year of high school graduation, these age cohorts correspond to the high school classes of 1984 through 1996.

[10] Information on quarter of birth would allow a more accurate assignment, but this variable is unavailable in the 2000 Census.

I limit the sample to those who currently live in, and were born in, the United States.[11] The results do not change if immigrants are included. Since the merit programs are largely a Southern phenomenon, I also check the sensitivity of the results to the choice of control group by limiting some analyses to the Southern Census Region.[12] Observations for which age, state of birth or completed education is imputed are dropped from the analysis. Analyses that include the imputed values produce substantively similar results. I do not use the census sample weights in the analysis. Use of the weights does not substantively alter the results.

Variable Definitions

At the college level, the census mainly measures degree completion. The 2000 census did not collect data on years of college attempted or completed. Rather than attempt to convert the categorical variable into a continuous one, I examine a series of dichotomous outcomes. I will mainly focus on whether an individual has received a college degree, either an associate's or bachelor's. Additional results will demonstrate the impact of the merit aid programs on the entire distribution of education, by estimating the program effects for all sixteen categories of the 2000 education variable.

Measures of race and ethnicity are included in some specifications. Mutually-exclusive indicator variables define individuals as Hispanics of any race, Black non-Hispanics, white non-Hispanics, and other non-Hispanics. The unemployment rate in an individual's state of birth at the time the person was 18 is matched onto the Census, using data from the Bureau of Labor Statistics. Table 2 contains the

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[11] Mississippi's program, introduced in 1996, is old enough to affect AA completion in 2000 but too young to affect BA completion. To simplify the analysis I have removed Mississippi from the sample; its inclusion does not alter the results substantially.

[12] This region includes the South Atlantic states (Delaware, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia and West Virginia, plus the District of Columbia); the East South Central states (Alabama, Kentucky, Mississippi and Tennessee); and the West South Central states (Arkansas, Louisiana, Oklahoma and Texas).

means of the key variables, listed separately by the individual's state of birth - Arkansas, Georgia, the rest of the South, and the rest of the United States.

Specification

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

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

Here, y_{iab} is a measure of the completed education of person *i* of age *a* born in state *b*. δ_b and δ_a denote state of birth and age fixed effects, respectively, and ε_{iab} is an idiosyncratic error term. The identifying assumption of this equation is that any relative increase across age cohorts in the schooling of those born in merit aid states is attributable to the merit program itself. If the identifying assumption is correct, β is the increase in education associated with exposure to a merit aid program. In much of the analysis I will perform this regression in cell means measured at the level of state of birth and age cohort. As discussed later in the paper, this approach will allow me to estimate consistent standard errors:

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

The source of identification in Equations (1) and (2) is the interaction of state and age, which defines eligibility for the merit aid programs. This approach is vulnerable to bias from underlying trends at the state level in educational outcomes. I therefore test the robustness of the results to the inclusion of trends in education. Alternatively, by adding another year of data, in the form of the 1990 census, I am also able to include the interaction of state and age effects, thereby identifying the program with the interaction of state, age and year. Both of these approaches are discussed in greater detail when they are implemented.

IV. Results and Robustness Checks

I begin with a visual inspection of the data that provide the identifying variation in the paper. Figure 1A plots the rate of degree completion in Georgia and the rest of the United States (excluding

Arkansas). The data are plotted by the year in which a census 2000 respondent would have been eighteen, so the points on the right represent the youngest members of the sample, who were 22 at the time of interview. Since this is a single cross-section, these graphs reflects both time and age variation in education. In particular, the downward slope on the right side of each line reflects the fact that degree completion rises sharply at young ages.

The shape of the US age-education profile provides the counterfactual for the paper's estimates. The identifying assumption is that Georgia's cohorts would have tracked the US cohorts in the post-program period the same as in the pre-program period. Note that Georgia and the control states roughly track each other among the pre-program age cohorts, with the share of the sample holding a college degree consistently lower in Georgia. This gap varies between six and ten percentage points; the year-to-year fluctuations are largely driven by Georgia, whose series is (unsurprisingly) noisier than that of the US.

The two series cease tracking each other with the first post-program cohort. For the control states, there is a smooth decline among these cohorts in the share holding a college degree: 22-year-olds are about 12 percentage points less likely to hold a college degree than 26-year-olds.[13] By contrast, the rate of degree completion in Georgia does not hold to this downward trend: degree completion is *higher* for the first post-program cohort than for the cohort that is one year older. The same is true for the second post-program cohort in Georgia: its rate of degree completion is higher than that of the cohort that graduated from high school two years earlier.

As a result, there is considerable post-program convergence between the two series. With the third eligible cohort the two series continue roughly in parallel. The identification of paper's estimates is driven by this convergence in the two series, and an even more striking convergence between Arkansas

[13] This is likely reflects not a cohort effect, but rather an age effect, with many people obtaining their first college degree in their mid- to late twenties. Turner (2004) documents that degree completion continues through the twenties and into the early thirties.

and the rest of the US, seen in Figure 1B. In both of these figures, the sharp convergence in the first program year supports the identifying assumption of the paper.

Baseline Results and Alternative Calculations of Standard Errors

Table 3 presents the results of estimating Equation (1). The coefficient of 0.0298 in Column (1) indicates that the share of individuals completing a college degree rose by 2.98 percentage points among those born in merit states, relative to those born in the rest of the US, for cohorts graduating college after a merit program was introduced. The unadjusted standard error, in brackets, is 0.96 percentage points.
This standard error does not account for the fact that the treatment varies not at the level of the individual but at the level of age cohorts within states. Allowing for arbitrary variance-covariance in errors within state-age cells corrects this problem, and the resulting standard errors are in brackets.[14] The standard error rises to 0.96.

As discussed by Bertrand, Duflo and Mullainathan (2004), even these adjusted standard errors are inconsistent, as they do not account for any serial correlation in outcomes within states across cohorts. Positive auto-correlation will produce unadjusted standard errors that are too small while negative auto-correlation will produce standard errors that are too large. The most straightforward solution to this problem is to collapse the data into state-age means and re-run the estimating equation, adjusting the resulting standard errors for clustering within state of birth. Monte Carlo simulations in Bertrand, Duflo and Mullainathan (2004) show that this approach produces hypothesis tests of the correct power.

These results are in Column (2) of Table 3. The regression is weighted by the size of the population within each cell; the slope coefficient is therefore identical to that obtained from the regression run in the micro data. The standard error drops by more than half, to 0.40 percentage points.

An alternative approach is to eliminate the time series variation in the data, which also eliminates the serial correlation. If Arkansas and Georgia had introduced their programs in the same year, this would

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[14] I use the cluster command in Stata to estimate these errors.

involve grouping the means by state of birth and pre/post-program, then running the regression in these means. The equivalent in the current context is to regress the outcome against state and year effects, form residuals, and for the treatment states only do a before/after comparison of the residuals. With this approach, the standard error drops even further, to 0.25 percentage points.

The estimates of the standard errors are summarized in Table 4. In this particular application, the coefficients are large enough, and even the largest estimates of the standard error small enough, that in no case would I fail to reject the null. However, accounting for serial correlation within states has a substantial impact on precision: the 95% confidence interval is four percentage points wide when unadjusted and one to two percentage points wide when adjusted.

Why are the standard errors so much smaller when adjusted for serial correlation? To check on the degree and sign of auto-correlation, I obtain residuals from a regression of *degree* on state-of-birth and age fixed effects (thereby isolating the variation that identifies the program effect). I regress these residuals against their first, second and third lags. The estimated first order auto-correlation coefficient is 0.05 and is not significant. The second and third auto-correlation coefficients are negative: -0.02 and -0.03, respectively. By comparison, Bertrand, Duflo and Mullainathan (2004) estimate first-, second- and third-autocorrelations in CPS wage data of 0.51, 0.41 and 0.33, respectively. The structure of their data produces unadjusted standard errors that are too small, while the structure of my data produces unadjusted standard errors that are too large. In the remainder of the paper, I will estimate regressions in grouped means (which accounts for any correlated shocks at the level of the state-year), with the standard errors then adjusted for autocorrelation within states.

Robustness to Choice of Control Group

If southern age cohorts are trending away from their peers in rest of the US in their rate of degree completion, then the US will be a poor control group for Arkansas and Georgia. In this case, limiting the sample to the south will return less biased estimates. In Table 5, I test the robustness of the results to the choice of control group. In the first column, the sample is limited to those with a southern state of birth.

The estimates are quite close to the estimates that include the non-Southern states in the control group: 2.8 percentage points for the South as compared to 2.9 for the US.

We might suspect that the merit states are somehow different from the non-merit states, and that the non-merit states therefore form a poor control for these purposes. We can test this by dropping from the sample the states that never introduce merit aid. In this approach, the states that ever introduce a merit program form their own control group. In Column (2), I limit the sample to those states that had introduced a merit program by 2003. The resulting estimate is 2.98, with a standard error of 0.48 percentage points. The point estimate is extremely close to that estimated from the larger Southern sample, but is more precisely estimated. In the last column, I drop the non-Southern merit states from the analysis (Michigan, Nevada and New Mexico); the estimate drops slightly, to 2.3 percentage points, with a standard error of 0.6 percentage points.

Falsification Exercise

To make clear that the identification in the paper is from state of birth, rather than current state of residence, Table 6 shows the results of re-estimating the equations with state of *residence* determining treatment status. That is, current state of residence, rather than state of birth, is assumed to be the state in which a person graduated from high school. These estimates are insignificant and close to zero: 0.49 and 0.01 percentage points when the entire US and the South, respectively, are used as the control groups. Both are insignificant.

These results strongly support the identification strategy of the paper. The states introducing these programs are not simply states to which the well-educated are migrating. In fact, since the estimates of the paper indicate that the merit programs are inducing more young people to complete college, the null results of Table 6 necessarily imply that either 1) these educated workers are subsequently emigrating from the merit states or 2) relatively uneducated workers are immigrating into the merit states. Which is the case is important from the state's perspective: it is quite different to educate a college graduate and have her leave than to have her stay and produce positive externalities. I leave the exploration of this interesting set of questions to a separate paper that explores the political economy of state funding of higher education. The present paper will focus on the efficacy of higher education policy in getting more people to complete college, wherever those workers may take their human capital after graduation.[15]

Robustness to Inclusion of Covariates

We might suspect that the timing of the introduction of a merit program is correlated with a state's economic condition. A state flush with funds due to a booming labor market would be more willing to fund a merit aid program. This would induce a correlation between the introduction of a merit-aid program and completed schooling, since these unobserved labor market conditions will affect schooling decisions independent of their impact on schooling costs. A booming labor market increases the opportunity costs of college, which will tend to reduce the share of young people completing a degree. A pre-program booming labor market will also boost the income of parents of college-age children, which in the presence of liquidity constraints will tend to *increase* the share of young people completing a degree. The net effect on the present estimates is ambiguous.

In Figure 2A, I plot the unemployment rate of young people in Georgia and the rest of the South (excluding Arkansas). The unemployment rate in Georgia is lower than that of the rest of the Southern census region through this period. The two lines track each other roughly. Key exceptions are in 1991 and 1992, the last two pre-program years. In 1991, unemployment dropped by half a percentage point in Georgia but rose 1.1 percentage points in the rest of the South. The gap between Georgia and the rest of the South in labor market conditions thereby widened considerably. In 1992, however, a sharp rise in Georgia's unemployment rate largely closed this gap.

[15] The scenario of an inflow of uneducated workers is consistent with Moretti (2004), which shows that a college-educated workforce generates positive wage externalities for both skilled and unskilled workers. His theoretical framework suggests that a state with a growing share of college-educated workers could attract unskilled labor seeking out these wage benefits.

In Figure 2B, I show the analogous graph for Arkansas. While Georgia roughly tracked the rest of the South, Arkansas's unemployment rate shows a fairly steady downward trend over this period. In 1984, Arkansas's unemployment rate was 1.2 percentage points above that of the rest of the South, while in 1995 it was 0.7 percentage points lower. If labor market conditions primarily affect degree completion through opportunity costs, this relative drop in Arkansas's unemployment rate would tend to bias downward estimates of the effect of merit aid that do not control for labor market conditions. The opposite holds if liquidity constraints are the primary channel through which labor market conditions affect degree completion.

To test whether the labor market conditions depicted in these figures, or other observables, are biasing the estimates I control for a set of characteristics measured at the level of the individual and state-of-birth/age cohort. To implement this approach while still obtaining standard errors that properly control for correlation at the state level, I regress the degree variable against a set of controls in the microdata and form residuals. Equation (2) is re-estimated with means of these residuals as the dependent variable. Identification of the program effect in these regressions is then from the relative changes in degree receipt in the merit states, conditional on observable differences between the merit states and the control states.

The controls include the unemployment rate for the respondent's state of birth, measured in the year in which he was 18 years old. Because a state's racial and ethnic composition may be changing as merit programs are being introduced, and both race and ethnicity are correlated with education, I also add a set of dummies for race and Hispanic ethnicity to the regression. To absorb residual variation and improve the precision of the estimates, I also include a sex dummy and interact it with race and ethnicity. All of these variables are interacted with age dummies, so that their relationship with schooling decisions is allowed to vary flexibly over time.

Results for this specification are in Table 7. The estimate from the specification that uses unconditional means is reproduced in Column (1). After controlling for covariates the estimate drops slightly, from 2.98 to 2.75 for the US. The estimate is slightly more precise, so statistical significance is maintained. The next column conditions on state of residence. Again, the estimate is essentially unaffected (2.58). In the final column, I control for the college premium in the state of birth for each age cohort; the premium is measured in the year in which a cohort was 18.[16] This variable is intended to pick up supply and demand shifts in the skilled labor market that are not reflected in the unemployment rate. With the addition of this final control there is no loss in precision and the point estimate is 2.35 percentage points, only slightly lower than the estimate based on the unconditional means (2.98 percentage points).[17]

Robustness to Inclusion of Trends

The specifications just discussed control for observable differences between the treatment and control states. However, unobservable differences could still be biasing the estimates. For example, if the treatment states were trending positively relative to the control states in the pre-program era, then the estimates discussed in the previous sections will be biased upward. We informally evaluated this threat to validity when we examined Figures 1A and 1B earlier in the paper. A more formal way to approach this issue is to control parametrically for differential trends in college completion. A typical approach to controlling for this source of bias is to include linear time trends in the regression and identify the program effect from deviations from those trends. Figures 1A and 1B make clear, however, that the counterfactual trend is not linear, but rather quadratic. I test both functional forms, as well as a non-parametric approach.

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[16] I use 35- to 54-year-olds in the1984-96 March CPS to estimate these college premia. The premium is defined as the difference between the mean log wages of year-round, full-time workers with 1) exactly a high school degree and 2) a BA or above.

[17] Conducting the same exercise with the Southern census region alone yields substantively similar results; as with the full sample, adding covariates reduces the point estimate by roughly a standard error and does not affect precision.

In Column (2) of Table 8, I add a linear term in age, interacted with the state of birth dummies, to the baseline regression.[18] This specification allows each state to be on its own linear trend in degree completion, with the program effect identified by deviations from these trends. The results in the first row use the entire US as the control group. The point estimate drops only slightly, from 2.98 to 2.21 percentage points, but the standard error more than triples, to 1.36. A similar pattern holds for the Southern census region, with the estimate dropping from 2.78 to 2.45 percentage points and the standard error rising to 1.83.

A linear trend may be a poor counterfactual in the current context, since the underlying education profile is concave in age. Column (3) addresses this issue by adding the square of age, interacted with state of birth, to the regression. The estimate based on the US sample is unchanged, while the Southern sample estimate rises to 3.44 percentage points. In both cases the standard errors are quite large. For both the US and Southern control groups, with linear and quadratic state-specific trends, then, the story is the same: the point estimates are quite stable but precision is compromised.

The approaches just discussed impose functional form on the underlying trends. Estimating a separate set of age effects for each division or state of birth, by contrast, allows the profiles to take whatever form the data suggest. In Column (4), I interact division effects with age effects. This allows each division to have a non-parametric age-education profile. Note than Arkansas and Georgia are in separate divisions, so separate profiles are estimated for each. In this specification the identifying variation is the same for the Southern and US samples, and so the estimates are identical: 2.35 percentage points, with a standard error of 1.5 percentage points.

While these results are supportive of the identifying assumption of the paper, we may be concerned that states that do and do not introduce merit programs have age-education profiles that are idiosyncratically different, and that these differences are not captured by the linear or quadratic functional

[18] Conducting this exercise with residuals as the dependent variable, rather than unconditional means, does not alter the results substantially.

forms. A solution is to allow each state its own non-parametric age-education profile. This approach cannot be executed in a single cross-section, since the treatment is identified by the interaction of state of birth and age. However, with two cross-sections, we can estimate non-parametric age-education profiles for each state, while still identifying the program effect.

I turn to the 1990 census for the required data, drawing a sample identical to the one in the 2000 census: native-born US residents between the ages of 22 and 34. With this combined 2000 and 1990 sample, I can add to the specification a full set of state-of-birth-age effect interactions, as well as all other second-order interactions of census year, state of birth and age effects. The program effect is then identified by changes between 1990 and 2000 in the state-specific age-education profiles. That is, it is identified by the triple interaction of state of birth, age cohort and census year. This specification controls for state-specific changes between 1990 and 2000 in the average rate of holding a college degree (through the interactions of census year and state of birth) as well as any changes over time in the shape of the age-education profile that are common across the states (through the interaction of census year and age).

Results for the US and the South are in Table 9. The coefficients are imprecisely estimated, with standard errors of about two percentage points.[19] But for both the US and South the estimates rise slightly and converge: to 3.50 for the US sample and 3.32 for the South. Again, a strong test of the identifying assumption of the paper returns results consistent with the simplest empirical approach.

Accounting for Classification Error in Treatment Status

The analysis has so far used state of birth and age to assign eligibility for a merit aid program, with each individual imputed to be eligible with probability one or probability zero. There are two sources of error in this method of assignment to treatment status. First, in the 2000 census, 24 percent of high school students lived outside their state of birth. Assuming this rate has not changed substantially over

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

time, assignment of eligibility for merit aid (which is established by state of high school residence) is incorrect for about 24 percent of the analytical sample. Second, many students are younger or older than 18 in the spring of their senior year, typically ranging in age from 17 to 19. For those high school seniors who were younger or older than 18 at the time a merit program was introduced in their state, the paper has incorrectly imputed eligibility.

In this section of the paper I correct for both of these sources of error in the assignment of treatment status. I address misclassification due to migration first. So far, I have used state of birth as a *proxy* for state of high school attendance, ignoring the information provided by current state of residence. In this section, I test using state of birth to *predict* state of high school attendance, using this predicted state of high school attendance to assign treatment status. That is, I allow treatment status to be a probabilistic (rather than deterministic) function of state of birth.

To implement this strategy, I use high-school-age youth (15- to 17-year-olds) in the 2000 census to estimate a matrix of transition probabilities between state of birth and state of high school attendance. In principal, this matrix could have 51 X 51 cells, one for each transition. However, since attendance in only two states (Arkansas and Georgia) produces a treatment, it is more efficient to estimate a matrix with dimension 51 X 2, corresponding to 51 states of birth and high school attendance in Georgia or Arkansas.[20] I apply this matrix to the older sample (22- to 34-year-olds) to yield predicted state of high school attendance. The resulting predicted probabilities are used to define *merit*, which now ranges from zero to one, and the key estimating equation is re-run.[21]

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[20] Specifically, I run OLS regressions of the following form, where *i* indexes individuals and *b* indexes state of birth:

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

[21] The treatment dummy is now defined as the sum of the interactions of two predicted probabilities with age, e.g., for person of age *a* born in state *b*:

$$merit_{ab} = \Pr_b\text{ (high school in Arkansas)} \times (a\text{<=18 in 1991}) + \Pr_b\text{ (high school in Georgia)} \times (a\text{<=18 in 1993})$$

Results are in Table 10. The estimate rises from 2.98 to 3.72 percentage points. This is quite close to the error-corrected estimate that would be appropriate if migration to and from the merit states were random. Aigner (1973) and Freeman (1984) show that the relationship between the true coefficient and that estimate in the presence of classification error is:

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

where $\hat{\beta}$ is the coefficient estimated in the presence of measurement error and δ is the degree of classification error.[22] Nationwide, 24 percent of high school seniors live outside their state of birth, so the baseline estimate of 2.98 corresponds to a error-corrected estimate of 3.92 (=2.98 /0.76).[23]

I next allow treatment status to be a probabilistic function of age. I use the 1989, 1990 and 1991 School Enrollment Supplements of the October CPS to estimate age-specific probabilities of being a high school senior for those age 16 through 24.[24] I use these probabilities to impute for the analytical sample the probability of being a senior in each of the years from 1984 through 2000. The resulting predicted probabilities are used to define *merit*, which now ranges from zero to one, and the key estimating equation is re-run.[25]

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[22] This result may seem obvious, since classical measurement error is known to produce attenuation in regression coefficients. However, measurement error in binary variables is non-classical: if a zero is observed, the measurement error can only be non-negative and if a one is observed the measurement error can only be non-positive. This violates the standard assumption that the measurement error is uncorrelated with the truth. Note that the correction shown holds for bivariate regressions and in the special multivariate case in which the classification variable and its error are uncorrelated with other regressors.

[23] Later, when I examine heterogeneity in treatment effects by race, ethnicity and sex, I estimate separate transition matrices for each subgroup.

[24] The questions needed to determine whether a person is enrolled for a high school senior are available for these ages only.

[25] The treatment dummy is now defined as the sum of the interactions of two predicted probabilities with state of birth, e.g., for person of age *a* born in state *b*:

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

The resulting estimate is in Table 7, Row 3(a): 4.02 percentage points, with a standard error of 0.69 percentage points. Note that this should be interpreted as the effect of merit aid on degree completion of those who were exposed to a merit aid program *as high school seniors*. The paper's previous estimates have been of the effect of merit aid upon those exposed *at age 18;* of these only a subset ever made it to senior year of high school. That is, for each person, the sum across years of the probabilities of being a high school senior is less than one. The point estimates of the treatment effect are higher in Row 3(a) (4.02) than in Row (1) (2.98), in part because they are inflated by the inverse probability of ever being a high school senior. If we instead constrain the probability of every being a high school senior to equal one, the estimate is correspondingly lower: 3.33, with a standard error of 0.57 (Row 3(b)).

In Row 4(a) I allow treatment status to be a probabilistic function of *both* state of birth and age, yielding an estimated program effect of 5.07 percentage points, with a standard error of 0.81. Again, constraining the probability of ever being a high school senior to one yields a lower estimate (4.19 percentage points, seen in Row 4(b)). The error corrected estimates of the effect of merit aid on degree completion therefore lie between 3.3 and 5.1 (that is, the point estimates are increased by ten to seventy percent).

V. Heterogeneity in Program Effects

The robustness checks of the previous section establish a strong case for causal interpretation of the estimated effects. Estimates that control for observable characteristics and underlying trends in unobservable determinants of degree completion return quite similar results. So do estimates that use as the control group the entire US, the south alone, and states that ever introduce a merit aid program. In every case, the estimates indicate that the merit aid programs increased the share of the young, working age population receiving a college degree by about three percentage points. Correcting for mis-classification in treatment status increases this point estimate to 3.3 to 5.1.

Having established a strong case for causality, I next turn to examining the populations, and margins of behavior, that are most affected by the programs. I first examine whether the effect of the subsidies varies by demographic characteristics. I split the sample into four mutually-exclusive groups: non-Hispanic white or Asian men, non-Hispanic white or Asian women, Hispanic and/or nonwhite men, and Hispanic and/or nonwhite women. I then run the baseline specification separately for each of these subgroups. Any heterogeneity in the treatment effect across groups could be driven by a variety of factors that vary systematically across the population: preparation at the high school level, labor market opportunities, returns to schooling, parental education, and liquidity constraints. The estimates in this section will capture the reduced-form impact of any such heterogeneity upon the sensitivity of a group to the merit aid programs.

Results are in the first column of Table 11. Women show a much greater sensitivity to the subsidies than do men. Within sex, however, the effects are quite similar across demographic groups. The estimates (and standard errors) for women are 3.79 (0.48) percentage points for white, non-Hispanics and 3.45 (2.14) for nonwhites and Hispanics. For men, the corresponding estimates are 1.58 (0.92) for white, non-Hispanics and 1.60 (0.34) percentage points for nonwhites and Hispanics.

More variation in the effect of the subsidies emerges when we look more closely at the margins of college completion that are affected by the programs. I break the college degree variable into two categories: those who hold only a two-year associate's degree, and those who hold or a BA or above. Columns (2) and (3) show the results of regressions that estimate the program effect separately for these two margins. For the entire sample, the effect is concentrated on the BA margin: 2.52 percentage points, as compared to 0.46 for the AA margin.[26] A similar relationship holds for white, non-Hispanic women, for whom the BA outcome is more sensitive than the AA (2.29 as compared to 0.87). Among nonwhite and Hispanic women, however, the AA margin dominates: the estimated effect is 2.63 percentage points for the AA as compared to 0.82 for BA or above. Among men, the estimated AA effect is negative,

[26] Among pre-program cohorts born in the treatment states, about 26 percent holds a BA or above, while about six percent holds an AA only. The BA effect is therefore larger in both absolute value and proportional to baseline.

significantly so for nonwhites and Hispanics, indicating that the subsidies are shifting this group from AA receipt toward BA completion.

In addition to examining point-wise shifts in the distribution of education for these groups, we can examine the effect of merit aid on the entire distribution. This exercise is of interest for two reasons. First, it provides a specification check, in that we can confirm that there is no significant change in education at levels unaffected by the policy. Second, it allows us to hypothesize about the marginal students whose behavior is affected by the program.

I pinpoint changes in the full distribution of schooling using the methodology of the preceding section. The census education question has sixteen categories, ranging from no schooling to holding a doctorate. I create fifteen dummies, each indicating that an individual's level of education is greater than or equal to a given schooling category. I then run separate regressions for each of these outcomes. These coefficients, along with their point-wise 95 percent confidence intervals, are plotted in Figure 3A.[27] For reference, Table 12 lists these coefficients and their standard errors.

Points above the axis in Figure 3A indicate outcomes whose likelihood is estimated to increase upon the introduction of a merit aid program. Positive masses indicate the estimated program effect on the probability of being *equal to or above* that level of education. The point above AA, for example, is 2.98, the estimate, seen previously in the paper, of the impact of the program on the probability of receiving any college degree.

In Table 12 and Figure 3A, we can see that there is no statistically significant impact on the rate of high school graduation: the point estimate is actually negative (-0.59 percentage points) but statistically indistinguishable from zero. Since the merit programs both reward academic performance in high school and increase the option value of high school graduation, it is at least plausible that they would have affected the high school graduation rate. However, the results suggest that that the students whose

[27] Individual points on these figures are not independent, and these point-wise confidence intervals do not take this interdependence into account. Future iterations of the paper will calculate appropriate confidence bands.

behavior is affected by these incentives (those close to having a B average in high school) are not at the margin of dropping out of high school and therefore unresponsive to this incentive.

The estimate of 1.59 at less than one year of college indicates that the merit aid programs increase the college entry rate, but the estimate is not significant. There is a larger impact on persistence through the first year of college: there is an increase of 1.94 percentage points in the probability of completing at least some years of college. In the 2000 census, 9 percent of those 22 to 34 have entered college without completing a single year. These results suggest that the aid programs substantially reduced this outcome in the treatment states. As discussed in the bulk of the paper, there is an even larger impact on degree completion; the results suggest that the merit programs increase by 2.98 percentage points the share of the population that has any postsecondary degree, from a base of 33 percent. There is also a statistically significant impact upon completing any education *beyond* the BA, a point I discuss in the next section.

Breaking these effects out by race and sex again reveals substantial heterogeneity in the effect of merit aid on completed schooling. The effects for white women (Figure 3B and Table 12B) are concentrated at the degree completion margins (increases of 3.2 percentage points in the receipt of any college degree, and 2.3 in the receipt of a BA or above), but there are also substantial effects at college entry (1.2 percentage points) and the completion at least a year of college (1.5 percentage points). All of these estimates are highly significant.

For nonwhite and Hispanic women (Figure 3C and Table 12C) the largest effects are concentrated lower in the educational distribution, and were therefore masked in the analysis of degree completion. Effects are strongest for this group at college entry and completion of a few years of college. The probability of completing any years of college for nonwhite and Hispanic women is estimated to increase by 7.44 percentage points (with a standard error of 0.65 percentage points), and the probability of attempting college increases 5.99 percentage points.

For white, non-Hispanic men, the effects are concentrated on the college degree margins. The results for Hispanic and nonwhite men are quite noisy, with a substantial (but insignificant) drop in the probability that this group will complete high school. This may indicate the effect of instructional resources being shifted away from students on the margin of dropping out of high school, among which nonwhite and Hispanic men are disproportionately represented.

VI. Discussion

Together, these tables and figures provide strong evidence that the merit aid programs increased the completed schooling of eligible youth. The estimated program effects are robust to the inclusion of covariates, the choice of control group, and the inclusion of variables to control for pre-existing trends in educational attainment. The estimates are robust to the choice of sample and control group.

Examination of program effects on the cumulative distribution function show statistically significant effects at the margins of behavior affected by the policy. Merit aid is estimated to increase the college entry rate by 1.6 percentage points, the share who complete any years of college by 1.94 percentage points, and the share who complete any college degree by 2.98 percentage points, and the share who complete a BA or above by 2.52 percentage points.[28] All but the first of these estimates are highly significant. All of these margins are ones that are plausibly affected by the merit aid programs, which decrease the cost of both entering and persisting through college.

The statistically significant impact (1.37 percentage points) on the probability of having a degree higher than a BA is troubling. This estimate may indicate that all of the paper's results are spurious, driven by divergent trends in education in the merit and non-merit states. The robustness checks of the previous sections, especially the sharp shifts in Figures 1A and 1B and the state-age controls in Tables 8 and 9, have provided strong support for the case that the estimated coefficients have a causal interpretation. There are several plausible explanations for a positive program effect above the BA level.

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[28] Dynarski (2004), using the annual October Current Population Surveys, finds a five to seven percentage point impact of the merit programs on the contemporaneous *attendance* rate of 18- to 19-year-olds. This estimate is not directly comparable to any of the present estimates, since the attendance rate conflates two outcomes: entry and persistence conditional upon entry. Further, the contemporaneous CPS attendance questions may capture short college spells forgotten by those answering retrospective Census questions. Card and Lemieux (2001) note divergence between education of cohorts as documented by Census and the CPS.

First, theory suggests that the future human capital investments of a college graduate will be affected by her past schooling expenditures. In particular, future investment will depend on the cost of past investment if the price of debt rises with its level.[29] As a result, a student who exits college without debt because of her merit scholarship may be more willing to borrow to fund a degree in education or social work than she would have been otherwise. Second, in many graduate programs, courses completed at the baccalaureate level can count towards the master's degree, so receiving aid to complete a BA can move a student closer to completing an MA.

Finally, the tuition subsidies may cause people to speed up their human capital investments, and thereby reach the MA earlier in life than they would have in the absence of the subsidy.[30] Aid may allow students to focus fully on their studies rather than work while in school. If this is the case, then the effect of the tuition subsidies on all types of degree completion will fade out as cohorts age. At present, the programs are too young to test directly for this dynamic. Note that there are positive welfare effects to earlier degree acquisition, since it allows a longer work life over which to recoup the returns to human capital, so a later finding that the program is primarily operating on this margin would reduce but not negate its positive welfare effects.[31]

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[29] There is evidence that students and families face rising interest rates when borrowing for college. The cheapest source of funds for most families is federally subsidized student loans, with housing equity the next alternative. If housing equity has been exhausted, families can turn to unsubsidized federal loans. As a last resort, families can turn to more expensive sources of funds, such as unsecured personal loans, retirement savings and credit cards.

[30] Turner (2004) documents an historical increase in the time required to finish a degree. Since it is optimal to complete human capital investments early in life, slow completion is consistent with the presence of liquidity constraints. A program that loosens those constraints may significantly speed up degree acquisition.

[31] While a typical subsidy to college costs might be predicted to speed time-to-degree by reducing the need for students to work for pay, the merit programs' particulars may create the opposite incentive. Georgia caps the number of credits HOPE will pay for, but not the number of semesters, or number of years, over which these credits are earned. This would tend to encourage students to take light course loads, so that they can concentrate on a few courses and maintain their 3.0 GPA and their scholarship. Cornwell, Lee and Mustard (2004b) argue that the HOPE program has caused students to take on lighter course-loads in their early college career, though they appear to compensate with heavier loads in later

How Does Merit Aid Affect Persistence in College?

We cannot separately identify the effect of merit aid on college entry and persistence conditional on entry, because we cannot identify the marginal entrant. Instead, we have measured the reduced-form impact of merit aid on completed schooling, which is the product of effects upon entry and persistence. While we cannot nail down a precise persistence effect, we can place informative bounds on its size, since at one extreme all of the marginal entrants complete a degree and at the other extreme none do. Note that in the treatment states for the pre-program cohorts, 51.5 percent attempted any college, while 26.7 percent completed a degree, a baseline persistence-to-degree rate of 51.8 percent:

$$persist = \Pr(complete \mid enter) = \frac{\Pr(complete)}{\Pr(enter)} = \frac{0.267}{0.515} = 0.518$$

An upper bound on the effect on aid on persistence of inframarginal college entrants is formed as follows. Assume none of the marginal entrants complete a degree – that is, their persistence rate is zero. Then *all* of the three percentage point increase in degree completion is attributable to the increased persistence of inframarginal students. An upper bound on the impact of merit aid on persistence to degree of inframarginal college entrants is therefore 5.8 percentage points (=3.0/51.5).

A lower bound on the effect of aid on persistence of inframarginal entrants assumes that all of the marginal entrants completed a degree – that is, their persistence rate is one. Then 1.6 percentage points of the three percentage point increase in degree completion is attributable to the marginal college entrants, and the remaining 1.4 percentage points to the inframarginal college entrants. An upper bound on the impact of merit aid on persistence to degree of inframarginal college entrants is therefore 2.7 percentage points (=1.4/51.5).[32] Given a baseline persistence rate of 51.8 percent, these bounds on the increase in

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years. The Arkansas program, by contrast, limits the award to eight semesters of college, and so would encourage students to speed up their degree completion.

[32] If we assume that marginal entrants persist at the *same* rate as pre-program college students, the implied increase in the persistence rate for inframarginal college entrants is 4.3 percentage points.

persistence of 2.7 to 5.8 percentage points correspond to proportional increases in the college persistence rate of five to eleven percent (or, equivalently, correspond to decreases in the college dropout rate of six to twelve percent).

What Behavioral Parameter Has Been Estimated?

It is tempting to take the paper's estimated increases in degree receipt, combine them with information about the value of the subsidy, and thereby back out a demand elasticity. However, previous research (Dynarski, 2000 and Long, 2004) has shown that merit aid programs tend to lead to a moderate increase in tuition prices, which affects those who are not eligible for the merit aid programs. Further, merit aid may crowd out need-based aid. The results discussed are therefore the reduced-form effect of merit programs, net of any decrease in education due to rising prices for those not eligible for merit aid and crowd-out of need-based aid among those who are eligible.

These reduced-form estimates also reflect the effect of merit aid on effort in high school. The estimated effect is the product of two behavioral parameters: 1) π, the responsiveness of youth whose grades make them eligible for the scholarship and 2) δ, the share of youth eligible for the scholarship:

$$\beta = \pi\delta$$

Note that δ is not fixed – in fact, one purpose of merit aid is to encourage effort in high school. Merit aid may increase schooling both by altering price and by altering academic effort. In the present analysis, we cannot disentangle these two channels through which merit aid operates.

A drawback is that we have not measured the purely financial impact of the program (π), and so the results should be extrapolated to other subsidy programs with caution. It is not clear whether the academic requirements of the programs will tend to lead to larger or smaller college completion effects than a non-merit subsidy. The merit programs' academic requirements may push students to work harder in college, and thereby make them more likely to succeed. This may make these programs particularly effective at increasing degree receipt. Conversely, though, they may deny subsidies to many students who are on the margin of completing a college degree, but whose grades are too low to maintain the scholarship. The programs require a 2.75 to 3.0 GPA in college, well above the GPA required to graduate. This may make the programs less effective at encouraging degree completion that one that is targeted at a lower point in the distribution of academic achievement.

VI. Conclusion

This paper has provided the strongest evidence to date that subsidies to college costs can induce more people to complete college. The results are robust to the inclusion of covariates, including measures of labor market demand in state of birth when the college entry decision was being made. The inclusion of flexibly-specified state-specific trends in education does not alter the conclusions. Estimates that control for underlying trends in unobservable determinants of degree completion return quite similar results. So do estimates that use as the control group the entire US, the south alone, and states that ever introduce a merit aid program.

I find a large and significant impact of these subsidies on both degree receipt and college entry. The results suggest that merit programs increase college degree attainment by three percentage points. Correcting for mis-classification in treatment status increases this point estimate to 3.3 to 5.1 percentage points. This is a substantial effect, given that the baseline share of the affected population with a college degree was just 26 percent. The effects on schooling are strongest among women, with white, non-Hispanic women increasing degree receipt by 3.8 percentage points and the share of Hispanic and nonwhite women attempting or completing any years of college increasing by six and eight percentage points, respectively. Among men, BA receipt is the most responsive margin.

While my reduced-form estimation strategy cannot separately identify the effect of aid on entry and persistence, I estimate fairly narrow bounds on the persistence effect. The merit aid programs appear to increase by five to eleven percent the probability of persistence to degree of those who would have gone to college in the absence of a merit aid program – that is, of inframarginal college entrants.

These results indicate that aid policy can play a substantial role in increasing the labor-market share of those with a college degree. Future research will explore the effects of these exogenous shifts in completed schooling on wages, health, family formation and other outcomes that are theoretically affected by completed schooling.

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Table 1
State Merit Aid Programs, 2003

State	Start	Eligibility	Award *In-State Only*
Arkansas+	1991	initial: 2.5 GPA in HS core & 19 ACT renew: 2.75 college GPA	public: up to $2,500* private: same
Florida	1997	initial: 3.0-3.5 HS GPA & 970-1270 SAT/20-28 renew: 2.75-3.0 college GPA	public: 75-100% tuition/fees* private: 75-100% avg public tuition/fees*
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* private: same
Louisiana	1998	initial: 2.5-3.5 HS GPA & ACT > state mean renew: 2.3 college GPA	public: tuition/fees + $400-800* private: avg public tuition/fees*
Maryland	2002	initial: 3.0 HS GPA in core renew: 3.0 college GPA	2-yr school - $1,000 4-yr school - $3,000
Michigan	2000	initial: level 2 of MEAP or 75th pctile of renew: NA	in-state: $2,500 once out-of-state: $1,000 once
Mississippi	1996	initial: 2.5 GPA & 15 ACT renew: 2.5 college GPA	fresh/soph: $500 jr/sr: $1,000
Nevada	2000	initial: 3.0 GPA & pass Nevada HS exam renew: 2.0 college GPA	public 4 yr: tuition/fees (max $2,500) public 2-yr: tuition/fees (max $1,900)
New Mexico	1997	initial: 2.5 GPA 1st semester of college renew: 2.5 college GPA	public: tuition/fees private: none
S. Carolina	1998	initial: 3.0 GPA & 1100 SAT/24 ACT renew: 3.0 college GPA	2-yr school - $1,000 4-yr school - $2,000
W. Virginia	2002	initial: 3.0 HS GPA in core & 1000 SAT/21 renew: 2.75-3.0 college GPA	public: tuition/fees private: avg public tuition/fees
Tennessee**	2003	initial: 3.0 college GPA or 890 SAT/19 ACT renew: 2.75 - 3.0 college GPA	2-yr school - $1,500 or tuition and fees 4-yr school - $3,000 or tuition and fees

*award varies with test score or GPA
** Award valid at all institutions in the Southern Association of Schools and Colleges

Table 2
Sample Means, by State of Birth
22- to 34-year olds
Census 2000, 1% Sample

	Arkansas	Georgia	Rest of South	Rest of US
Any College Degree	0.223	0.260	0.282	0.337
Associate's Degree Only	0.058	0.060	0.068	0.083
Bachelor's Degree or Above	0.165	0.200	0.214	0.254
Nonhispanic White or Asian	0.786	0.682	0.707	0.780
Unemployment Rate in State of Birth, at Age 18	0.072	0.056	0.066	0.064
N	3,467	9,225	97,321	332,347

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

Table 3
OLS Estimates of Effect of
Merit Aid Programs on College Degree Attainment
22- to 34-year-olds, 2000 Census

	US		
	(1) *Micro Data*	(2) *Cell Means, Weighted by N_{ab}*	(3) *Cell Means, unweighted*
Merit Aid Program	0.0298	0.0298	0.0319
	(0.0082)	(0.0040)	(0.0040)
	[0.0096]		
Age Fixed Effects	Y	Y	Y
State of Birth Fixed Effects	Y	Y	Y
N	345,039	650	650
Mean of Y	0.33	0.33	0.33

Notes:
Micro regression: Unadjusted standard error in parentheses; standard error adjusted (in Stata, clustered) for correlation within state of birth and age in brackets.
Cell-mean regressions: standard errorr adjusted for correlation within state of birth.Regression in Column (2) weighted by number of observations within the age-state of birth cell.

Table 4
Robustness of Statistical Inference

		US	South
		(1)	(2)
(1)	**I. Micro Data**	**0.0298**	**0.0278**
	Alternative approaches to computing standard error		
(2)	no adjustment for autocorrelation	(0.0082)	(0.0085)
(3)	arbitrary variance-covariance in error term, by state of birth	(0.0038)	(0.0074)
(4)	arbitrary variance-covariance in error term, by state of birth & age	(0.0096)	(0.0085)
(5)	**II. Unconditional Cell Means**	**0.0298**	**0.0278**
	Alternative approaches to computing standard error		
(6)	no adjustment	(0.0100)	(0.0091)
(7)	arbitrary variance-covariance in error term, by state of birth	(0.0040)	(0.0079)
(8)	**III. Eliminate Time Series Variation**	**0.0282**	**0.0244**
(9)	before/after comparison of treatment state residuals	(0.0025)	(0.0021)

Notes: All methods of estimating the standard errors account for heteroskedasticity.
I. Micro Data: Specification consists of degree regressed on merit dummy, state of birth fixed effects and age fixed effects.
II. Unconditional Cell Means: Same as above, but data aggregated to state-of-birth by age cells and regression weighted by cell size.
III. Eliminate Time Series Variation: Cell means regressed on state of birth and age fixed effects, and resulting residuals (for treatment states only) regressed against merit dummy.

Table 5
Robustness Check: Choice of Control Group
Weighted cell means

	Southern States	States that Ever Introduce Merit Aid	Southern States that Ever Introduce Merit Aid
	(1)	(2)	(3)
Merit Aid Program	0.0278 (0.0079)	0.0298 (0.0048)	0.0229 (0.0060)
Age Fixed Effects	Y	Y	Y
State of Birth Fixed Effects	Y	Y	Y
N	208	156	117
Mean of Y	0.28	0.28	0.27

Notes:
Micro regressions: standard errors adjusted for correlation within state of birth in parentheses; within state of birth and age in brackets.
Cell-mean regressions: weighted by cell size with standard errors adjusted for correlation within state of birth.

Table 6
Falsification Exercise
Assignment of Treatment Status Based of State of *Residence*

	US		South	
	(1)	(2)	(3)	(4)
	State of Birth	*State of Residence*	*State of Birth*	*State of Residence*
Merit Aid Program	0.0294 (0.0039)	0.0049 (0.0149)	0.0268 (0.0078)	0.0014 (0.0176)
Age Fixed Effects	Y	Y	Y	Y
State of Birth Fixed Effects	Y		Y	
State of Residence Fixed Effects		Y		Y
N	650	650	208	208

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

Table 7
Robustness Check: Control for Covariates

	US			
	(1) Baseline	(2) Covariates	(3) State-of-residence FE	(4) Control for Return to College
Merit Aid Program	0.0298	0.0275	0.0258	0.0235
	(0.0040)	(0.0035)	(0.0040)	(0.0041)
Covariates		Y	Y	Y
Race/hispanicity, female, race/hispanicity X female, unemployment rate in state of birth when 18; all covariates interacted with age dummies				
State of residence fixed effects			Y	Y
Return to college				Y
N	650	650	650	650

Notes: Dependent variable is state-age mean residual from a regression of degree against the indicated covariates. Regressions are weighted by cell size and standard errors are adjusted for correlation at the state level. Return to college is for state of birth in year individual was 18 and is calculated from the 1984-96 March CPS among 35-54-year-olds; see text for details.

Table 8
Robustness Check:
Linear, Quadratic and Non-Parametric Controls for Underlying Trends

	Baseline	*State of Birth X Age Controls*		*Census Division X Age Controls*
		Linear	Quadratic	Nonparametric
	(1)	(2)	(3)	(4)
United States	0.0298 (0.0040)	0.0221 (0.0136)	0.0216 (0.0209)	0.0235 (0.0145)
South	0.0278 (0.0079)	0.0245 (0.0183)	0.0344 (0.0235)	0.0235 (0.0149)

Notes: Dependent variable is state-age mean of degree. Regressions are weighted by cell size and standard errors are adjusted for correlation at the state level. All regressions include state-of-birth and age fixed effects. Specification in Column (2) includes a separate linear trend in age for each state of birth. Specification in Columns (3) includes a separate *quadratic* trend in age for each state of birth. Column (4) includes a full set of age-effect X division-effect interactions.

Table 9
Robustness Check:
Non-Parametric Controls for Age, by State of Birth

	US		South	
	(1)	(2)	(3)	(4)
	Baseline	*Control for State-of-Birth X Age*	*Baseline*	*Control for State-of-Birth X Age*
	2000	*1990 & 2000*	*2000*	*1990 & 2000*
	0.0298	0.0350	0.0278	0.0332
	(0.0040)	(0.0205)	(0.0079)	(0.0218)
Age Dummies X State of Birth		Y		Y
Age Dummies X Census Year		Y		Y
Census Year X State of Birth		Y		Y
N	650	1,300	208	416

Notes: Standard errors adjusted for heteroskedasticity and correlation within state of birth and census year All regressions are weighted by cell size and include state-of-birth and age fixed effects.

Table 10
Correct for Measurement Error in Treatment Status
US Sample, 22-34-year-olds

(1)	Baseline	0.0298 (0.0040)
(2)	Predict State in Which High School Senior	0.0374 (0.0054)
(3)	Predict Year in Which High School Senior	0.0336 (0.0075)
(4)	Predict State and Year in Which High School Senior	0.0428 (0.0086)

Notes:

Row (1): *Treatment status is a deterministic function of age and state of birth.*
Individuals are assumed to attend high school in state of birth and be a high school senior at age 18.

Row (2): *Treatment status is a deterministic function of age and a probabilistic function of state of birth.*
High-school age (15- to 17-year-old) individuals in the 2000 census are used to estimate a transition matrix between state of birth and state of residence. Matrix is used to estimate the probability of residence in a merit-aid state during high school for the analytical sample.

Row (3): *Treatment status is a probabilistic function of age and a deterministic function of state of birth.*
1989-1991 School Enrollment Supplements of the October CPS are used to estimate age-specific probabilities of being a high school senior among 16- to 24-year-olds. Separate probabilities are estimated by sex-race. These probabilities are used to estimate the probability of being a senior in each of the years from 1984 through 2000 for the analytical sample. The sum of these probabilities is constrained to equal one.

Row (4): *Treatment status is a probabilistic function of age and a probabilistic function of state of birth.*
Methods of both Row (2) and Row (3) are used to predict treatment status.

Table 11
Heterogeneity in Treatment Effects By Race, Ethnicity and Sex
US Sample

	(1) Any College Degree	(2) BA or above	(3) AA Only
Full Sample	0.0298	0.0252	0.0046
	(0.0040)	(0.0044)	(0.0025)
White Non-Hispanic Women	0.0379	0.0229	0.0087
	(0.0048)	(0.0050)	(0.0020)
Nonwhite and Hispanic Women	0.0345	0.0082	0.0263
	(0.0214)	(0.0138)	(0.0082)
White Non-Hispanic Men	0.0158	0.0193	-0.0035
	(0.0092)	(0.0050)	(0.0093)
Nonwhite and Hispanic Men	0.0160	0.0279	-0.0120
	(0.0034)	(0.0047)	(0.0028)

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

Table 12A
Program Effect by Level of Education
22- to 34-year-olds, US born

Effect on probability that education is ***greater than or equal to…***	coefficient	se
No schooling		
Nursery to 4	-0.0014	(0.0003)
5-6	-0.0010	(0.0003)
7-8	-0.0014	(0.0006)
9	0.0015	(0.0007)
10	-0.0017	(0.0048)
11	-0.0056	(0.0050)
12, no diploma	-0.0064	(0.0087)
12, diploma	-0.0059	(0.0056)
< 1 year college	0.0159	(0.0100)
some college, no degree	0.0194	(0.0042)
AA	0.0298	(0.0040)
BA	0.0252	(0.0044)
MA	0.0137	(0.0047)
Prof Degree	0.0046	(0.0020)
PhD	0.0012	(0.0003)

Table 12B
Program Effect by Level of Education
22- to 34-year-olds, US born
Non-Hispanic White Women

Effect on probability that education is ***greater than or equal to…***	coefficient	se
No schooling		
Nursery to 4	-0.0013	(0.0004)
5-6	-0.0011	(0.0005)
7-8	-0.0015	(0.0013)
9	-0.0010	(0.0008)
10	-0.0080	(0.0043)
11	-0.0146	(0.0033)
12, no diploma	-0.0140	(0.0029)
12, diploma	-0.0110	(0.0062)
< 1 year college	0.0123	(0.0037)
some college, no degree	0.0151	(0.0071)
AA	0.0316	(0.0048)
BA	0.0229	(0.0050)
MA	0.0134	(0.0054)
Prof Degree	0.0046	(0.0028)
PhD	0.0014	(0.0004)

Table 12C
Program Effect by Level of Education
22- to 34-year-olds, US born
Hispanic and Nonwhite Women

Effect on probability that education is ***greater than or equal to…***	coefficient	se
No schooling		
Nursery to 4	0.0000	(0.0016)
5-6	0.0006	(0.0015)
7-8	0.0019	(0.0025)
9	0.0069	(0.0029)
10	0.0078	(0.0022)
11	0.0002	(0.0035)
12, no diploma	0.0084	(0.0064)
12, diploma	0.0132	(0.0071)
< 1 year college	0.0599	(0.0132)
some college, no degree	0.0744	(0.0065)
AA	0.0346	(0.0214)
BA	0.0082	(0.0138)
MA	-0.0032	(0.0029)
Prof Degree	-0.0023	(0.0012)
PhD	0.0013	(0.0004)

Table 12D
Program Effect by Level of Education
22- to 34-year-olds, US born
Non-Hispanic White Men

Effect on probability that education is ***greater than or equal to…***	coefficient	se
No schooling		
Nursery to 4	-0.0011	(0.0007)
5-6	-0.0012	(0.0007)
7-8	-0.0028	(0.0017)
9	-0.0005	(0.0026)
10	-0.0019	(0.0108)
11	-0.0042	(0.0108)
12, no diploma	0.0043	(0.0103)
12, diploma	0.0052	(0.0038)
< 1 year college	0.0086	(0.0156)
some college, no degree	0.0080	(0.0067)
AA	0.0158	(0.0092)
BA	0.0193	(0.0050)
MA	0.0143	(0.0064)
Prof Degree	0.0062	(0.0026)
PhD	0.0011	(0.0004)

Table 12E
Program Effect by Level of Education
22- to 34-year-olds, US born
Hispanic and Nonwhite Men

Effect on probability that education is ***greater than or equal to…***	coefficient	se
No schooling		
Nursery to 4	-0.0038	(0.0014)
5-6	-0.0019	(0.0011)
7-8	-0.0019	(0.0027)
9	0.0047	(0.0043)
10	0.0017	(0.0036)
11	0.0037	(0.0059)
12, no diploma	-0.0387	(0.0186)
12, diploma	-0.0531	(0.0296)
< 1 year college	-0.0199	(0.0119)
some college, no degree	-0.0195	(0.0064)
AA	0.0160	(0.0034)
BA	0.0279	(0.0047)
MA	0.0106	(0.0038)
Prof Degree	0.0014	(0.0009)
PhD	-0.0002	(0.0010)

Figure 1A: Proportion Holding a College Degree, by Age Cohort, 2000 Census

Line indicates last pre-program year

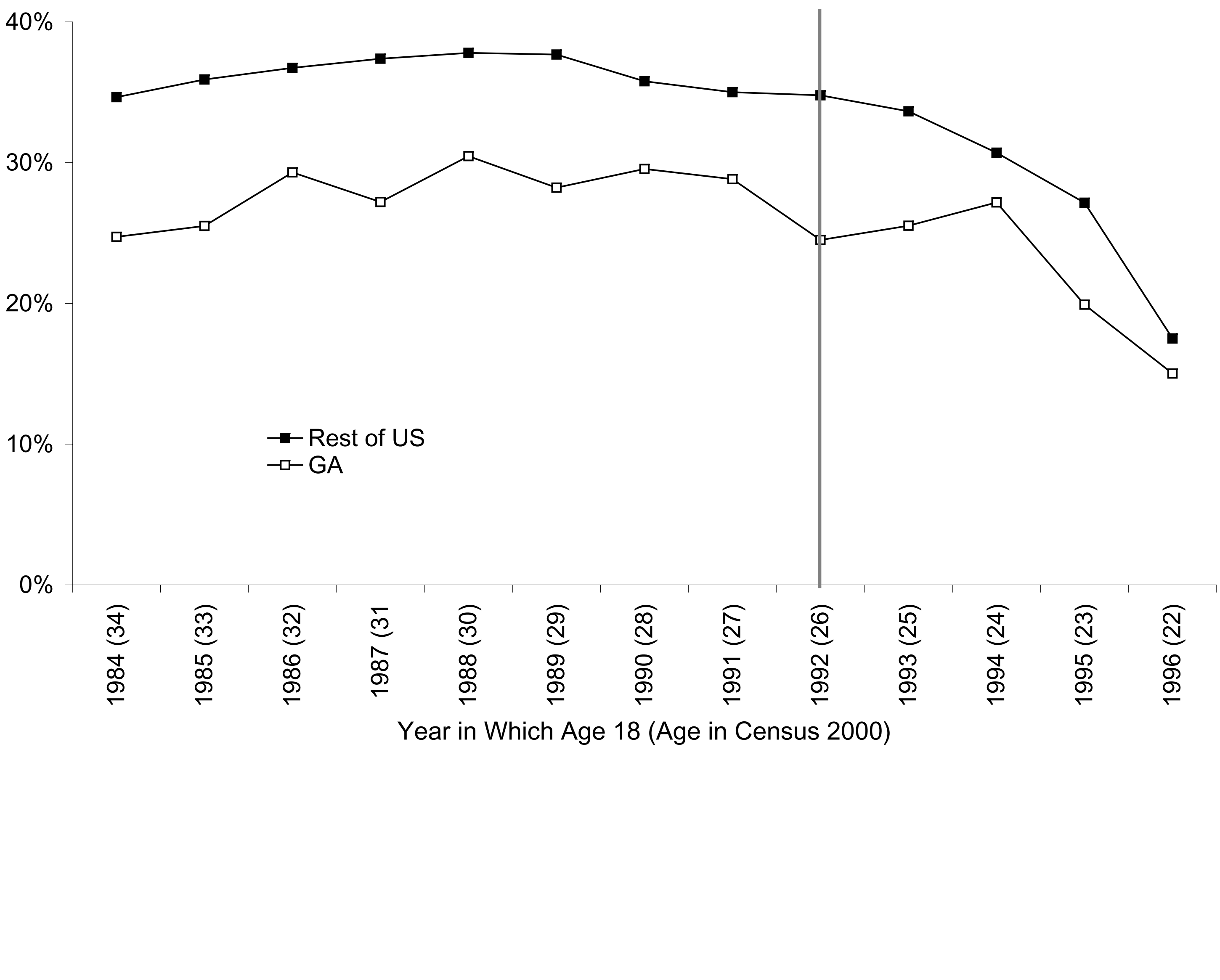


Figure 1B: Proportion Holding a College Degree,
by Age Cohort, Census 2000
Line indicates last pre-program year

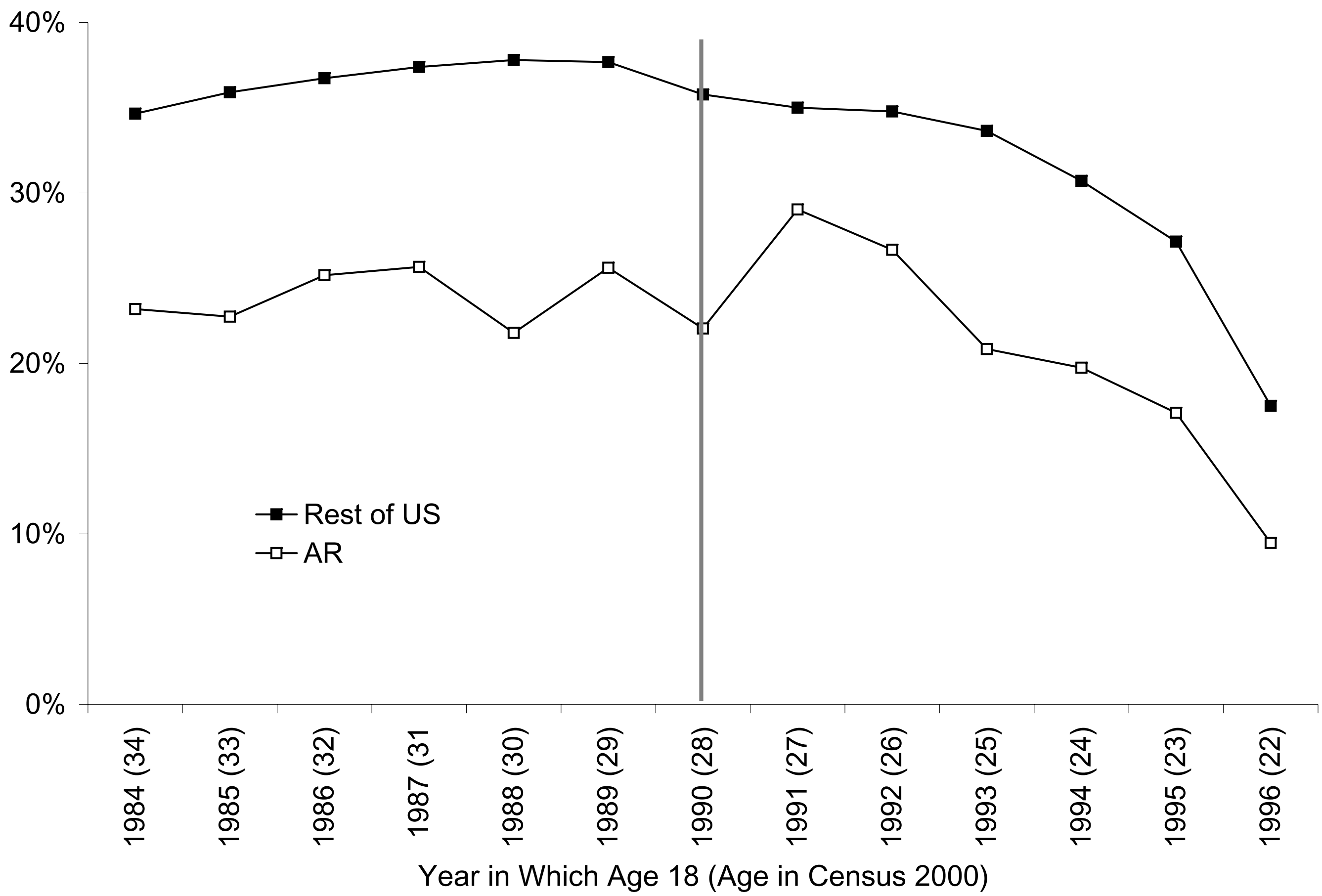


Figure 2A
Unemployment Rate of Young People
Georgia and the Rest of the South
Line indicates last pre-program year

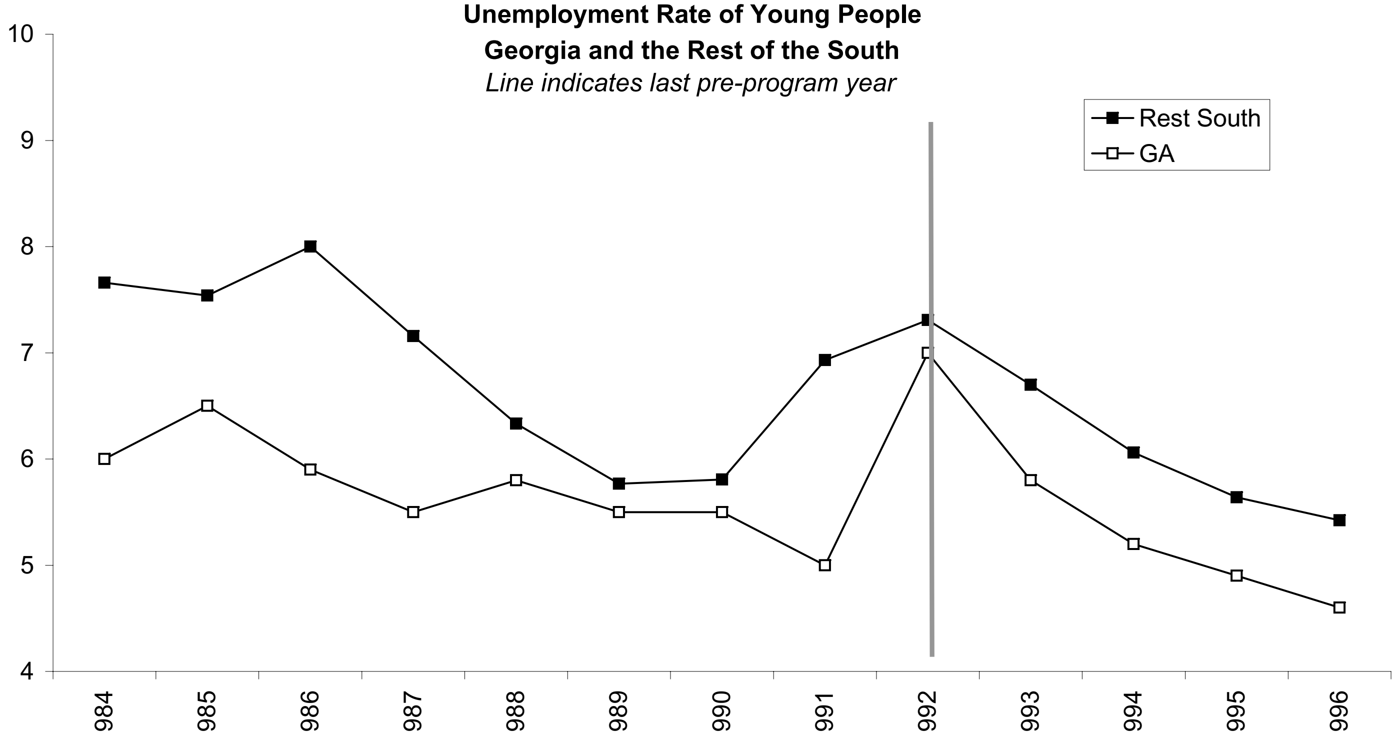


Figure 2B
Unemployment Rate of Young People
Arkansas and the Rest of the South
Line indicates last pre-program year

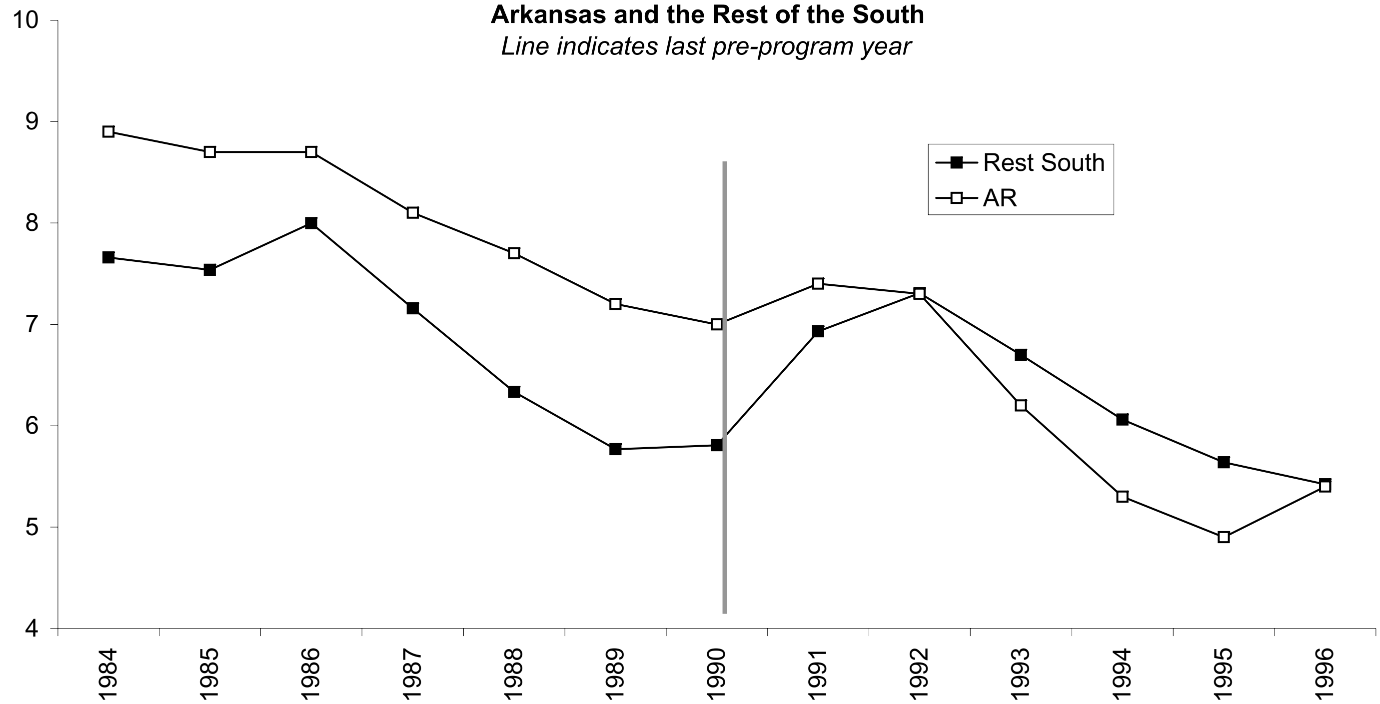


Figure 3A
Effect of Merit Aid of Full Distribution of Education
Plotted is effect on Pr(Educ>=X)

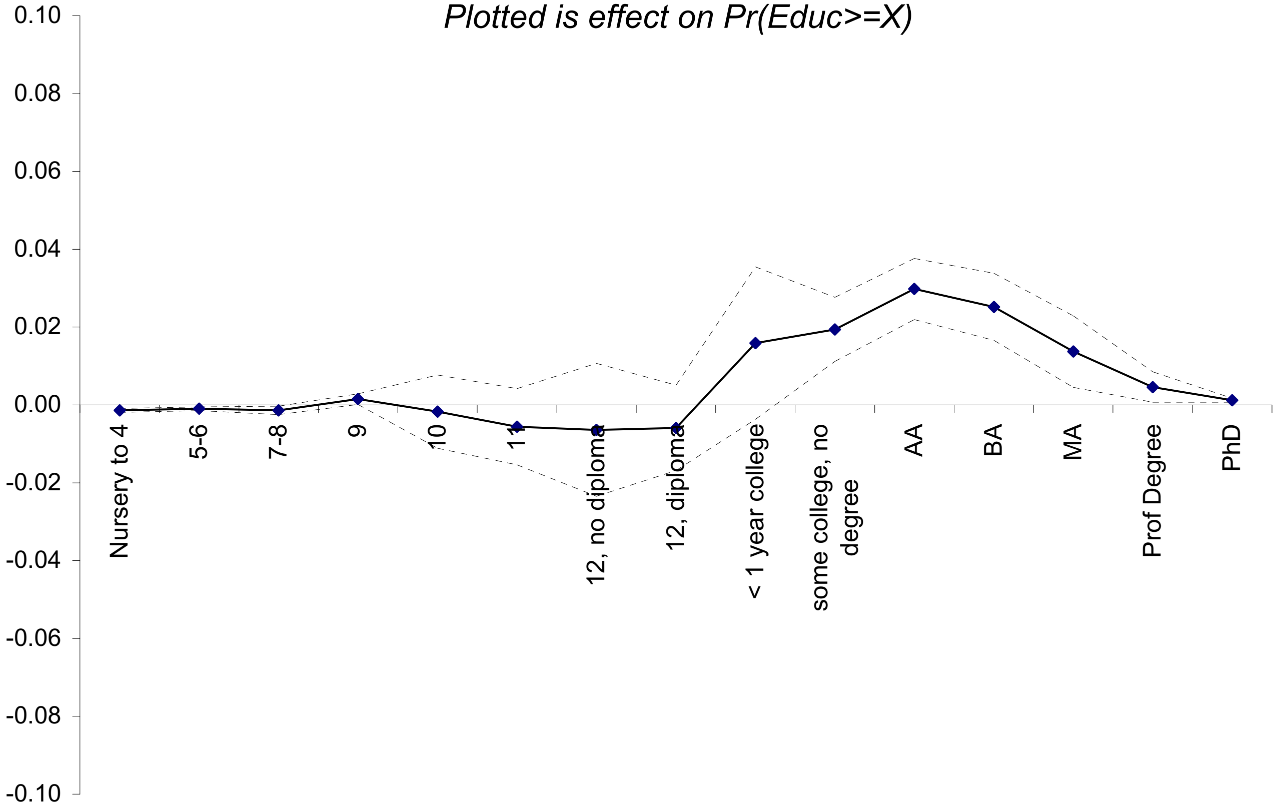


Figure 3B
Effect of Merit Aid of Full Distribution of Education
Plotted is effect on Pr(Educ>=X)
Non-Hispanic White Women

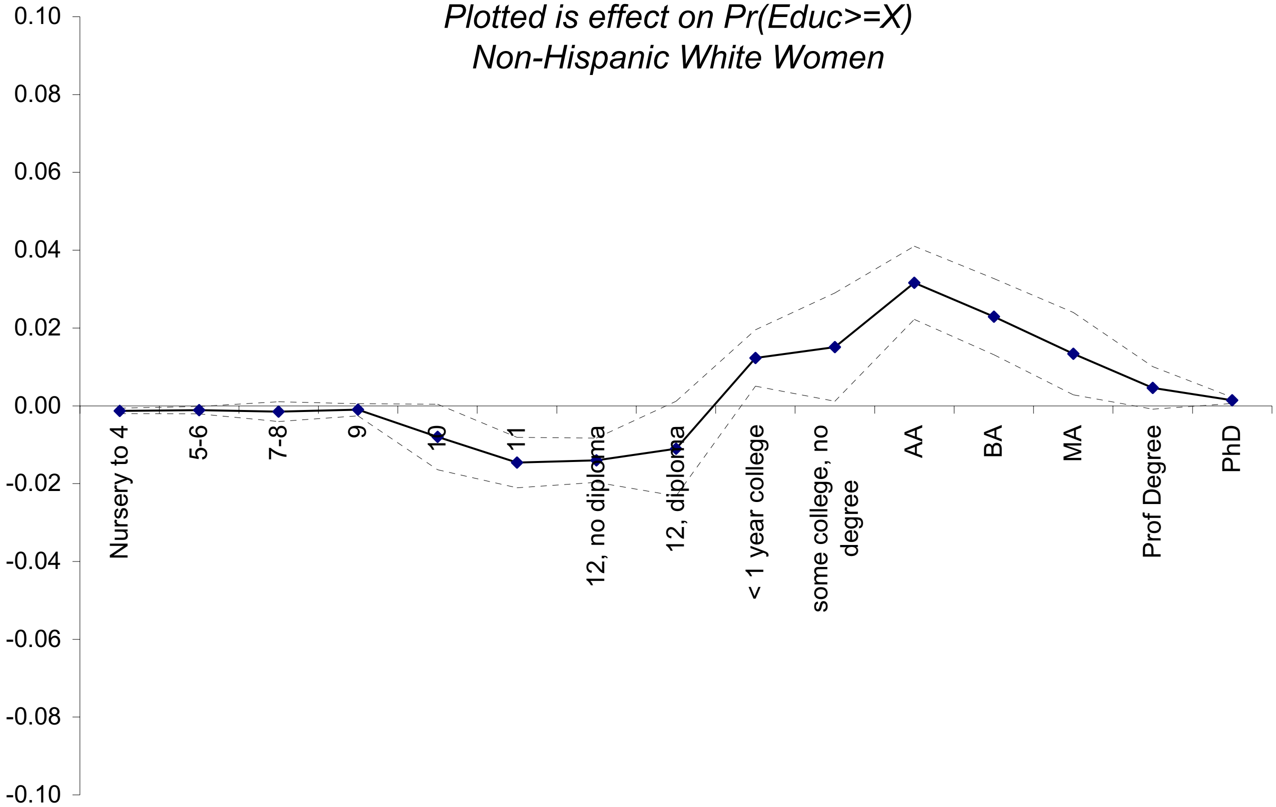
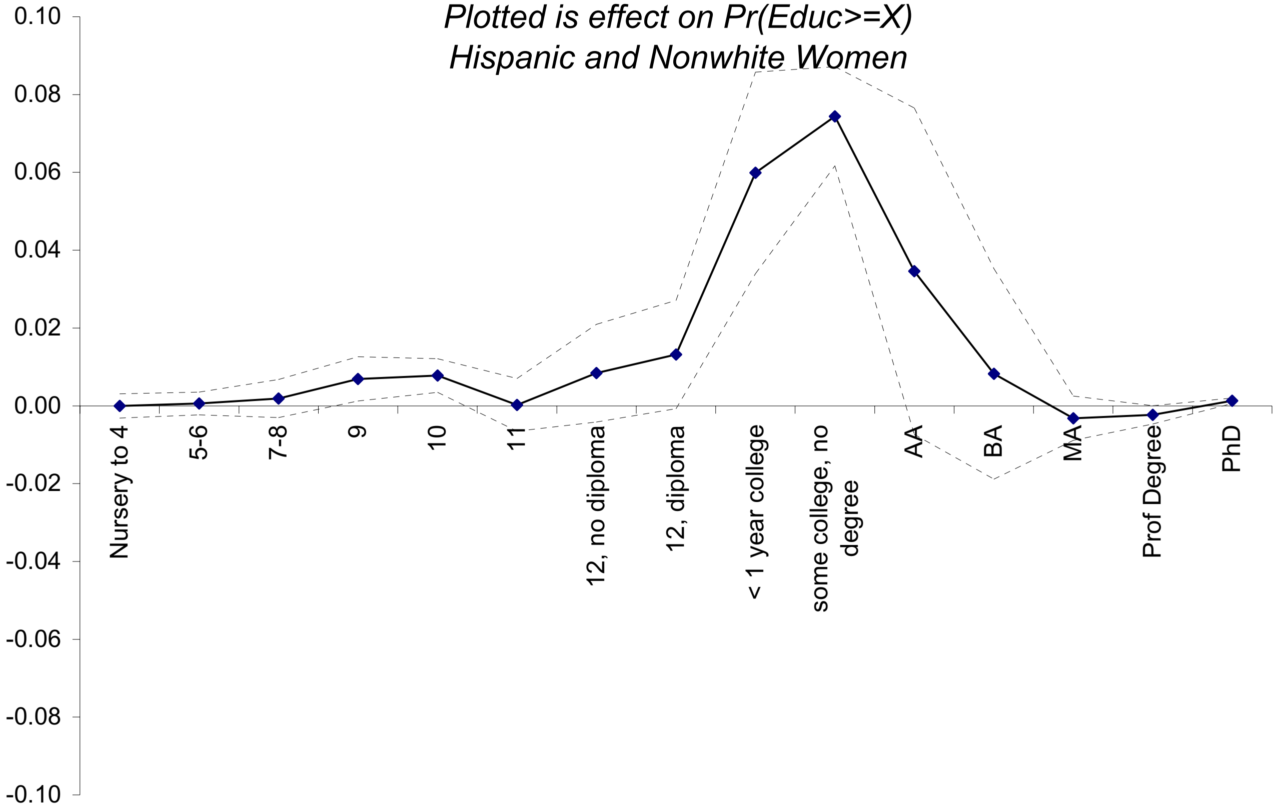


Figure 3C
Effect of Merit Aid of Full Distribution of Education
Plotted is effect on Pr(Educ>=X)
Hispanic and Nonwhite Women



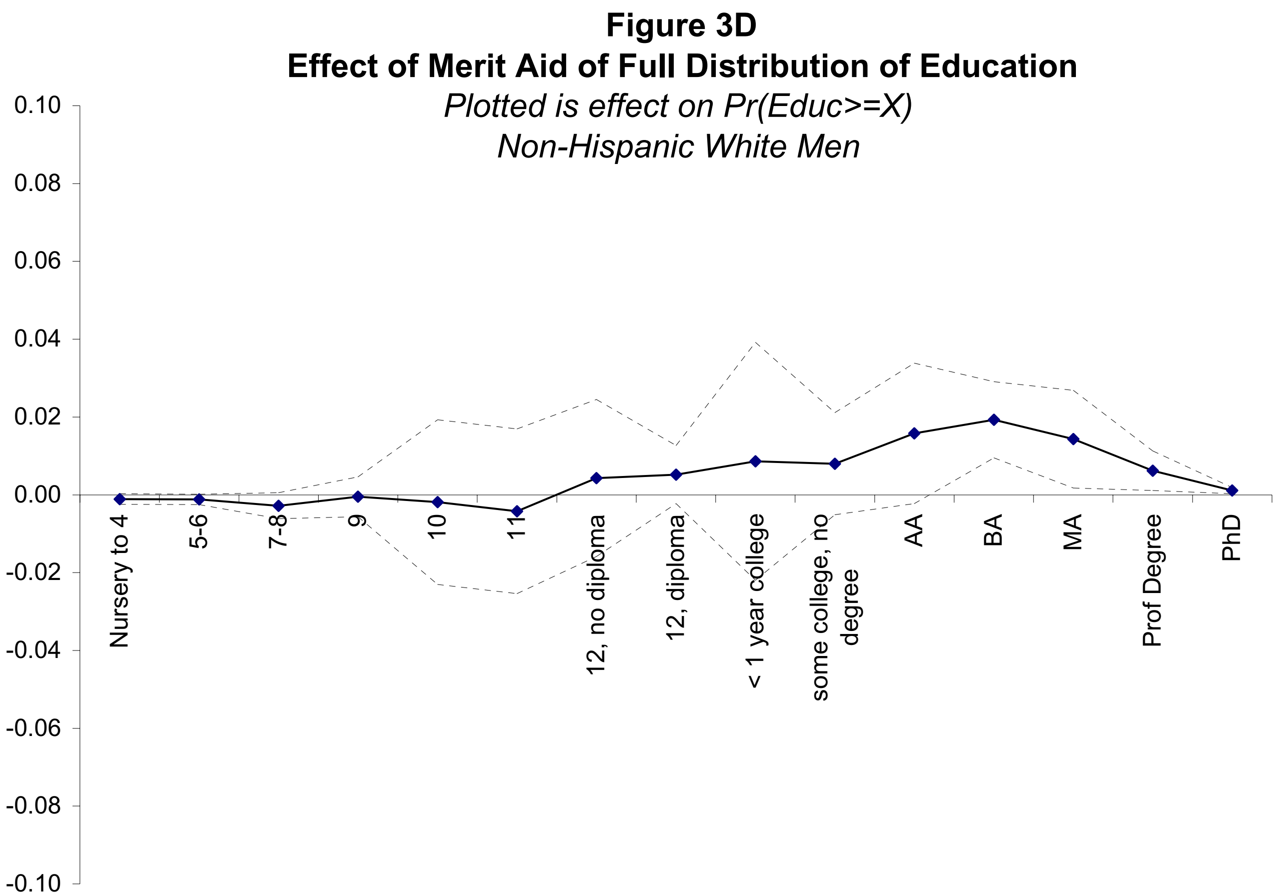


Figure 3D
Effect of Merit Aid of Full Distribution of Education
Plotted is effect on Pr(Educ>=X)
Non-Hispanic White Men
0.10
0.08
0.06
0.04
0.02
0.00
-0.02
-0.04
-0.06
-0.08
-0.10
Nursery to 4
5-6
7-8
9
10
11
12, no diploma
12, diploma
< 1 year college
some college, no degree
AA
BA
MA
Prof Degree
PhD

Figure 3E
Effect of Merit Aid of Full Distribution of Education
Plotted is effect on Pr(Educ>=X)
Hispanic and Nonwhite Men