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Post-Baccalaureate Migration and Merit-Based Scholarships*

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Abstract

For policymakers aiming to alter the migratory patterns of skilled labor, one potential tool involves subsidizing higher education. We present new evidence on the effects of merit aid scholarship programs — programs that offer partial or full tuition subsidies to high-achieving in-state students. Using Census data on 24 to 32 year olds in the U.S. from 1990 to 2010, we show that eligibility for merit aid programs slightly increases the propensity of state natives to live in-state, while also extending in-state enrollment into the late twenties. However, the share of a cohort both living in-state and having a BA is unchanged, with a possible decline in overall BA attainment. These patterns notwithstanding, the magnitude of merit aid effects is of an order of magnitude smaller than size of the treated population, suggesting that nearly all of the spending on these programs transfers resources to individuals whose ultimate migration decisions remain unchanged.

Keywords

Interstate Migration; Higher Education; Brain Drain

1 Introduction

Social scientists have long sought to understand what drives interstate migration, particularly of high-skilled (college educated) workers (Greenwood and Gormely, 1971; Zodekar and Seetharam, 1972; Sandefur and Scott, 1981; Sandefur, 1985; Beine, Docquier and Rapoport, 2001; Frey, 2002, 2004). Policymakers, too, are interested in the interstate migration of high-skilled workers, with an eye toward increasing the skill of the workforce and improving their

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economies. One of the more popular tools used to create and retain a highly-skilled workforce is the merit based scholarship for in-state college attendance. These scholarships reward students who perform above a minimum academic level with grants, provided they attend an in-state postsecondary institution — often either private or public. The hope is that, by compelling students to remain in-state for college, these scholarships will induce college-goers to remain in-state after they graduate.

One of the earliest and most prominent broad-based merit aid scholarships was the Georgia HOPE scholarship, which began in 1993.¹ Since then, more than a dozen other states have introduced similar programs on a large scale. In the 2010-2011 academic year, 28 states offered some sort of merit aid program, totaling \$3.9 billion in spending (NASSGAP, 2011). The expansion and popularity of these programs has driven an extensive body of research, but the majority of it has focused on individual states or on the short-term outcome of college/postsecondary enrollment (Dynarski, 2000; Cornwell, Mustard and Sridhar, 2006; Orsuwan and Heck, 2008; Zhang and Ness, 2010). Merit-based scholarship programs seek not only to promote initial college enrollment but also to enhance the quality of the adult workforce. Now that these programs have existed for some time in many states, a broader range of tests is not only possible, but also of the upmost timeliness and relevance.

In this study, we utilize the broader geographic scope these programs now command and their extended time horizon to build a comprehensive picture of how merit aid scholarships affect residential migration and completed schooling levels among residents old enough to have potentially completed their college educations. To do this, we incorporate data on the introduction of broad-based merit aid programs for fifteen states and Decennial Census and American Community Survey data on 24- to 32-year-olds in the U.S. from 1990 to 2010 in a difference-in-difference framework. Our identification of the programs' effects on residential mobility and educational attainment stems from differences in these outcomes for cohorts of residents in states with and without broad-based merit scholarships before and after the programs were introduced. In other models, we use only states with broad-based merit aid programs and rely on the timing of program introduction for identification of program effects.

Our contribution to this literature is fourfold. First, the long horizon over which some of these programs have operated allows us to focus our analysis on a larger set of lifecycle outcomes than previously available, most prominently residential migration and longer-term educational attainment. Second, we expand the scope of our analysis to include fifteen states with broad-based merit aid programs. As mentioned, much of the existing research focuses on the effect of broad-based merit aid on overall college enrollment and enrollment specifically at in-state postsecondary institutions. Previous studies typically find a positive effect on overall college enrollment of 18- and 19-year-olds (Dynarski, 2000; Cornwell, Mustard and Sridhar, 2006; Conley and Taber, 2011) and a concurrent increase in in-state college attendance, particularly at four-year public postsecondary institutions (Dynarski,

¹We adopt the focus of Dynarski (2000, 2004) on merit aid programs with academic performance requirements that make them accessible to a large portion (30 percent) of residents, i.e. broad-based merit aid programs. For more information on this classification, see Dynarski (2000, 2004).

2000; Cornwell, Mustard and Sridhar, 2006; Orsuwan and Heck, 2008; Zhang and Ness, 2010; Goodman, 2008). Results from a smaller set of studies on educational attainment are more mixed. Some studies find increases in educational attainment (Dynarski, 2008; Scott-Clayton, 2011) while others find little effect or even negative effects (Sjoquist and Winters, 2012a; Cohodes and Goodman, 2014; Goodman, 2008). Most of these studies focus on one or two states, making it difficult to know whether the results generalize to other settings.²

Third, the larger sample sizes that results from analyzing a wider set of merit aid scholarship programs allows us to present new evidence on differences in the effect of merit aid across demographic groups—i.e. gender and race. Finally, relative to the few recent studies that use similar data to answer questions related to ours (i.e. Sjoquist and Winters (2014a) and Sjoquist and Winters (2014b)) our study captures a measure more relevant to policy-makers concerned about the local labor market. When quantifying the effect of merit aid on the migration of high-skilled labor, we focus on the change in the probability of both living in-state and having attained a bachelor's degree (BA). This is contrasted with previous literature, which estimates the change in the probability of living in-state, conditional on having a college degree. In particular, Sjoquist and Winters (2014a) find that conditional on having a BA, individuals are more likely to reside in their home state as a result of merit aid. If there is any effect of merit aid on degree receipt, conditioning the sample in this way limits the ability of previous studies to speak to the effects of the program on the broader population. We show that, after taking into account a decrease in overall degree attainment, the net effect is no measurable increase in the retention of BA recipients. Although the theoretical difference between these effects may not be readily apparent, we explain below in Section 5.7 that there is a substantive difference between the two effects, and the effect on which we focus is most economically relevant from the perspective of the local labor market.

We have three main findings. First, we find that residents born in a merit aid state in a cohort eligible for the scholarships are 1 percentage point (1.5 percent) more likely to live in the state at ages 24 to 32 than those born in cohorts ineligible for such scholarships. Second, eligible cohorts are no more likely to have received a bachelor's degree. In fact, our point estimates for BA attainment are negative, though not consistently statistically significant at conventional levels. This reduction in completion appears to be accounted for by an increase in the likelihood of only finishing some college. Third, we find that merit-aid-eligible 24- to 32-year-old residents are 0.4 percentage points (3 percent) more likely to be currently enrolled in college without having yet obtained a BA. This increased college enrollment in later years seems entirely driven by increases in enrollment at in-state institutions. Importantly, while previous studies have documented increased enrollment among the college-aged (e.g. 18 to 19 year olds), ours is the first study we know of to recover a slight increase in college enrollment among older students, those aged 24 to 32.

Unfortunately, the Census data do not allow us to definitively say whether newly retained residents remained in-state for college or simply relocated back to the home state after

²There are three recent exceptions – recent papers by Sjoquist and Winters (2014b) and Sjoquist and Winters (2014a) look at educational attainment and migration, respectively, using Census data in a fashion similar to our current research design. We discuss below and in detail in Section 5.7 how our results differ in interpretation. Jia (2013), also a current working paper, uses 2010 ACS data and finds a significant increase in attainment of any degree, including both bachelor's and associate's degrees.

attending college elsewhere. Previous studies have found that merit aid availability at the time of college attendance changes students' location of college enrollment, making them more likely to remain in their home state (Dynarski, 2000; Cornwell, Mustard and Sridhar, 2006; Orsuwan and Heck, 2008; Goodman, 2008; Zhang and Ness, 2010). Therefore, it is most likely that the increased retention of state residents is driven by changing the decision about where to enroll in college.

While our confidence intervals do suggest some scope for broad-based merit aid programs to alter outcomes, the extent of their impact is quite limited. This suggests that nearly all of the spending on merit aid represents a transfer to inframarginal residents who ultimately do not alter their educational or migration decisions. Thus, any evaluation of these programs must take into account the distributional implications of directing funds to students who would have attended college even in the absence of the aid.

In the next section, we briefly summarize the history and characteristics of merit-based scholarship programs. In Section 3, we present our empirical methodology. In Section 4, we describe our data. This is followed in Section 5 with our estimates of the program effects on each of the aforementioned outcomes. Finally, we conclude in Section 6 with a synthesis of the programs' overall effects and a discussion of their economic significance.

2 Merit Aid Programs

Although college financial aid contingent on a student's achievement has been offered in some form for many decades (e.g. National Merit Scholars are often awarded scholarships based on their performance on standardized tests), Arkansas was the first state to adopt a broad-based merit-based scholarship in 1991. Since then, dozens of other states have followed, spending a total of \$3.9 billion during the 2010-2011 academic year (NASSGAP, 2011). A key feature of these programs is that they are available to students from a given state provided that the student pursue higher education in that same state.

Merit aid programs vary in their standards for award receipt. For example, some states only require the maintenance of a minimum grade point average in high school and college (e.g. Georgia) while other states also feature a minimum standardized test score in order to qualify (e.g. Tennessee). The stringency of the requirements alters the impact of a merit aid program. If the standards are so high that the marginal college student in a state is unable to meet them, they are unlikely to change educational choices. Furthermore, the effect of a program may prove harder to detect if eligibility is limited to a very small fraction of the population. For this reason, we adopt a convention of the existing literature and focus on states where the merit-based scholarship program was lenient enough to include at least 30 percent of high school graduates (Dynarski, 2004).³ This leads us to focus on the 15 states

³Appendix table A.1 delineates the expansion of these programs over time and details their grant limits. The programs listed include all of the states in Dynarski (2004) and two additional states that introduced programs since 2002, South Dakota and Wyoming. There are a number of programs we do not include because of known lower eligibility levels: e.g. Massachusetts (capped at 25%), New Jersey (15%), Texas (10%), and Washington (up to 15%). Note that much of the previous work has focused on the merit aid programs in Georgia and Arkansas, perhaps because they were the first states to have such programs (e.g. Dynarski, 2000, 2004; Sjoquist and Winters, 2012b). In Appendix D, we examine the pattern of results using just these early adopters and find that the patterns described here hold.

that have broad-based merit aid programs. In our most recent year of data, these states spent more than \$1.4 billion per year, or \$2,191 per recipient per year on merit aid.⁴

The programs usually, though not always, grant aid to students attending either public or private institutions of higher education in the state. The amount of the scholarship varies from \$100 to the full cost of both tuition and fees at public institutions. In eight of the 15 broad-based merit aid states the full scholarship during the time of our data would have covered average tuition at four-year public colleges in the state in 1999. In this way, many of these scholarships can be considered potentially full-tuition scholarships even if they are not explicitly labeled as such.

To be clear, there are states with merit aid programs that we do not classify as merit aid states in our analysis. These include some states with programs introduced too recently for those in our sample to have been affected (e.g. Alaska) and some with programs that are smaller in scope than those we include (e.g. Massachusetts). Including states with smaller merit aid programs in the comparison group will attenuate our program effects if those smaller programs also affect migration or educational attainment behavior. To ensure our results are not driven by including more narrowly-based merit aid states in the comparison group, we conduct separate analyses using only the broad-based merit aid states in the sample (and therefore eliminating both non-merit aid states and more narrowly-based merit aid states from the comparison group). We have also conducted analyses, which we do not report, that include non-merit aid states in the comparison group but omit more narrowly-based merit aid states from the sample.⁵ The results are virtually identical under this alternative specification, which suggests that only including broad-based merit aid programs does not drive our results.

3 Empirical Strategy

To investigate whether merit aid scholarships affect the residential migration, college enrollment, or college completion of eligible residents, we estimate the following:

$$y_{sct} = \beta \text{Merit}_{sct} + \eta_s + \delta_c + \gamma_t + \Gamma X_{sct} + \varepsilon_{sct} \quad (1)$$

where y_{sct} is the fraction of people in the cohort born in year c , in state s , interviewed in year t and who have attained one of our outcome characteristics. We first investigate whether merit aid eligibility makes it more likely for residents to remain in their state of birth and whether high-skilled people — i.e. those with who have only attended some college or those with a BA — are more likely to live in their state of birth. We are also interested in whether merit aid makes it more likely that a person obtains at least a BA or has only attended some college, regardless of their place of residence. In addition, we examine whether individuals are enrolled in college at the time of the survey and have not yet received a BA. That is, we

⁴These figures are generated by the authors' calculation of merit aid data collected from the NASSGAP annual report (NASSGAP, 2011) and additional contact with individual state administrative offices. Data are only available for 11 of our 15 broad-based merit aid programs

⁵These results are available from the authors upon request.

measure college enrollment during for a set age range in any state and as well as in the state of birth.

$Merit_{sct}$ is a dummy variable that equals one if an individual born in year c in state s would have been eligible for a merit aid scholarship in year $c + 18$, i.e. the year she would most likely have entered college.⁶ The fixed effects — η_s , δ_c and γ_t — are state of birth, cohort and survey-year fixed effects respectively. The vector X_{sct} consists of additional cohort-level demographic characteristics, including age controls, race and gender. We also control for the state appropriations to postsecondary institutions in year $c + 18$ to account for other possible changes to a state's higher education funding.⁷ Because economic conditions at the point of high school graduation may affect the probability of going to college overall and affect a family's ability to finance different types of college attendance, we control for the unemployment rate in a cohort's state of birth at the time of high school graduation (year $c + 18$). Since contemporary economic conditions also affect educational investment and residential decisions, we also control for the unemployment rate in a cohort's state of birth at the time of the survey.⁸

We estimate these state-by-cohort-by-year specifications using cell averages over respondents, where the cell averages are computed using the person weights provided by the Census Bureau. The cell averages in the regressions are then weighted by the number of survey respondents represented in the cell.⁹ Standard errors are clustered at the state level, in order to account for potential serial correlation.¹⁰

The coefficient of interest in Equation 1 is β , which gives the probability that a person aged 24 to 32 was induced to change her behavior because of her merit aid eligibility. The underlying assumption in this interpretation of β is that there were not other changes in policy or environment concurrent with the introduction of merit aid programs that affected the college attendance, college completion and residential decisions of 24- to 32-year-olds. In other words, we compare cohort outcomes in states and years when merit aid is available to cohort outcomes in other states or years where merit aid is not available. By controlling for cohort, state-of-birth and calendar year fixed effects in Equation 1, we are essentially comparing treated cohorts in a given state to earlier, untreated cohorts in the same state.

One potential concern is that eligible cohorts occur systematically later than ineligible cohorts. In that case, we may be worried that our estimates are picking up secular trends in

⁶We also estimate specifications with other measures of eligibility — the percent of tuition at a public four-year postsecondary institution covered by the merit program or the number of awards granted per high school graduate. See Section 5.6.

⁷In most specifications, we additionally control for age patterns in our outcomes by including age fixed effects. Obviously, the full set of cohort, survey year and age fixed effects are collinear and cannot all be estimated. On the other hand, solely including cohort and survey year fixed effects is not sufficient for absorbing constant differences across age groups. We therefore include a combination of cohort, survey year and age fixed effects that, while not immediately interpretable, do absorb variation in outcomes across ages. In Table 2, we present results with and without age fixed effects to show that the inclusion of the age fixed effects barely changes our estimates.

⁸Contemporaneous unemployment rates may be endogenous to the policy in question, and therefore, controlling for this may absorb some of the meaningful variation created by the policy. However, our results are not sensitive to omitting contemporaneous unemployment as a control variable. Results are available from the authors upon request.

⁹Results are nearly identical when using individual-level data and Census person weights to do the analysis. Individual-level results are available from the authors upon request.

¹⁰In Appendix C additional inference is performed using nonparametric bootstrap and wild bootstrap procedures (Cameron, Gelbach and Miller, 2008) as well as randomization inference (Bertrand, Duflo and Mullainathan, 2002).

our key outcomes. We account for secular trends in our outcomes with cohort-specific fixed effects. Nevertheless, it may be the case that these trends differ significantly between states that ever adopt merit aid programs and those that do not. To account for this concern, we present specifications which only feature broad-based merit aid states. An underlying assumption of these specifications is that merit programs are not adopted in response to state-specific trends in migration and education. While we are not able to test this assumption directly, we use pre-treatment data to explore whether the passage of merit aid programs is systematically related to pre-existing trends in outcomes across cohorts. To do so, we include a linear trend in the number of cohorts relative to the cohort first eligible for a merit aid program to control for patterns that may not be captured by the characteristics we observe (see Wolfers, 2006).¹¹ Finally, we include time-varying demographic controls to mitigate concerns about other possible changes occurring simultaneously with program introduction.¹²

Beyond the endogenous timing of program adoption, there is a concern that families may sort residentially based on the availability of merit aid scholarships. For example, if families of college-bound students move to Georgia in part because of the HOPE scholarship program, the estimate for the effect of merit aid on college attendance would be biased upward. For this reason, we use a person's state of birth rather than her state of high school attendance to determine treatment eligibility. Though this is not how true merit aid eligibility is defined, it allows us to avoid bias from immigrational selection into merit aid states might cause. For example, consider two families in Georgia giving with children born in 1974 and 1975, respectively. We do not expect the second family to differ from the first due to expecting their child will be eligible for the HOPE scholarship in 1993. One implication of this approach is that our estimates measure an intent-to-treat effect, given that not all residents born in a state remain in the state until age 18. Our results will therefore understate the treatment effect of merit aid, a point we return to in the conclusion.

4 Data

We use public-use data on 24- to 32-year-old respondents to 1990 and 2000 Decennial Censuses and the 2001 to 2010 American Community Survey (Ruggles et al., 2010). The Decennial Census data we use are 5 percent samples, which are a 1-in-20, nationally representative sampling of the U.S. population. The American Community Surveys (ACS) between 2001 and 2004 are nationally representative, 1-in-232 to 1-in-261 samples of the U.S. population. From 2005 to 2010, the ACS is a 1-in-100, nationally representative samples. For our analysis, we select a minimum age of 24, as a majority (85 percent) of BAs are received by this age in our sample, and a maximum age of 32, which ensures that at least three treatment states are contributing to estimates for all age groups.¹³

¹¹Since the patterns in our event study figures we show later appear linear, we expect a linear trend to provide the best fit. We tested this assumption by experimenting with quadratic trends as well. F-tests for the joint significance of higher ordered polynomial terms in the cohort relative to the last treated failed to reject the null of linearity. Note, given the set of fixed effects we estimate, we can only estimate this additional trend among samples that include non-merit aid states.

¹²To further assess our assumptions regarding pre-treatment trends, in Appendix C we present results using a more flexible, event study specification and also conduct placebo tests using only pre-treatment data.

In each survey, the Census Bureau collects information on place of residence, place of birth, current enrollment in school and educational attainment. We use this information to create our dependent variables of interest, which include i) residential migration, ii) educational attainment, and iii) current college attendance.¹⁴ Residential migration is defined as living in a state other than the state of birth.¹⁵ We restrict our definition of college enrollment to current undergraduate study.¹⁶

The Census Bureau also collects gender and race information, which we include in our set of demographic controls. To these data, we add current and lagged state unemployment levels from the Bureau of Labor Statistics. We also include annual state appropriations to postsecondary institutions as reported in the Integrated Postsecondary Educational Data System.¹⁷

In Table 1, we present descriptive statistics that summarize the characteristics of our state-cohort-year cells.¹⁸ The average demographic composition and economic conditions are as expected, given the time period studied. The average rate of migration out of one's state-of-birth for this sample is approximately 34 percent. The probability of receiving a BA is between 23 and 27 percent. Of these, about half, or 12 to 15 percent of individuals, have a BA and are living in their state of birth. Meanwhile, current college enrollment among our sample of people ages 24 to 32 is 8 percent, 5 percentage points of which is in the state of birth.¹⁹

5 Effects of Merit Aid Eligibility

5.1 Residential Migration

We first explore the effects of merit aid eligibility on the residential migration of people old enough to have likely completed their undergraduate schooling. Specifically, we estimate the effect of merit aid eligibility on the probability that people ages 24 to 32 live in their state of birth using three alternative specifications of Equation 1. Table 2 presents these estimates. Column (1) reports estimates using all non-merit states to simulate counterfactual residential migration patterns in the absence of the merit aid program. The estimate is positive, suggesting that merit aid eligibility for in-state college attendance increases the probability that 24- to 32-year-olds live in their state of birth by 0.9 percentage points.

¹³In 2010, 32 year olds in Arkansas, Georgia and Mississippi had been eligible for merit aid at age 18. In the 2010 sample, the oldest cohorts to have been eligible for merit aid are the 37 year olds in Arkansas, followed by 35 year olds in Georgia who were the first eligible cohort in 1993. Results using 24 to 37 year olds lead to the same conclusions and are presented in Appendix G.

¹⁴In our analysis samples, we include individuals with allocated values of these key variables. If place of birth is allocated, it could attenuate our estimates. If any of our outcomes are allocated, it will reduce our power. Our results are generally robust to leaving out those respondents with allocated characteristics, though the resulting smaller samples lead to a slight decrease in statistical power. Results excluding individuals with allocated observations from the sample are in Appendix F.

¹⁵To create educational attainment measures, we use the public-use-file variable EDUCD. A person is categorized as having a BA degree if EDUCD is at least 101 and as having a graduate or professional degree if EDUCD is at least 114. We would have also liked to include measures of Associate's Degree attainment, but the 2003 ACS questionnaire included a rewording of the measurement of Associate's Degree attainment in way that precludes consistent comparisons across time.

¹⁶In results not reported here, we have separately estimated the effects of merit aid on graduate enrollment and they were zero. Results are available from the authors upon request.

¹⁷The results are virtually unchanged with and without controls for state appropriations to postsecondary institutions, indicating that when they occur, shifts in expenditure do not offset the effects of merit aid eligibility.

¹⁸We present data at the state-cohort-year cell, as this is the level of variation used in our subsequent regression analysis.

¹⁹Results using a wider range of ages are available in Appendix G.

However, the estimate is not statistically significant. In the second column, we again estimate Equation 1 on the sample of all 24- to 32-year-olds in the U.S., but include a linear trend in “event time” to capture the differential trend in college attendance in states with merit aid programs and those without.²⁰ The inclusion of the trend changes the estimate to 0.4 percentage points, although it is not statistically different from the estimate in the first column.

It may be the case that states that introduce merit aid programs are somehow different than those that do not. In Column (3), we present estimates using only merit aid states to ensure our estimates are not biased by these types of differences.²¹ The estimate is similar in magnitude to that in the first column, though it is now statistically significant. Our estimate in Column (3) suggests that merit aid eligibility increases the probability of people between the ages of 24 and 32 living in the same state in which they were born by 1 percentage point.²² Given that about 66 percent of 24- to 32-year-olds live in their home states, this represents a decrease in mobility of about 1.5 percent.

In Panel B of Table 2, we repeat each of the analyses described above while adding age fixed effects. This controls for any patterns of migration that differ across people of different ages. The results are virtually unchanged from those just described. Since this remains the case in our subsequent specifications, we henceforth only report estimates that include these fixed effects.

5.2 Residential Migration of High Skill Workers

One major goal of merit aid programs as described by policymakers is to increase the retention of high-skilled workers in the state. To determine whether the increased retention of residents seen in Table 2 is indeed increased retention of high-skilled residents, we examine whether merit-aid-eligible cohorts are more likely to both live in the state and have at least a BA or at least some college attendance. These results are presented in Table 3, which has a similar structure to Table 2. In Panel A, the dependent variable represents the outcome of both living in one's state of birth and having a BA; in Panel B, it corresponds to living in one's state of birth and having attended college, but not obtaining a BA degree.

Looking across the columns of Panel A of Table 3, we see that merit aid generally does not have an effect on the probability of living in the state and holding at least a BA. With the exception of the first column specification, the estimated effect is nearly zero and is not statistically significant. For example, in Column (3), the estimate is -0.1 percentage points and the standard error suggests the estimate is between -0.3 and 0.1 percentage points.

²⁰We include a linear trend in number of years between each cohort in our sample and the last cohort ineligible for merit aid in each merit state. Note that this trend in year of birth is set to zero for the last ineligible cohort in merit states and for all cohorts in states without merit aid programs. Henceforth, for simplicity we will refer to this as simply “the trend.”

²¹In these specifications we do not estimate a model with a trend because with only merit states in the sample the trend is not separately identified from time fixed effects.

²²Since the estimate in Column (3) comes from a specification that includes only merit aid adopting states and controls for possible differences across states in both time-invariant and time-variant characteristics we will, in the interest of brevity, focus on interpreting estimates from it in the following sections. However, throughout the paper, we present all three models discussed here; generally, they produce a similar pattern of results.

Overall, the results provide little evidence of an effect on the share of population that is both high-skilled and remains in-state.

In Panel B, we see that merit aid eligibility actually had a positive effect on the likelihood of state residents remaining in their state of birth and having completed only some college by ages 24 to 32. In Column (3), the estimate suggests that the probability increased by 0.9 percentage points, an estimate that is statistically significant at the five percent level. Given that the average propensity of 24- to 32-year-olds to live in their state of birth and have attended college for at least a short period of time is 35 percent, this represents a 4 percent increase.

5.3 College Completion in Any State

Thus far, we have shown that merit aid eligibility increased the probability of living in the state one was born in, but it did little to change the probability of both living in the state and having a BA. This raises the question of whether merit-based scholarships affect overall BA attainment for a treated cohort, regardless of current residence. To investigate the effects of merit aid eligibility further, we present results in Panel A of Table 4 where the dependent variable of interest is attainment of at least a BA. Across all columns, the estimated effects of merit aid eligibility are negative. The results, however, are not consistently statistically significant and, therefore, offer moderate evidence that merit aid may reduce BA attainment. The result in Column (3), which includes only merit aid states, suggests that merit aid eligibility decreased BA attainment by 0.7 percentage points and is statistically significant at the ten percent level. Given baseline rates of degree receipt, this represents a 3 percent decrease in BA attainment.

To summarize, merit aid decreases the overall probability of receiving a BA, but has no net effect on the probability of living in-state with a BA. This suggests that some BA holders, who would have lived elsewhere between the ages of 24 to 32, must have been induced to live in their home state.²³ Unfortunately, we cannot directly identify the marginal recipient nor where the marginal degree is earned.²⁴ The estimates are consistent with ex-ante mobile college students staying in-state to take advantage of merit aid and, in the process, crowding out ex-ante immobile students from getting a degree. However, there are alternative stories that are equally consistent with the findings. For example, the reduction in degrees may be concentrated among those students now induced to remain in-state, perhaps due to a poorer match with an in-state school relative to the counterfactual, out-of-state school. Such a mismatch could be driven by lower school quality (Cohodes and Goodman, 2014) or general features of attending an in-state school, e.g. distractions stemming from a closer proximity to one's home.

²³This assumes that the differential mortality has not also changed the denominator used in calculating these shares.

²⁴With the Census data it is not possible to know where respondents attended college once they are no longer enrolled. Two data issues prevent us from using Census data on 18- to 23-year-olds. First, because from 2001 to 2005 the ACS did not include residents of group quarters (e.g. dorms), we are limited to ACS data in only the years from 2006 to 2010, which severely limits the number of post-treatment cohorts available in each state. Second, since most programs were introduced in the 1990s, our only pre-treatment data would have come from the 1990 Decennial. Given the rapidly changing patterns of college enrollment over this period (Fitzpatrick and Turner, 2007), we thought it imprudent to use data in which we could not be sure pre-treatment trend differences in treated states drove our results.

To further understand a potential decrease in BA attainment, we present results in Panel B of Table 4 where the dependent variable indicates attending some college without receiving a BA. The estimate in Column (3) suggests that merit aid increases the likelihood of this outcome for 24- to 32-year-olds. The point estimate is 0.008 and it is statistically significant. Thus, the evidence is consistent with the notion that merit programs are associated with a drop-off in college completion within a treated cohort.²⁵

The possible decrease in BA attainment is a bit surprising given the rest of the literature, which suggests that merit aid introduction increases enrollment in college and graduation, particularly at in-state postsecondary institutions (Dynarski, 2004, 2008; Zhang and Ness, 2010; Scott-Clayton, 2011). However, our results do not necessarily represent a complete departure from earlier work. First, our work does not speak to the location of college enrollment for ages younger than 24 and thus has no bearing on previous results regarding the location of enrollment. In fact, finding no change in college attendance can be reconciled with finding increased in-state enrollment if the new in-state enrollment comes from students who would have attended college elsewhere.²⁶ Similarly, previous work finds increased college enrollment regardless of location for merit aid-eligible 18- and 19-year-olds (Dynarski, 2004). These findings are consistent if merit aid simply accelerates the timing of college entry. Given that many states condition merit aid receipt on enrollment within one or two years of high school graduation, a shift in timing does not seem implausible, though our results are not dispositive of this phenomenon.

With respect to BA attainment, there have been a number of studies that focus on the effects of merit aid on degree attainment of eligible residents. The first of these was Dynarski (2008), who used the one-percent Decennial Census samples from 1990 and 2000 in a difference-in-difference strategy analogous to the one we use here. This study focused on the only two states that had programs available at the time, Arkansas and Georgia. Dynarski (2008) found that merit aid eligibility increased the probability of 22- to 34-year-olds obtaining at least a BA by 2.5 percentage points (and at least an Associate's degree by 3 percentage points). In a replication exercise, Sjoquist and Winters (2012a) show that the result is not robust to the use of five percent Census samples in place of the one percent samples.

The availability of the 2001-2010 ACS makes it possible to extend the previous analyses to additional merit aid states and to additional treated cohorts in Arkansas and Georgia. In Appendix Table D.1, we replicate this previous work in Columns (1)-(3). In Column (4), we use the same sample definitions and specification choices as the earlier studies, but with the 5 percent samples for the period from 1990 to 2000. The result is consistent with those in our Table 5. We show in Columns 5 and 6 that the additional differences in our approach

²⁵Our definitions of college enrollment are conditional on having received a high school diploma (or the equivalent). If broad-based merit aid scholarship introduction caused or was driven by declining high school graduation rates, we might find merit aid programs had a negative effect on BA attainment. In results reported in Panel C of Table 5, we check to be sure high school graduation rates did not change, estimating Equation 1 using graduation from high school as the dependent variable of interest. The coefficient is negative but not statistically significant in the models using only the sample of states that introduced merit aid.

²⁶Indeed Cornwell, Mustard and Sridhar (2006) find that in the case of the Georgia HOPE program the increase in in-state, 4-year college enrollment is driven by students who would have attended college out of state and students who would have attended 2-year institutions. This pattern is consistent with our finding of no overall change in ever have enrolled in any college.

(e.g. expanding the sample to 2010, using new merit aid states, etc.) have little further effect on the estimates. Thus, roughly half of the difference between ours and previous estimates can be attributed to a difference between the 1 and 5 percent 2000 Decennial samples, and the rest can be explained by including the 1990 Decennial.²⁷

5.4 College Attendance

We observe no change in having started college, combined with a possible decrease in degree attainment. This leads naturally to the question of whether these 24- to 32-year-olds have indeed finished their schooling or whether they are currently enrolled in college. In Table 5, we re-estimate Equation 1 where the dependent variable is an indicator for college enrollment without yet attaining a BA. Results are presented separately for enrollment anywhere in the U.S. at the time of the survey (Panel A) and enrollment in one's home state at the time of the survey (Panel B). Focusing on our specification in Column (3), we find that cohorts eligible for merit aid have an increased likelihood of college enrollment driven by increased enrollment in their home states. College enrollment in any state went up by 0.3 percentage points, while in-state college attendance went up by 0.5 percentage points. Both estimates are statistically significant at the five percent level in Column (3), but the two estimates are not statistically different from each other.

With the data available, we cannot determine if the same people who would have held degrees are instead currently enrolled. However, these estimates are consistent with about 70 percent of the 0.7 percentage point decrease in BA attainment among 24- to 32-year-olds being due to still being enrolled.²⁸ The increase in college enrollment of people older than traditional college ages is a novel finding in the merit aid literature. Eligibility restrictions make it extremely unlikely that enrollment at these ages is subsidized by merit aid.²⁹ We suspect increased college attendance or mismatch driven by merit-aid leads to a crowding out of resources and prolonged enrollment of marginal college students. However, given data constraints, we cannot rule out other possible mechanisms.

5.5 Heterogeneous Treatment Effects

In Table 6 we present results separately by race or gender. We find that merit aid eligibility has a positive effect on the likelihood of living in-state, with or without a BA, for black residents, and has essentially no effect for white residents. In addition, the degree attainment of whites decreases by 1 percentage point because of merit aid (a result that is statistically significant at the 10 percent level), while there is a negligible positive effect for black residents. Previous studies of merit aid have reported less positive effects for minorities and low-income students, when focusing only on the Georgia HOPE program (Dynarski, 2002; Cornwell and Mustard, 2002). However, Dynarski (2004) shows the opposite is true among 3 other prominent merit aid programs. Cohodes and Goodman (2014) show that a merit aid

²⁷Note that even when we omit the 1990 Census in row 5 of Table E.1 we still find negative point estimates, albeit statistically indistinguishable from 0.

²⁸We calculated this estimate by dividing the increase in college enrollment (0.5 percentage points) by the decrease in degree attainment (0.7 percentage points).

²⁹Generally, merit aid programs require students to enroll in college within a year or two of high school graduation, complete enrollment within some number of years (between 4 and 7), limit the overall number of scholarship participation years per student, or some combination of the three.

scholarship in Massachusetts shifts enrollment to covered schools for both white and nonwhite students, but only results in lower graduation rates for white students. Our results, which feature an even broader set of merit aid programs, are consistent with the latter two findings. Dynarski (2004) discusses why merit aid effects may differ across racial groups. On the one hand, the baseline likelihood of qualifying for merit aid tends to be negatively correlated with non-white status. On the other hand, non-white students may be differentially sensitive to financial aid. Our findings suggest that the latter factor dominates.

With respect to gender, Table 6 shows little difference in the effects of merit aid on overall migration for men and women. However, women tend to demonstrate larger and more statistically significant effects with respect to BA attainment, current enrollment, and migration at different levels of educational attainment. The literature comparing these effects for men and women finds mixed results. Dynarski (2008) finds a larger effect of merit aid on educational attainment among women, especially nonwhite and Hispanic women. On the other hand, neither Sjoquist and Winters (2012a) nor Cohodes and Goodman (2014) find significant differences between men and women in their preferred specification. In these cases, the results apply either to one specific merit aid program, or a smaller set of merit aid programs. Our results are over a broader set of merit aid programs and a longer time horizon and point toward a larger behavioral response among women than men. This is also consistent with Card and Lemieux (2001), who find women's enrollment substantially more sensitive to tuition levels. Thus, the effects we see seem to be driven by women.

5.6 Continuous Treatment Effects

In Table 7 we consider alternative specifications that explore differences in treatment effects by program type. We also present results that incorporate variation in tuition generosity across merit aid programs. All of the samples are restricted to merit aid states and the specifications correspond to Column (3) of Tables 2–5. Columns (1) and (2) report coefficients from a regression with a treatment indicator for merit aid eligibility and an interaction between this indicator and one indicating programs that only allow use of the merit aid at public institutions, respectively. With respect to migration, the states that only allow merit aid to be used at public institutions appear to have greater retention, but lower likelihood of both living in-state and having a BA. On net, these states appear to experience a larger decrease in BA attainment without as much of a compensating increase in college attendance. This is consistent both with previous literature on merit aid (Cohodes and Goodman, 2014) and theoretical models of private consumption crowd-out when goods are publicly provided (Peltzman, 1973). Finally, these programs do not appear to have any significantly different effects on current college enrollment.

In Column (3), we report an alternative specification where the indicator for merit aid eligibility is replaced with the fraction of average tuition and fees covered by the merit aid scholarship at in-state four-year public institutions. This fraction ranges from 0 for ineligible cohorts to 0.13 in Michigan to 1.11 in Arkansas. The coefficient is interpreted as the change in the outcome associated with going from no merit aid to full tuition coverage. In general, we arrive at results similar to our specifications using the indicator for merit aid eligibility.

In theory, this measure may be associated with greater precision, as it distinguishes between weaker and stronger programs. This seems to be the case in many of the outcomes.

Finally, in Column (4), we use as our main regressor a proxy for eligibility. We take one-fourth of the total number of awards given in 2009-2010 and divide it by the number of high school graduates in that same year. The coefficients estimated should be interpreted as the effect on the outcome of going from zero to full eligibility. This measure allows us to potentially scale the effects relative to the number of students eligible. In general, we see an increase in the size of our point estimates, usually by an order of magnitude. The results are qualitatively similar when using this measure, but are more likely to be statistically significant than when using fraction of tuition covered. When using a measure of merit aid that accounts for generosity and eligibility, we typically find even stronger evidence in support of our main results.

5.7 Interpretation of Key Parameters

We return to our results from Section 5.2, where we examined the effect of merit aid programs on the migration of high-skilled workers. As mentioned above, this research question is also explored in Sjoquist and Winters (2014a). It is important, however, to draw a distinction between the two approaches. In our current study, we feature the joint probability of having a college degree (or having attended some college) and living in one's state of birth as the key outcome, while Sjoquist and Winters (2014a) focus on the probability of living in one's state of birth, conditional on having a college degree. In other words, we estimate Equation 1 using the share of a cohort both living in-state and having a BA for y_{sct} — Sjoquist and Winters (2014a) use the share of a cohort living in-state for y_{sct} and restrict the sample to those with a BA. Although the difference between a joint and a conditional probability may seem semantic, we will now show that the two need not lead to the same conclusion.

To illustrate our point, we rely on a simple potential outcomes framework, in the spirit of Imbens and Angrist (1994). Let $M_i \in \{0, 1\}$ be a binary indicator for the presence of a merit aid program for individual i . Furthermore, let $S_i(M_i)$ and $C_i(M_i)$ be binary indicators for living in one's state of birth and attaining a BA, both of which are potentially functions of merit aid. Denote $\beta_1 \equiv \mathbb{E}[S_i(1)C_i(1) - S_i(0)C_i(0)]$ as our parameter of interest, i.e. the effect of merit aid on the probability of *both* living in one's birth state and having a college degree. Sjoquist and Winters (2014a) alternatively estimate the effect of merit aid on the probability of living in one's birth state, *conditional* on having a college degree, which we will denote as $\beta_2 \equiv \mathbb{E}[S_i(1)|C_i(1)=1] - \mathbb{E}[S_i(0)|C_i(0)=1]$. Finally, denote $\beta_3 \equiv \mathbb{E}[C_i(1) - C_i(0)]$ as the effect of merit aid on degree attainment, for individuals living either in- or out-of-state. The relationship between the three parameters can be expressed as follows:³⁰

³⁰We derive this result in Appendix B.

$$\underbrace{\mathbb{E}[S_i(1)C_i(1) - S_i(0)C_i(0)]}_{\beta_1} = \underbrace{[C_i(1)] \cdot [\mathbb{E}[S_i(1)|C_i(1)=1] - \mathbb{E}[S_i(0)|C_i(0)=1]]}_{\beta_2} + \underbrace{\mathbb{E}[C_i(1) - C_i(0)] \cdot [S_i(0)|C_i(0)=1]}_{\beta_3}.$$

(2)

As can be seen, the parameters β_1 and β_2 are proportional to each other only when there is no effect of merit aid on overall educational attainment, i.e. $\beta_3 = 0$. In fact, β_1 and β_2 can even have different signs. In our sample we can estimate β_2 via the specification from Column (3) in Table 2, but this time restricting the sample to those with a college degree. We get a point estimate of 0.014. We use the point estimate of -0.006 for β_3 , taken from Column (3) of Table 4. Finally, using Column (2) from Table 1 we can calibrate values for $\mathbb{E}[C_i(1)]$ and $\mathbb{E}[S_i(0)|C_i(0)=1] = 0.23$ and $0.12/0.23 = 0.52$, respectively. Plugging these values into Equation 2, we get $\beta_1 \approx 0$, which is essentially what we estimate directly in Column (3) of Table 3. Thus, our simple identity reconciles the apparent discrepancy between what we conclude in the current study regarding the migration of high-skilled workers and what is concluded in Sjoquist and Winters (2014a).

We choose to focus on β_1 , as it is the parameter that is economically relevant from the standpoint of the local labor market. For simplicity, assume that the size of a cohort is fixed at N . It follows that the effect of merit aid on the supply of high-skilled workers is $\beta_1 \cdot N$. Our estimates imply that the net effect of merit aid on this quantity is essentially zero ($p = 0.657$). This is even though the effect of merit aid on the share of those with a BA who remain in-state is positive and statistically significant in our sample — $\beta_2 = 0.014$ and $p = 0.038$ — and has been estimated as positive in previous studies (Sjoquist and Winters, 2014a).³¹

In other words, a positive β_2 suggests that the choice of location for those with a BA tilts toward the home state after merit aid programs are introduced. However, our negative point estimate of β_3 suggests that the total size of this group is also smaller. Thus, the value of zero we find for β_1 indicates that because the tilt in location toward the home state among those with a degree is coupled with a decrease in degree attainment, the merit aid states do not see an increase in high-skilled workers. Therefore, our estimate improves upon past parameters by adding relevant information for policymakers that care about both the educational attainment and retention of workers in their states.

5.8 Robustness Checks

In Appendices C-H we explore a number of alternative specifications, in order to assess the robustness of our results. First, we generalize our approach to an event study framework and also conduct placebo tests in order to check for pre-treatment trends. Next, we address the possibility that our inference may be biased by a small number of treatment states. We also redo our analysis using a range of alternative sample selection criteria. Finally, we assess the

³¹Using a slightly different sample, Sjoquist and Winters (2014a) report $\beta_2 = 0.02$ as their preferred estimate.

sensitivity of our analysis to our definition of a merit aid state. Our results are generally robust across this wide range of specification choices.

6 Conclusion

We have shown that merit aid programs modestly increase the probability that residents live in their state of birth after they are old enough to have graduated from college. We additionally show that the share of the treated cohort both living in-state and having attended college modestly increases. The share of a cohort both living in-state and having obtained a BA, however, does not appear to increase. Thus, merit aid programs have the potential to retain a slightly larger workforce of state-born individuals who are more likely to have attended college.

This retention of high-skilled workers might come at a price, however, as overall rates of degree attainment among a treated cohort may decrease slightly with the introduction of merit aid programs, at least in the short run. This is despite the fact that there are no changes in high school diploma receipt or the probability of ever having attended college. This result is not statistically significant across all of our methods of inference. However, an overall decrease in BA attainment combined with no change in the share of a cohort living in-state with a BA suggests that some residents who would have obtained their college degrees and lived elsewhere are induced by their merit aid eligibility to live in-state. Interestingly, we also find increased rates of college enrollment among residents older than traditional college ages, which may indicate that some of the reduction in BA attainment is explained by a delay in degree receipt.

We find the migration effects to be primarily concentrated among black residents. On the other hand, the decrease in BA attainment is only significant among white residents. The migration responses are similar across gender groups. Meanwhile, all of the educational attainment effects are driven by women. These patterns are generally consistent with previous literature, although our estimates apply to a broader set of merit aid programs and a longer time horizon.

There are at least three interpretations of the possible reduction or delay in BA attainment. First, the merit aid may attract previously out-migrating college enrollees to in-state schools, which in turn crowds out in-state students who would have never migrated. This may be especially true if in-state education supply is inelastic and if would-be outmigrants are of higher ability or income, allowing them to outbid non-migrants. With more in-state college attendance among traditional college age students, some may find it hard to fulfill requirements for graduation in a timely manner or at all. Second, the marginal migrants and degree recipients may be one and the same. That is, merit aid may cause price-sensitive students to remain in-state, resulting in institution or location mismatches. Such a mismatch may be due to differences in quality (Cohodes and Goodman, 2014; Chakrabarti and Roy, 2012) or general location-specific factors. Note, these explanations require in-state enrollment during college-age years to respond to merit aid, which we cannot directly show, but which has been demonstrated in other contexts (Dynarski, 2000; Cornwell, Mustard and Sridhar, 2006; Orsuwan and Heck, 2008; Zhang and Ness, 2010). A final interpretation is

that merit-aid induces students to switch from 2-year institutions to 4-year institutions in a way that ultimately reduces the probability of graduation – a mismatch story of a different sort.

Each of these mechanisms may also generate the reduction in ultimate out-migration that we observe. In the “crowd-out” case, those students who are newly induced to remain in-state for college may remain in-state due to location-specific human or social capital.

Alternatively, non-migrants now crowded out of a BA may experience a reduction in mobility, which would likewise increase the share of residents remaining in-state. In this case, however, at least some of the out-migrants now enrolling in-state would have to remain in-state in order for the share of the cohort both living in-state and having a BA to remain constant.

Marginal out-migrants remaining in-state to take advantage of merit aid and endure a mismatch might also reduce out-migration. In this case, the mismatched student may experience less mobility due to reduced educational attainment. Similarly, the extent to which prospective 2-year students substitute to 4-year institutions prematurely and do not graduate may likewise generate a reduction in mobility for this group. This also requires the retention of out-migrants with BAs to hold constant the share of the cohort both living in-state and having a BA.³²

More importantly, the magnitude of our results suggest that only a small fraction of the eligible population responds to merit aid eligibility by changing educational or migration decisions. Indeed, when we exclude data from 1990 (see Appendix E), the merit program effects appear to be even smaller, though the estimates in that sample tend to rely more heavily on later-adopting states and may therefore not be directly comparable. In any event, nearly all of the \$1.4 billion in spending among our broad-based merit aid states appears to represent a transfer to inframarginal, college-going residents. Thus, merit aid programs must also be viewed in light of distributional concerns, largely benefitting students who are ex-ante most likely to attend college and less targeted than, say, need-based educational grants.

An alternative way of interpreting the results is to scale our effects by the share of a treated cohort that actually receives a merit aid scholarship, in order to arrive at a “Treatment-on-the-Treated” (TOT) effect using recipients. In our case, this would take into account the fact that only 70% of a cohort born in a state is around for high school, less than 100% of students are eligible and even fewer students actually take-up the scholarship. Even so, it will still be the case that our treatment effects are an order of magnitude smaller than the treated population. Furthermore, this calculation assumes that all behavioral responses are driven by scholarship recipients. In fact, we cannot rule out the possibility that some of the merit aid effects are driven by spillovers to non-recipients. For this reason, we retain the cohort, including recipients and non-recipients, as the treated unit, or at least the share of the cohort still living in-state at the time of high school graduation.

³²Technically, the marginal in-state enrollee and the marginal residential migrant need not be as interconnected as these stories suggest. We cannot rule out a general pattern of separate and independent effects of merit aid on educational attainment and residential migration.

One final caveat worth noting is that our research design combines patterns across 15 merit aid programs. We acknowledge that the average patterns at a national level mask some heterogeneity across different states. For this reason, our overall estimates may differ from some of the evidence found in specific settings (e.g. Scott-Clayton, 2011; Bettinger et al., 2016). Unfortunately, we do not have the power to separately determine the effect of each individual program with our data. Nonetheless, we think it important to explore the overall merit aid experience.

Our results present a mixed picture regarding the evaluation of merit-based scholarships. In most cases, the stated aims of the programs are to subsidize the cost of attending college, increase educational attainment and help maintain a highly educated labor force. In fact, it seems that states with these programs do retain more residents, albeit a small fraction compared to the targeted population. Furthermore, the net effect on the skill composition of the labor force is ambiguous. The number of degrees may have actually decreased in the short-run, but the resulting effect on the average quality of degree holders in these states may yet have increased depending on how the characteristics of the marginal degree recipient and out-migrant have been affected. Finally, better data and more work are needed to determine whether the composition of high-skilled workers leads to any increased economic growth for the state. Even if it does, further research will also be required to determine how distorting migration between states affects economic growth at a national level.

Appendix A: Merit Aid States and Program Characteristics

Table A.1
Timing of Merit Aid Program Introduction

State	Year Program Introduced	Funding Cap at Public Institutions	Private Allowed	Max Eligibility Horizon
Arkansas	1991	\$5,000	Yes	4 years
Georgia	1993	Full Tuition *	Yes	4 years
Mississippi	1996	\$500-\$1,000 *	Yes	4 years
Florida	1997	\$1,100-\$1,600	Yes	7 years
New Mexico	1997	Full Tuition	No	4 years
Louisiana	1998	Full Tuition ⁺	Yes	4 years
South Carolina	1998	\$5,000	Yes	4 years
Kentucky	1999	\$100-\$1,000 ⁺	Yes	5 years
Nevada	2000	\$1,440 *	Yes	6 years
Michigan	2000	\$1,000	Yes	4 years
West Virginia	2002	\$4,750	Yes	6 years
Maryland	2003 [†]	\$3,000	Yes	4 years
South Dakota	2004	\$1,000 *	Yes	9 years
Tennessee	2004 ^{††}	\$4,000	Yes	5 years
Wyoming	2006	\$1,600-\$3,200 ⁺	No	6 years

Note: Only includes merit aid programs where eligibility was lenient enough to include at least 30 percent of high school students (Dynarski, 2004). The column labeled private allowed indicates whether the scholarship can be used at in-state private institutions. Max eligibility horizon indicates the number of years from high school graduation a student would have before her eligibility for the scholarship expired.

* indicates the scholarship amount depends on course load or year in school.

+ indicates scholarship amount depends on academic performance level (e.g. GPA or standardized test scores).

† Maryland only offered a program for two years.

†† Tennessee introduced its program in 2003, offering scholarships to currently enrolled first and second year college students and high school graduates in the following year.

Appendix B: Proof of Equation 2

Using the notation from Section 5.7, we first define the observed outcomes of state residence and degree attainment:

$$\begin{aligned} S_i &\equiv M_i \cdot S_i(1) + (1 - M_i) \cdot S_i(0) \\ C_i &\equiv M_i \cdot C_i(1) + (1 - M_i) \cdot C_i(0) \end{aligned}$$

Next, we can show the following:

$$\begin{aligned} [S_i(1)C_i(1) - S_i(0)C_i(0)] &= [S_i(1)C_i(1)] - [S_i(0)C_i(0)] \\ &= \Pr(S_i=1, C_i=1|M_i=1) - \Pr(S_i=1, C_i=1|M_i=0) \\ &= \Pr(S_i=1|C_i=1, M_i=1) \cdot \Pr(C_i=1|M_i=1) \\ &\quad - \Pr(S_i=1|C_i=1, M_i=0) \cdot \Pr(C_i=1|M_i=0) \\ &= \Pr(S_i=1|C_i=1, M_i=1) \cdot \Pr(C_i=1|M_i=1) \\ &\quad - \Pr(S_i=1|C_i=1, M_i=0) \cdot \Pr(C_i=1|M_i=0) \\ &\quad + \{\Pr(S_i=1|C_i=1, M_i=0) \cdot \Pr(C_i=1|M_i=1) \\ &\quad - \Pr(S_i=1|C_i=1, M_i=0) \cdot \Pr(C_i=1|M_i=1)\} \\ &= \Pr(C_i=1|M_i=1) \times [\Pr(S_i=1|C_i=1, M_i=1) \\ &\quad - \Pr(S_i=1|C_i=1, M_i=0)] \\ &\quad + [\Pr(C_i=1|M_i=1) - \Pr(C_i=1|M_i=0)] \\ &\quad \times \Pr(S_i=1|C_i=1, M_i=0) \\ &= [C_i(1)] \cdot [S_i(1|C_i(1)=1) - S_i(0|C_i(0)=1)] \\ &\quad + [C_i(1) - C_i(0)] \cdot [S_i(0|C_i(0)=1)]. \end{aligned}$$

In the second line, we used the definitions of the potential outcomes and in the third line we made use of Bayes' rule. In the fourth line, we added and subtracted a term — which leaves the equation unaffected — and in the fifth line, we rearranged terms. Finally, in the sixth line, we again make use of the definitions of the potential outcomes.

Appendix C: Robustness Checks

C.1 Event Study Analysis

To explore the extent to which our results may be confounded by pre-treatment trends in merit aid states, we now use a more flexible specification to determine whether there may be patterns in our outcomes of interest that predate the introduction of merit aid. To do this, we employ the following specification:

$$y_{sct} = \sum_{k=-11}^{16} \beta_k D_{sct}^k + \eta_s + \delta_c + \gamma_t + \Gamma X_{sct} + \varepsilon_{sct} \quad (\text{C.1})$$

In equation C.1, we include a set of new dummy variables, D_{sct}^k . These variables take a value of one if cohort c is k cohorts removed from the last cohort not eligible for merit aid in state s and zero otherwise. In our estimation of (C.1) we include a dummy variable for each of the possible cohorts before and after treatment. Figure C.1 plots the coefficient estimates and 95 percent confidence intervals from estimating equation C.1 for each of our outcomes in turn. In each panel, the solid line presents estimates of β_k . The dashed lines present the confidence intervals for each of the coefficient estimates.³³

Three observations are worth mentioning. First, our identification strategy relies on the assumption that there were no pre-treatment trends in educational attainment and residential migration for cohorts in states that introduce merit aid programs relative to those that do not. The evidence in Figure C.1 generally supports this notion. There is no statistically significant pattern of a pre-treatment trend in any of the outcomes. However, the point estimates suggest a slight upward trend in the case of living in-state (Panel a) and living in-state with some college attendance (Panel d), and a more pronounced trend for some college attendance (Panel b). These trends appear to be driven by extreme cohorts, in excess of 10 years before the introduction of the merit programs. These cohorts comprise a relatively small share of our sample. Our analysis below in Section C.2 provides additional evidence that the pre-treatment trends are not driving our results.³⁴

Second, in estimating individual effects for each cohort, we sacrifice a good deal of power. As a result, our confidence intervals in these figures are fairly wide. Finally, our estimates become much more erratic as we trace out the effects to those later cohorts in states where the programs have been in existence the longest. These later cohorts tend to be dominated by a select few early adopting states. For this reason, we focus on the patterns among cohorts closer to the inception of the program, which are comprised of a more balanced sample of merit aid states. In general, the patterns in Figure C.1 reinforce those described earlier using the traditional difference-in-difference specification.

C.2 Placebo Tests

We can further assess the validity of our identification strategy by conducting placebo tests. In this case, we use only the pre-treatment data on merit aid states. We define a placebo treatment date equal to three, four, five or six years before the actual date of merit aid introduction. With each different placebo treatment date, the sample is defined to have the

³³Because the omitted category is the last ineligible cohort, its coefficient is constrained to be zero, which is why there are no confidence interval bounds at time zero in the graph.

³⁴We present results in Appendix E that omit data from 1990, i.e. cohorts more than 10 years before merit aid program introduction. In this case, the visual evidence of any pre-treatment trends is no longer apparent. The results we estimate are also attenuated in this subsample. This may suggest that our patterns in the full sample are affected by pre-existing trends. However, it is not clear that the data omitting 1990 should directly correspond to the full sample, as dropping 1990 restricts the within state variation in merit aid eligibility to be more concentrated among later adopting states.

same number of pre- and post-treatment cohorts. We then estimate equation 1 on each sample. The results of our placebo exercise are in Table C.1. Only two of the twenty estimates have p-values lower than 10 percent. This evidence further supports the assumptions underlying our difference-in-difference estimation strategy.³⁵

C.3 Addressing Intra-Cluster Correlation of Errors

Thus far, we have clustered our standard errors at the state level in order to deal with the possible correlation in the error terms among the members of a cohort and across cohorts within each state. However, this approach typically relies on the number of clusters being large. To be sure our inference is conservative, here we consider three leading methods for addressing intra-cluster correlation in the presence of a small number of clusters: the non-parametric cluster bootstrap-t technique, the wild cluster bootstrap-t technique and randomization inference.

We implement our non-parametric cluster bootstrap-t and wild cluster bootstrap-t following the prescriptions of Cameron, Gelbach and Miller (2008). In the first case, clusters of cohorts, grouped by state, are resampled with replacement from our data. Our specifications are then rerun, generating a Wald statistic. After many draws, our original t-statistic is compared to the empirical distribution of bootstrapped t-statistics. The wild bootstrap-t procedure generates pseudo-samples using clustered residuals from a regression imposing the null hypothesis. We then relate our true Wald statistic to an empirical distribution of bootstrapped t-statistics. Further details are available in Cameron, Gelbach and Miller (2008).

Randomization inference involves a permutation method proposed by Bertrand, Duflo and Mullainathan (2002).³⁶ In this case, we generate placebo treatments by randomly reassigning merit status and timing of merit aid at the state level. The timing is drawn from the empirical distribution of merit introduction dates. We then estimate our regressions under these reassigned policies and generate a Wald statistic, simulating effects under the null hypothesis. Our original t-statistic is again compared to this distribution of placebo statistics.

Table C.2 presents the results of these alternative methods of inference. Each column represents a different outcome, where we focus on the specification with only merit states, i.e. Column (3) in Tables 2–5. In the first row, we remind the reader of the point estimate, and in the final four rows, we present p-values for each of the four inference methods discussed above. In general, the results are in accord with each other, with statistical significance slightly decreasing as we move from clustered standard errors to the nonparametric bootstrap to the wild bootstrap and finally to randomization inference. In our most conservative case, randomization inference renders one of our effects — BA attainment — insignificant. These results generally indicate that our estimates are not overly reliant on a failure to account for a small number of clusters.

³⁵Note that even in cases where we find no effect of merit aid, it is still important to verify that there are no pre-treatment trends, so as not to mistake the flattening of an existing trend for a zero effect.

³⁶Note, this method is described in the working paper version of their paper, but not in the published version.

Table C.1
Placebo Treatment Effects

	(1)	(2)	(3)	(4)	(5)
	Living In-State	Living In-state w/ BA	BA Attainment	Currently Enrolled in College	Currently Enrolled in College In- State
Panel A. Placebo 3 Years Before Treatment Merit Eligible <i>N</i> = 474	0.003 (0.003)	0.000 (0.002)	-0.002 (0.003)	0.001 (0.002)	0.001 (0.004)
Panel B. Placebo 4 Years Before Treatment Merit Eligible <i>N</i> = 660	0.001 (0.004)	-0.007 *** (0.002)	-0.006 * (0.003)	0.000 (0.002)	-0.001 (0.002)
Panel C. Placebo 5 Years Before Treatment Merit Eligible <i>N</i> = 823	-0.004 (0.004)	-0.001 (0.002)	0.001 (0.004)	-0.002 (0.003)	0.000 (0.002)
Panel D. Placebo 6 Years Before Treatment Merit Eligible <i>N</i> = 949	0.001 (0.005)	0.001 (0.002)	-0.001 (0.003)	0.000 (0.002)	0.001 (0.002)

Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. Here, treatment is defined to occur in the year indicated by the row header. Each sample includes an equal number of before and after the placebo treatment year and all data occurring after the actual introduction of merit aid are dropped from the sample. The dependent variable is the share of a cohort born in a state and aged 24 to 32 at the time of the survey who have the outcome indicated by the column header. Each regression includes state, year, cohort and age fixed effects and is analogous to column (3) of Tables 2 – 5. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state level.

***, ** and * indicate statistically significant coefficients at the one, five and ten percent levels respectively.

Table C.2
Robustness Check: Intra-Cluster Correlation of Errors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Living In-State	BA Attainment	Only Some College	Living In- State w/ BA	Living In-State w/Only Some Col.	Currently Enrolled in College	Currently Enrolled in College In- State
Merit Eligible	0.011	-0.006	0.008	-0.001	0.009	0.003	0.005
p-values:							
Clustered SE	0.003 ***	0.061 *	0.001 ***	0.619	0.000 ***	0.002 ***	0.003 ***
Bootstrap-t	0.018 **	0.052 *	0.000 ***	0.668	0.002 ***	0.008 ***	0.024 **

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Living In-State	BA Attainment	Only Some College	Living In-State w/ BA	Living In-State w/ Only Some Col.	Currently Enrolled in College	Currently Enrolled in College In-State
Wild Bootstrap-t	0.018**	0.096*	0.006***	0.722	0.006***	0.010**	0.032**
Rand. Inference	0.044**	0.152	0.010**	0.720	0.018**	0.026**	0.046**

Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. Coefficients in first row correspond to original treatment effects estimated column (3) Tables 2 – 5. Each regression includes state, year, cohort and age fixed effects. Regressions are weighted by the number of observations in the given state-cohort-year cell. Each of the four bottom rows corresponds to a different method of inference. The clustered standard error p-values are reported from the specifications used in Tables above. The Bootstrap-t (nonparametric) and Wild Bootstrap-t are implemented as prescribed in Cameron, Gelbach and Miller (2008). Finally, Randomization Inference p-values are generated via a permutation method similar to the one described in Bertrand, Duflo and Mullainathan (2002).

***, ** and * indicate statistically significant coefficients at the one, five and ten percent levels respective

Figure C.1. Event Study Figures

Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. Solid lines plot coefficients on dummy variables for each cohort relative to the last cohort ineligible for merit aid. Sample includes all states and the regressions also include cohort, state and year of survey fixed effects as well as time-varying state characteristics as described in the text. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state level. Dotted lines represent upper and lower 95 percent confidence intervals. Each panel reports information from a separate regression with the dependent variable indicated by the panel label.

Appendix D: Additional Results

Appendix E: Omitting the 1990 Census

Table E.1
Effects of Merit Aid Eligibility, Ages 24 to 32, Omitting 1990 Census

	(1)	(2)	(3)
Outcome	All States	Merit States	
Living In-State	0.002 (0.007)	0.005 (0.005)	0.003 (0.005)
Living In-State w/ BA	-0.005 * (0.002)	-0.002 (0.002)	-0.002 (0.003)
Living In-State w/ Only Some College	0.003 (0.003)	0.003 * (0.002)	0.002 (0.002)
BA Attainment	-0.004 (0.003)	-0.003 (0.003)	-0.004 (0.003)
Only Some College	0.003 (0.003)	0.001 (0.002)	0.002 (0.002)
Currently Enrolled in College	0.003 * (0.002)	0.002 (0.001)	0.002 (0.001)
Currently Enrolled in College In-State	0.003 (0.002)	0.003 ** (0.001)	0.003 ** (0.001)
N	5,049	5,049	1,485
Trend	N	Y	N

Note: Estimates use 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. The dependent variable is the share of a cohort born in a state and aged 24 to 32 at the time of the survey for whom the designated outcome is true. Each regression includes state, year, cohort and age fixed effects. Where indicated, the regression also includes a trend in year of birth relative to the year the first treated cohort was born for states with merit aid programs. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state level.

***, ** and * indicate statistically significant coefficients at the one, five and ten percent levels respectively.

Figure E.2. Event Study Figures, Omitting 1990 Census

Note: Estimates use 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. Solid lines plot coefficients on dummy variables for each cohort relative to the last cohort ineligible for merit aid. Sample includes all states and the regressions also include cohort, state and year of survey fixed effects as well as time-varying state characteristics as described in the text. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state level. Dotted lines represent upper and lower 95 percent confidence intervals. Each panel

reports information from a separate regression with the dependent variable indicated by the panel label.

Appendix F: Using Unallocated Census Data

Table F.1
Effects of Merit Aid Eligibility, Ages 24 to 32, Omitting
Allocated Data

	(1)	(2)	(3)
Outcome	All States		Merit States
Living In-State	0.008 (0.008)	0.002 (0.007)	0.012 ** (0.004)
Living In-State w/ BA	-0.007 ** (0.003)	-0.002 (0.002)	-0.002 (0.003)
Living In-State w/ Only Some College	0.010 *** (0.004)	0.003 (0.004)	0.009 *** (0.002)
BA Attainment	-0.010 *** (0.003)	-0.003 (0.003)	-0.007 * (0.004)
Only Some College	0.011 *** (0.004)	0.002 (0.003)	0.008 *** (0.002)
Currently Enrolled in College	0.007 *** (0.002)	0.003 * (0.002)	0.003 ** (0.001)
Currently Enrolled in College In-State	0.007 *** (0.002)	0.004 * (0.002)	0.005 *** (0.002)
<i>N</i>	5,508	5,508	1,620
Trend	N	Y	N

Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. The dependent variable is the share of a cohort born in a state and aged 24 to 32 at the time of the survey for whom the designated outcome is true. Each regression includes state, year, cohort and age fixed effects. Where indicated, the regression also includes a trend in year of birth relative to the year the first treated cohort was born for states with merit aid programs. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state level.

***, ** and * indicate statistically significant coefficients at the one, five and ten percent levels respectively.

Appendix G: Using Different Age Ranges

Table G.1
Effects of Merit Aid Eligibility, Ages 24 to 37

	(1)	(2)	(3)
Outcome	All States		Merit States
Living In-State	0.006 (0.009)	0.000 (0.008)	0.009 * (0.005)
Living In-State w/ BA	-0.006 * (0.003)	-0.002 (0.003)	-0.001 (0.003)
Living In-State w/ Only Some College	0.009 ** (0.004)	0.002 (0.004)	0.008 *** (0.002)
BA Attainment	-0.007 ** (0.003)	-0.001 (0.004)	-0.005 (0.004)
Only Some College	0.010 *** (0.003)	0.001 (0.003)	0.006 ** (0.002)
Currently Enrolled in College	0.006 *** (0.002)	0.002 (0.002)	0.004 ** (0.002)
Currently Enrolled in College In-State	0.006 ** (0.003)	0.003 (0.002)	0.005 *** (0.002)
<i>N</i>	8,313	8,313	2,445
Trend	N	Y	N

Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. The dependent variable is the share of a cohort born in a state and aged 24 to 37 at the time of the survey for whom the designated outcome is true. Each regression includes state, year, cohort and age fixed effects. Where indicated, the regression also includes a trend in year of birth relative to the year the first treated cohort was born for states with merit aid programs. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state level.

***, ** and * indicate statistically significant coefficients at the one, five and ten percent levels respectively.

Appendix H: Sensitivity to the Set of Merit Aid States Included and Spillover Effects

To be sure our results are not driven by any one outlier merit aid state, we now examine how the results change when we leave out each merit aid state in turn in Table H.1. Panels A and B contain the results with the probability of living in-state and BA attainment, respectively. For living in one's state of birth, the results range from 0.7 to 1.2 percentage point increases and none are statistically differentiable from the others. The negative effect on degree attainment ranges from 0.4 to 0.8 percentage points. Again, the results are not statistically distinguishable from one another. The results in Table H.1 reassure us that there is no single state driving the decreases in residential mobility and degree attainment apparent in the full sample.

It could be the case that merit aid programs in neighboring states affect the retention of college students. For example, when Georgia introduced HOPE, more Georgians stayed instate for college, which likely meant that fewer Floridians migrated to Georgia for college. In turn, these Floridians may have been more likely to stay in Florida for college and afterwards. If this is the case, our estimates will be biased downward because our comparison group has increased retention due to their neighbors' merit aid program. To be sure this type of spillover is not affecting the size of our estimated effects of merit aid, we also estimate the effects of a state's merit aid programs using indicator variables to control for the timing of merit aid adoption by neighboring states. This produces results that are quite similar to those discussed, which suggests our main estimates are not attenuated due to issues of spillovers with neighboring states.³⁷

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³⁷Results available from authors upon request.

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Highlights

- Merit aid programs slightly increase the propensity of state natives to live in-state, and extend in-state college enrollment into the late twenties.
- Merit aid programs have little effect on the share of a cohort both living in-state and having a BA, with a possible decline in overall BA attainment.
- These patterns notwithstanding, the magnitude of merit aid effects of an order of magnitude smaller than size of the treated population, suggesting that nearly all of the spending on these programs transfers resources to individuals whose ultimate migration decisions remain unchanged.

Table 1
Descriptive Statistics, Census and American Community Survey, 1990 – 2010, Ages 24 to 32

	(1)	(2)
	All States	Merit States
Female	0.50	0.50
Black	0.14	0.22
Hispanic	0.14	0.08
Current Unemployment	0.05	0.06
Lagged Unemployment	0.06	0.07
Living In-State	0.66	0.66
BA Attainment	0.27	0.23
Only Some College	0.33	0.32
Living In-State w/ BA	0.15	0.12
Living In-State w/ Only Some Col	0.22	0.21
Currently Enrolled	0.08	0.08
Currently Enrolled In-State	0.05	0.05
<i>N</i>	5,508	1,620

Note: Statistics calculated using 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data collapsed at the state-cohort-year-level for individuals ages 24 to 32 at the time of survey. Cell averages are weighted by the number of observations in the given state-cohort-year cell.

Table 2
Effects of Merit Aid Eligibility on Probability of Living in One's State of Birth, Ages 24 to 32

	(1)	(2)	(3)
	All States		Merit States
Panel A. No Age Fixed Effects Merit Eligible	0.009 (0.008)	0.003 (0.007)	0.011 ** (0.004)
Panel B. Age Fixed Effects Merit Eligible	0.009 (0.008)	0.003 (0.007)	0.011 ** (0.004)
<i>N</i>	5,508	5,508	1,620
Trend	N	Y	N

Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year-level. The dependent variable is the share of a cohort born in a state and aged 24 to 32 at the time of the survey that are living in their state of birth. Each regression includes state, year and cohort fixed effects. Panel B also includes age effects. Where indicated, the regression also includes a trend in year of birth relative to the year the first treated cohort was born for states with merit aid programs. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state level.

***, ** and * indicate statistically significant coefficients at the one, five and ten percent levels respectively.

Table 3
Effects of Merit Aid Eligibility on Probability of Living in One's State of Birth and
Attending or Graduating from College, Ages 24 to 32

	(1)	(2)	(3)
	All States		Merit States
Panel A. BA Attainment Merit Eligible	-0.006 ^{**} (0.003)	-0.002 (0.002)	-0.001 (0.003)
Panel B. Only Some College Merit Eligible	0.010 ^{***} (0.004)	0.004 (0.004)	0.009 ^{***} (0.002)
<i>N</i>	5,508	5,508	1,620
Trend	N	Y	N

Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. The dependent variable is the proportion of a cohort born in a state and between the ages of 24 and 32 at the time of the survey who both live in the state at the time of the survey and have reached the level of educational attainment indicated by the Panel header. Each regression includes state, year, cohort and age fixed effects. Where indicated, the regression also includes a trend in year of birth relative to the year the first treated cohort was born for states with merit aid programs. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state level.

^{***}, ^{**} and ^{*} indicate statistically significant coefficients at the one, five and ten percent levels respectively.

Table 4
Effects of Merit Aid Eligibility on Educational Attainment, Regardless of Current Residence, Ages 24 to 32

	(1)	(2)	(3)
	All States		Merit States
Panel A. BA Attainment Merit Eligible	-0.009 *** (0.003)	-0.003 (0.003)	-0.006 * (0.003)
Panel B. Only Some College Merit Eligible	0.011 *** (0.004)	0.002 (0.003)	0.008 *** (0.002)
Panel C. Only High School Degree Merit Eligible	0.005 (0.004)	-0.006 (0.006)	-0.005 (0.006)
<i>N</i>	5,508	5,508	1,620
Trend	N	Y	N

Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. The dependent variable is the share of a cohort born in a state and aged 24 to 32 at the time of the survey who have obtained a BA, only some college or only a high school degree, regardless of current residence. Each regression includes state, year, cohort and age fixed effects. Where indicated, the regression also includes a trend in year of birth relative to the year the first treated cohort was born for states with merit aid programs. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state level.

***, ** and * indicate statistically significant coefficients at the one, five and ten percent levels respectively.

Table 5
Effects of Merit Aid Eligibility on Current College Attendance, Ages 24 to 32

	(1)	(2)	(3)
	All States		Merit States
Panel A. Currently Enrolled in College Merit Eligible	0.007 *** (0.002)	0.003 * (0.001)	0.003 ** (0.001)
Panel B. Currently Enrolled in College In-State Merit Eligible	0.007 ** (0.002)	0.003 * (0.002)	0.005 ** (0.002)
<i>N</i>	5,508	5,508	1,620
Trend	N	Y	N

Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. The dependent variable is the share of a cohort born in a state and aged 24 to 32 at the time of the survey who have completed a HS education, are currently enrolled in school and have not yet attained a BA in any state in the U.S. (Panel A) or in school in their state of birth (Panel B). Each regression includes state, year, cohort and age fixed effects. Where indicated, the regression also includes a trend in year of birth relative to the year the first treated cohort was born for states with merit aid programs. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state-cohort level.

***, ** and * indicate statistically significant coefficients at the one, five and ten percent levels respectively.

Table 6
Effects of Merit Aid, By Race and Gender, Ages 24 to 32

	(1)	(2)	(3)	(4)
Outcome	Black	White	Female	Male
Living In-State	0.027 * (0.014)	0.003 (0.005)	0.010 ** (0.004)	0.010 * (0.006)
Living In-State w/ BA	0.008 ** (0.003)	-0.003 (0.003)	-0.002 (0.004)	0.000 (0.002)
Living In-State w/ Only Some College	0.007 * (0.004)	0.009 * (0.004)	0.012 *** (0.003)	0.004 (0.003)
BA Attainment	0.004 (0.005)	-0.010 * (0.005)	-0.009 * (0.005)	-0.006 (0.004)
Only Some College	0.009 * (0.004)	0.007 * (0.004)	0.010 ** (0.004)	0.003 (0.003)
Currently Enrolled in College	0.006 (0.004)	0.003 (0.002)	0.007 *** (0.002)	0.001 (0.002)
Currently Enrolled in College In-State	0.003 (0.002)	0.004 ** (0.002)	0.008 *** (0.002)	0.002 (0.002)
<i>N</i>	1,494	1,620	1,620	1,620

Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year-race or state-cohort-year-gender level. Each estimate is from a separate regression using only individuals in the group indicated by the column header. The dependent variable is the share of a cohort of the indicated race or gender born in a state and aged 24 to 32 at the time of the survey for whom the designated outcome in the row header is true. Each regression includes state, year, cohort and age fixed effects and is restricted to strong merit aid states. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state level.

***, ** and * indicate statistically significant coefficients at the one, five and ten percent levels respectively.

Table 7
Heterogeneous Effects of Merit Aid, Ages 24 to 32

	(1)	(2)	(3)	(4)
Outcome	Merit Aid	Merit Aid $\times$ Public Only	Fraction of Tuition Covered	Awards per HS Graduate
Living In-State	0.010 ** (0.004)	0.021 ** (0.010)	0.007 (0.007)	0.026 * (0.014)
Living In-State w/ BA	-0.001 (0.002)	-0.015 ** (0.005)	-0.002 (0.003)	-0.007 (0.011)
Living In-State w/ Only Some College	0.009 *** (0.002)	0.005 (0.011)	0.011 ** (0.004)	0.035 *** (0.011)
BA Attainment	-0.005 (0.003)	-0.020 *** (0.003)	-0.006 * (0.004)	-0.022 (0.014)
Only Some College	0.009 *** (0.002)	-0.016 *** (0.005)	0.010 ** (0.004)	0.035 ** (0.012)
Currently Enrolled in College	0.004 *** (0.001)	-0.004 (0.004)	0.003 *** (0.001)	0.012 ** (0.005)
Currently Enrolled in College In-State	0.005 ** (0.002)	-0.003 (0.007)	0.004 * (0.002)	0.016 ** (0.007)
<i>N</i> = 1,620				

Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. The dependent variable is the share of a cohort born in a state and aged 24 to 32 at the time of the survey for whom the designated outcome in the row header is true. Columns (1) and (2) present results from a specification with a general merit indicator and an indicator for those merit aid states allowing enrollment only at public institutions of higher education. Column (3) presents results using a merit variable defined by the fraction of average tuition and fees at 4-year public institutions in the state that are covered by the merit program. Column (4) presents results using the ratio of merit awards to high school seniors. Each regression includes state, year, cohort and age fixed effects and is restricted to strong merit aid states. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state level.

***, ** and * indicate statistically significant coefficients at the one, five and ten percent levels respectively.

Table D.1

Comparison of BA Degree Results Across Different Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Arkansas and Georgia Only	0.025 ^{***} (0.004)	0.016 ^{**} (0.006)	0.008 ^{**} (0.004)	-0.007 (0.009)	-0.009 (0.006)	-0.007 (0.005)	-0.011 ^{***} (0.003)
All Merit Aid States					-0.006 (0.004)	-0.010 [*] (0.005)	-0.007 [*] (0.004)
Years	2000	1990-2000	2000	1990-2000	1990-2010	1990-2010	1990-2010
Samples	1 percent	1 percent	5 percent	5 percent	5 percent	5 percent	5 percent
Age by Year, Age by State FE	N	Y	N	Y	Y	N	N
Cohort FE	N	N	N	N	N	N	Y
Weighted	N	N	N	N	N	N	Y
Allocated Data	N	N	N	N	N	N	Y
Ages	22-34	22-34	22-34	22-34	22-34	22-34	24-32

Note: Estimates use 1990 and 2000 Decennial and 2001-2010 ACS data at the state-cohort-year level. The dependent variable is the share of a cohort born in a state and aged 22 to 34 (or 24 to 32) at the time of the survey who have obtained a BA degree. Each regression includes state, year and age fixed effects. Where indicated, age by year fixed effects are also included, data is not weighted by Census, observations with allocated information on state of birth, date of birth or educational attainment are omitted. Observations from Mississippi are also omitted. When the coefficient of interest is the effect of merit aid in Arkansas and Georgia only and the data extend past 2000, other merit aid states are dropped from the analysis. Standard errors are clustered at the state level.

***, ** and * indicate statistically significant coefficients at the one, five and ten percent levels respectively

Table H.1

Sensitivity Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Omitted State	Arkansas	Florida	Georgia	Kentucky	Louisiana	Maryland	Michigan	Mississippi	Nevada	New Mexico	S. Carolina	S. Dakota	Tennessee	W. Virginia
Panel A. Living In-State Merit Eligible	0.009 [*] (0.004)	0.009 ^{***} (0.003)	0.012 [*] (0.006)	0.012 ^{**} (0.004)	0.014 ^{***} (0.004)	0.012 ^{***} (0.003)	0.013 ^{***} (0.004)	0.011 ^{***} (0.005)	0.011 ^{***} (0.004)	0.010 [*] (0.004)	0.012 ^{**} (0.004)	0.012 ^{**} (0.004)	0.011 ^{**} (0.003)	0.011 ^{**} (0.004)
Panel B. BA Attainment Merit Eligible	-0.007 [*] (0.004)	-0.007 (0.004)	-0.006 (0.004)	-0.006 [*] (0.003)	-0.007 ^{**} (0.003)	-0.003 (0.003)	-0.007 [*] (0.004)	-0.006 [*] (0.003)	-0.007 [*] (0.003)	-0.006 (0.003)	-0.006 (0.004)	-0.007 [*] (0.003)	-0.007 (0.004)	-0.005 (0.003)
<i>N</i> = 1,512														

Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. The sample includes all of the merit aid states other than the state indicated in the column header. Specifications correspond to those in Column (3) of Tables 2 and 4. The dependent variable is the share of a cohort born in a state and aged 24 to 32 at the time of the survey who have the outcome indicated by the panel header. Each regression includes state, year, cohort and age fixed effects. Regressions are weighted by the number of observations in the given state-cohort-year cell. Standard errors are clustered at the state level.

^{***}, ^{**} and ^{*} indicate statistically significant coefficients at the one, five and ten percent levels respectively.