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HIGHER EDUCATION, MERIT-BASED SCHOLARSHIPS AND POST-BACCALAUREATE MIGRATION

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ABSTRACT

Many merit-based scholarships for college are administered at the state level, targeted to in-state residents and require attendance at an in-state institution. Though these subsidies have the potential to affect lifetime education and migration decisions, much of the literature to date has focused on just one or two outcomes (e.g. college attendance and completion) and one or two states (e.g. Georgia). Given that one of the stated goals of these programs is to increase the quality of a state's workforce, understanding the long-term effects of merit-based scholarships on mobility is crucial for evaluating their effectiveness. In this paper, we utilize the broader expansion and long history of these programs to build a comprehensive picture of how merit aid scholarship availability affects residential migration and educational attainment. To do this, we incorporate data on the introduction of broad-based merit aid programs for fifteen states and Census data on all 24 to 32 year olds in the U.S. from 1990 to 2010. We use variation in merit aid eligibility across cohorts and within states to identify treatment effects. Eligibility for merit aid programs slightly increases the propensity of state natives to live in-state, while also extending enrollment in-state into the late twenties. These patterns notwithstanding, the magnitude of merit aid effects is of an order of magnitude smaller than the population treated, suggesting that nearly all of the spending on these programs is transferred to individuals who do not alter educational or migration behavior.

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1 Introduction

Merit-based scholarships given to residents for in-state college attendance have become increasingly popular. These scholarships reward students who perform above a minimum academic level with grants, provided that they attend an in-state, postsecondary institution – often either private or public. One of the earliest and most prominent 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, with aims ranging from incentivizing greater preparedness among high school students to increasing enrollment in-state institutions of postsecondary education to retaining a high skill labor force. In the 2010-2011 academic year, 28 states offered some sort of merit 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 enrollment (Dynarski, 2000; Cornwell et al., 2006; Orsuwan and Heck, 2008; Zhang and Ness, 2010). Since the time of these initial studies, merit-based scholarship programs have become widespread and have been in effect long enough to impact longer-term outcomes such as lifetime schooling and migration decisions.

In this paper, 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 mobility and completed schooling levels among residents old enough to have potentially completed their college educations. Our goal in this paper is to provide a more comprehensive framework for understanding the effects of merit-based scholarships on residents' migration and educational attainment patterns. To do this, we incorporate data on the introduction of merit aid programs for fifteen states and Decennial Census and American Community Survey data on all 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

[1] We 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 Section II (Dynarski, 2000, 2004).

mobility and educational attainment stems from differences in these outcomes for cohorts of residents in states with and without merit-based scholarships just before and after the programs were introduced.

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. By lowering the price of in-state college attendance, the merit aid scholarships may induce some students to attend college who otherwise would not have and may also induce students who would have out-migrated to stay in the state for college. However, if the state aid merely crowds out federal assistance such as the Pell grant (Cornwell et al., 2006) or if tuition and fees increase with merit scholarship introduction (Long, 2004), then there may not be any net effect on enrollment, particularly of low-income college attendees. Also, if supply of higher education is not perfectly elastic (Bound and Turner, 2007), the retention of students who would have otherwise migrated out of the state may serve to crowd other students out of postsecondary institutions. Previous studies typically find a positive effect on overall college enrollment (Dynarski, 2000; Cornwell et al., 2006; Conley and Taber, 2011) and a concurrent increase in in-state college attendance (Dynarski, 2000; Cornwell et al., 2006; Orsuwan and Heck, 2008; Zhang and Ness, 2010).[2]

Of equal importance is whether changes in college enrollment are translated into changes in college completion. This will depend on whether there are changes in attendance and, if so, whether the marginal student induced to change her college-going behavior by the scholarship has the ability to complete college. Along those lines, some states condition the scholarships on satisfactory academic performance while in college, which may increase persistence. On the other hand, if merit aid introduces mismatch between students and institutions or if students are crowded out of classes by increased in-state enrollment, students may be less likely to persist. In addition, the incidence of merit aid on non-scholarship students may

[2]Goodman (2008) also finds that a merit-based scholarship provided only for attendance at public postsecondary institutions of higher education increased the propensity of high school students in Massachusetts to attend these schools.

be manifested in higher tuition (Long, 2004) and, as a result, lower completion rates among credit-constrained students. Here the results have been more mixed. Some studies find increases in educational attainment (Dynarski, 2008; Scott-Clayton, 2011) while others find little effect (Sjoquist and Winters, 2012a). These studies have all focused on one or two states, making it difficult to know whether the results generalize to other settings.

Another interesting research question that has received less attention is whether or not the increased likelihood of attending an in-state postsecondary institution as a result of merit-based scholarships in turn leads to an increased likelihood of a student remaining in the home state after graduation. If college provides location-specific human or social capital, students who are induced to remain in their home state for college may remain in the state after graduation. On the other hand, if the skills acquired during college are relatively portable geographically, states with merit aid policies may find themselves paying to train the future workforces of other states.[3] In a recent exception, Sjoquist and Winters (2012b) use administrative data on enrollment and employment in Georgia to measure whether the HOPE scholarship program changed post-schooling retention of in-state college attendees.[4] They find HOPE had little effect on retention, but their analysis is limited to studying the effects of HOPE using information encompassing only attendees at public institutions and employees actually working in Georgia. To the extent that the program has broader effects (e.g. on private school attendees) or that it differs from other broad-based merit aid programs, these results may not generalize.

Our contribution to this literature is fourfold. First, the long horizon over which some of these programs have been operating allows us to focus our analysis to a larger set of lifecycle outcomes than previously available, most prominently residential migration. Second, as mentioned, we expand the scope of our analysis to include fifteen states with broad based

[3]There is a broader literature focused on the effects of attending college in a state on the probability of remaining in the state for employment after graduation. Of particular note is Groen (2004), who addresses the issue of selection into in-state colleges using college application choices to create comparison groups. Groen (2004) finds modest effects of college location choice on future residential decisions.

[4]The comparison is of migration patterns in their Georgia administrative data to those in other states in the Current Population Survey for cohorts of residents before and after merit aid introduction.

merit aid programs. Third, the extended time horizon we use allows us check whether differential, pre-treatment trends in migration and educational patterns among merit-adopting states are our difference-in-difference results. Finally, we offer a unified interpretation of the various effects of merit aid on migration and educational attainment that captures the key tradeoffs for states offering these grants.

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 (2 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 ever attended college or to have received a bachelor's degree. Third, we find that merit aid eligible 24 to 32 year olds residents are 0.4 percentage points more likely to be currently enrolled in college. 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 (18 to 23 year olds), ours is the first study we know of to recover a slight increase in college enrollment among older students, aged 24 to 32 year olds.

Unfortunately, our primary 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 attending college elsewhere. We therefore investigate whether other administrative data on first-time college enrollment provide estimates consistent with a behavioral change in location of college enrollment. This additional evidence suggests increased retention of in-state residents at postsecondary institutions, reinforcing the notion that that merit cohorts do respond by staying home for college.

While our estimates do suggest some scope for merit aid programs to impact outcomes, the extent of their impact is strikingly limited. The magnitude of our effects ($\leq$ 3 percent) is of an order of magnitude smaller than the target population ($\geq$ 30 percent). 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.

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 impact on both the economies of merit aid states and the country as a whole.

2 Merit Aid Programs

Merit-based scholarships given to residents for in-state college attendance have become increasingly popular. Although financial aid for college that is based on a student's achievement has been around 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 state-wide 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).

Merit aid programs vary in their standards for award receipt. For example, in some states, scholarship receipt requires only the maintenance of a minimum grade point average in high school and college (e.g. Georgia) while other states also place restrictions on the minimum standardized test scores needed in order to qualify (e.g. Tennessee). The stringency of the requirements will alter the impact of a merit aid program. If the standards are so high that the marginal college student in a state are unable to meet them, they are unlikely to change enrollment. Furthermore, programs limited in scope may prove harder to detect if they only affect a very small fraction of the population. For this reason, we adopt a convention in the literature and focus on states where the merit-based scholarship program was lenient enough to include at least 30 percent of high school students (Dynarski, 2004).[5] This leads us to

[5]We therefore include the states listed in Table 1, which includes all of the states in Dynarski (2004) and two additional states that introduced programs since 2002, South Dakota and Wyoming. There are a

focus on the 15 states that have broad-based merit aid programs. In our most recent year of data, these states spent about \$1.4 billion per year, or \$2,191 per recipient per year on merit aid.[6]

Table 1 delineates the expansion of these programs over time and details their grant limits. 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. 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.

3 Empirical Strategy

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

$$y_{sct} = \beta Merit_{sct} + \eta_s + \delta_c + \gamma_t + \Gamma X_{sct} + \epsilon_{sct} \tag{1}$$

where y_{sct} is the fraction of people in the cohort born in year c in state s interviewed at the time of the survey, t, who have attained one of our outcome characteristics. Our set of outcomes includes the following: living in one's state of birth, BA degree attainment, enrollment in college, enrollment in college in one's state of birth, and combinations of the

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 an Appendix, we examine the pattern of results using just these early adopters and find that the patterns described here hold.

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

above outcomes (e.g. living in one's state of birth and having a BA degree). $Merit_{sct}$ 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 the year she turned 18, i.e. the most likely year she would enter college, had she met the merit standards.

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 demographic characteristics of a cohort, age controls, economic conditions facing the cohort in a particular survey year and lagged economic conditions facing the cohort at age 18, when the college-going decision was likely to be made.[7] We estimate these state-by-cohort-by-year level 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. Standard errors are clustered at the state level and additional inference is performed using nonparametric bootstrap and wild bootstrap procedures (Cameron et al., 2008) as well as randomization inference (Bertrand et al., 2002).

The coefficient of interest in (1) is β, which gives the probability that a person aged 24 to 32 was induced to change their behavior because of their 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. While we are not able to directly test this assumption, we can make use of our pre-treatment data to explore whether merit aid programs are passed in response to pre-existing trends in outcomes across cohorts. To do so, we present results using a more flexible, event study specification with distributed lags and leads and also conduct placebo tests using only pre-

[7] In 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.

treatment data. In addition, we examine the robustness of our results to including a linear trend in the number of years between a cohort and the first treated cohort for states that introduce merit aid programs to control for patterns that may not be captured by our observable characteristics (see Wolfers, 2006).[8] Finally, we include time-varying controls to mitigate concerns about other possible changes simultaneous with program introduction.

Beyond 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, it would bias the effect of merit aid on college attendance 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 the bias that emigrational selection into merit aid states might cause. For example, consider two families in Georgia giving birth to children in 1974 and 1975 respectively. We do not expect the second family to differ from the first based on the expectation that 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.

4 Data

We use public-use data on respondents ages 24 to 32 from the 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 2001 to 2004 American Community Surveys

[8]Since the patterns in our event study figures we show later appear linear, we expected 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 zero.

(ACS) are nationally representative samples of between 1-in-232 to 1-in-261 of the U.S. population. The later ACS samples, from 2005 to 2010, are 1-in-100 nationally representative samples. We select lower bound age of 24, as a majority (85 percent) of BAs are received by this age in our sample, and an upper bound age of 32, as this is the oldest age to have been affected by a merit scholarship.

In each of the surveys, the Census Bureau collects information on place of residence, place of birth, enrollment in school and educational attainment, which we use to create our three sets of dependent variables of interest.[9] Specifically, we are interested in i) residential migration, ii) degree completion, and iii) college attendance. First, we investigate whether merit aid program eligibility makes it more likely for residents to remain in the state in which they were born and whether high skilled people (those with college degrees) are more likely to live in their states of birth. We are also interested in whether merit aid makes it more likely that a person obtains at least a college degree (BA), regardless of their place of residence.[10] In addition, we examine both whether individuals have ever attended any college and whether they are enrolled in college at the time of the survey. That is, whether they are currently enrolled while between the ages of 24 to 32 in any state and, furthermore, in one's state of birth.

The Decennial Census and ACS also include gender and race information that we use to control for compositional changes in a state's population over time. To these data, we add information on state unemployment levels from the Bureau of Labor Statistics. Because economic conditions at the point of high school graduation may affect the probability of going to college overall and affect families' ability to finance different types of college attendance,

[9]In our analysis samples, we include individuals with imputed values of these key variables. If place of birth is imputed, it could attenuate our estimates. If any of our outcomes are imputed, it will reduce our power. Our results are generally robust to leaving out those respondents with imputed characteristics, though the resulting smaller samples lead to a slight decrease in statistical power. Results are available from the authors upon request.

[10]To 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 Associates Degree attainment, but the IPUMS compilation of Census Data includes a recoding of Associate Degree attainment in way that precludes consistent comparisons across time.

we control for the unemployment rate in a cohort's state of birth at the time of high school graduation. 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.[11]

Before turning to our estimates, it is worth noting that the roll out of merit aid programs across states over time coupled with the timing of the Census data implies that the treated states do not all contribute to our estimated effects in the same way. To illustrate, in Figure 1 we present counts of the number of states with data for each cohort relative to the last untreated cohort. The peak in the figure at 15 states indicates the 15 states that have introduced merit aid. One state, Wyoming, introduced merit aid in 2006, too early for anyone in our sample of people ages 24 or older in 2010 to have actually been eligible. Therefore, only 14 states have at least one treated cohort. Note, however, that in the figure the number of states for which we have at least five treated cohorts drops to ten and the number of states for which we have ten treated cohorts is only two. This illustrates the unbalanced nature of our treated panel and the relative importance of the contributions of early adopting states to our estimates.

Panel B of Figure 1 presents counts of the number of states with data for each cohort relative to the last untreated cohort for people in our sample ages 24 and 32 separately. Here, we see the relative importance of early adopting states for the older members of our sample in particular. While there are at least ten states for each of the five first merit aid eligible cohorts of 24 year olds, there are at most three states with eligible 32 year olds.

In Table 2, we present descriptive statistics that summarize the characteristics of our state-cohort-year cells.[12] The average demographic composition and economic conditions are as expected given the time period studied. The average rate of migration out of one's

[11]Contemporaneous 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.

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

state-of-birth for this sample is approximately 34 percent. The probability of receiving a BA degree is between 23 and 27 percent. Of these, about half, or 13 to 15 percent of individuals have a BA degree and are living in their state of birth. Meanwhile, current enrollment in a BA-granting institution among our sample of people ages 24 to 32 is 13 percent, 8 percentage points of which is in their state of birth.

5 Effects of Merit Aid Eligibility

5.1 Residential Migration

Our first set of estimates of the effects of merit aid eligibility focuses on the residential migration of people of an age at which they are likely to have completed their undergraduate schooling. Specifically, we investigate whether merit aid eligibility had any effect on the probability of people ages 24 to 32 living in their state of birth. To answer this question, in Table 3, we present estimates of equation 1 using various sets of comparison groups for the states that introduce merit aid programs. In column (1), we report 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 will live in their states of birth by 0.9 percentage points. 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.[13] The inclusion of the trend changes the estimate to 0.4 percentage points, although it is also not statistically different from the estimate in the first column.

[13] We 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.

The rest of the columns in Table 3 contain estimates of the effect of merit aid eligibility on residential mobility using alternative sets of states as the comparison group for merit aid states. Most states that have introduced merit aid programs have been geographically concentrated in the southern U.S. Following Dynarski (2008), the samples in columns (3) and (4) include merit states and any non-merit aid, Southern U.S. states, with and without the control for the trend, respectively.[14] With this comparison group, the estimated effect increases to between 1.5 and 1.9 percentage points and is now statistically significant.

It still may be the case that states that introduce merit aid programs are somehow different than those that do not. To be sure that our estimates are not biased by these types of differences, in column (5) we present estimates including only merit aid states in the sample.[15] The estimate is similar in magnitude to that in the first column, though now statistically significant. Our estimate in column (5) 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.[16] Given that about 50 percent of 24 to 32 year olds live in their home states, this represents a decrease in mobility of about 2 percent.

In Panel B of Table 3, we repeat each of the analyses described above 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. This indicates a slightly larger effect of merit-based scholarships than Sjoquist and Winters (2012b), and while our confidence intervals do overlap, our result is statistically

[14]We use the set of states defined as the South region by the Census Bureau. In addition to the merit aid states, this also includes Alabama, Delaware, Washington D.C., North Carolina, Oklahoma, Texas and Virginia.

[15]In 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.

[16]Since the estimate in column (5) 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 six models discussed here; generally, they produce a similar pattern of results.

significant and theirs is not.[17]

5.2 Residential Migration of College Graduates

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 3 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 degree. These results are presented in Table 4, which has a similar structure to Table 3.

Looking across the columns of Table 4, we see that merit aid generally does not have an effect on the probability of living in the state with at least a BA degree. With the exception of the first column specification, the estimated effect is nearly zero and is not statistically significant. For example, in column (5), the estimate is -0.1 percentage point. The differences in the results when comparing the first column to the rest may suggest that there may have been different trends in mobility and degree receipt between merit and non-merit states. Given the relative strength of the underlying identification assumption when the sample includes only merit aid eligible states, we prefer the specification in column (5). This leads us to conclude that merit aid eligibility did not change the proportion of a cohort born in a state that resides in the state and holds a BA degree.

5.3 College Completion in Any State

Thus far, we have showed that merit aid eligibility increased the probability of living in the state one was born in but did little to change the probability of both living in the state and having a BA degree. This combination of results suggests there may have been negative effects of merit aid on educational attainment. To investigate this, in Panel A of Table 5 we

[17] This may be driven by the broader set of states included in our analysis or the broader sample considered. When restricting our analysis to Georgia, we do not find as strong of a migration effect. Results are available from the authors upon request.

present results where the dependent variable of interest is BA degree attainment or higher. Across all five 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 actually reduce degree attainment. Focusing on the result in column (5) that includes only merit states, the estimate suggests that merit aid eligibility decreased BA degree attainment by 0.7 percentage points and is statistically significant at the ten percent level. Given baseline rates of degree receipt, this represents a 4 percent decrease in BA degree attainment.

In order for the probability of living in-state as a BA degree holder to stay the same while the overall probability of degree receipt among a cohort decreases, some degree holders who would have lived elsewhere between the ages of 24 to 32 must have been induced to live in their state.[18] Unfortunately, we cannot determine with this data whom the marginal degree recipient is nor where the marginal degree is earned.[19] The data are consistent with a story where ex-ante mobile college students now stay in-state to take advantage of merit aid and, in the process, crowd out ex-ante home state students from getting a degree. However, there are alternative stories that are equally consistent with the findings.[20]

5.4 College Attendance

The possible decrease in BA degree attainment is a bit surprising given the rest of the literature, which suggests that merit aid introduction increases enrollment in college and

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

[19]With 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.

[20]For example, the reduction in degrees may be concentrated among those students now induced to remain in-state, perhaps due to a lower quality match with in-state school relative to the counterfactual, out-of-state school.

graduation, particularly at in-state postsecondary institutions (Dynarski, 2004; Zhang and Ness, 2010).[21] To further understand what is driving our estimate of decreased BA degree attainment, in Panel B of Table 5 we present results where the dependent variable is having completed any amount of college, not just degree receipt. Our estimate in column (5) suggests that merit aid did little to change the probability of some college attendance for those ages 24 to 32. The point estimate is -0.001 and it is not statistically significant.[22]

The lack of a change in the probability of having started college combined with a possible decrease in degree attainment leads naturally to the question of whether these 24 to 32 year olds have indeed finished their schooling or whether they might still be currently enrolled in college. In Table 6, we re-estimate equation (1) using two measures of college enrollment: i) enrollment anywhere in the U.S. at the time of the survey (Panel A) and ii) enrollment in one's home state at the time of the survey (Panel B). Focusing on our specification in column (5), we find that cohorts eligible for merit aid have an increased likelihood of college enrollment that is driven by increased enrollment in their home states. College enrollment in any state went up by 0.4 percentage points, while in-state college attendance went up by 0.5 percentage points. Both estimates are statistically significant at the one percent level in column (5), but the two estimates are not statistically different from each other. This is consistent with about 70 percent of the extra 0.7 percentage points of 24 to 32 year olds who have not yet graduated college still being enrolled.[23] However, we cannot determine whether

[21] The differences may be driven by our inclusion of additional merit aid states, the longer time horizon we use (previous work has focused on the 2000 Decennial), specification choices about controls or sample selection choices (e.g. ages of residents included). In Appendix A, we present results from specifications replicating previous work and changing the specification along each of these dimensions. The inclusion of a longer time horizon, whether it be the 1990 or 2001 to 2010 data, renders the previously positive relationship between BA degree attainment and merit aid to be zero and using the entire set of available data causes the point estimate to be negative. Because the 1990 data provide valuable information on pre-treatment outcomes and the later data allow for the investigation of the broadest set of merit aid states, we use all years.

[22] Our 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 not reported, 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.

[23] We calculated this estimate by dividing the increase in college enrollment (0.5 percentage points) by the

the people who are no longer holding degrees and those who are now enrolled at these ages are one in the same.

5.5 Event Study Analysis

To explore the extent to which our results may be driven 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:

$$y_{sct} = \sum_{k=-11}^{16} \beta_k D_{sct}^k + \eta_s + \delta_c + \gamma_t + \Gamma X_{sct} + \epsilon_{sct} \tag{2}$$

In equation (2), we include a set of new dummy variables, D_{sct}^k. These variables take a value of one if cohort c was the k cohorts removed from the last cohort not eligible for merit aid in state s and zero otherwise. In our estimation of 2 we include a dummy variable for each of the 11 cohorts before and 16 cohorts after treatment. Figure 2 plots the coefficient estimates and 95 percent confidence intervals from estimating equation 2 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.[24]

Three observations are worth pointing out. First, our identification strategy relies on the assumption that there were no pre-treatment trends in educational attainment and residential mobility for cohorts in states that introduce merit aid programs relative to those that do not. The evidence in Figure 2 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 more pronounced trend for some college attendance (Panel B). These trends appear to be driven by extreme

decrease in degree attainment (0.7) percentage points

[24]Because 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.

cohorts, in excess of 10 years before the introduction of the merit programs. As indicated in Figure 1, these cohorts comprise a relatively small share of our sample. Our analysis below in Section 5.6 provides additional evidence that the pre-treatment trends are not driving our results.[25]

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. As we have pointed out in Figure 1, these later cohorts tend to be dominated by a select few early adopting states. For this reason, we focus on the treatment effects among cohorts closer to the inception of the program, which are comprised of a more balanced sample of merit aid states.

The patterns in Figure 2 match those described earlier using the traditional difference-in-difference specification. In Panel A, we show that residential out-migration of merit aid eligible cohorts is lower than their ineligible counterparts. The point estimates suggest that cohorts eligible for merit-based scholarships are 1 percentage points more likely to live in their state of birth when they are between the ages of 24 to 32 than their ineligible counterparts. This represents a decrease in out-migration of 2 percent. Our confidence intervals in this graph, however, do not allow us to rule out effects of zero in most years.

In Panel B, the eventual degree attainment of most of the eligible cohorts is below that of the ineligible cohorts.[26] The negative effects in the figure peak at 1.2-percentage point decrease in BA degree attainment. Given BA attainment rates of 25 percent for those aged 24 to 32, this translates into a 5 percent decrease in BA degree receipt. In Panel C, we see that the merit eligible cohorts are no more or less likely to have obtained at

[25] We present results in Appendix B which omit data from 1990. In this case, the visual evidence of any pre-treatment trends is no longer apparent. The results we estimate are also significantly attenuated in this subsample. This may suggest that our patterns are driven 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.

[26] In interpreting these estimates, we focus on the first several cohorts eligible for merit aid because the later treated cohorts are disproportionately concentrated in a relatively small set of early-adopting merit states in our sample.

least some part of a college education before they are aged 24 to 32. Finally, in Panels D and E, the visual evidence regarding the patterns of current college attendance is mixed when estimating individual effects by each cohort. The pattern appears to be relatively flat, although estimates for enrollment in one's home state are slightly more consistent with a positive effect, though not statistically significant. In all cases, however, we fail to detect any significant evidence of a pre-treatment trend.

5.6 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 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 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 7. With the exception of one case, none of the estimates are statistically significant at conventional levels. This evidence further supports the assumptions underlying our difference-in-difference estimation strategy.

5.7 Sensitivity to the Set of Merit States Included

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 state in turn in Table 8. Panels A and B contain the results with the probability of living in-state and BA degree 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 8 reassure us that there is no single state driving the decreases in residential mobility and degree attainment apparent in the full sample.

5.8 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 et al. (2008). In the first case, clusters of cohorts, grouped by state, are resampled with replacement our data. Our specifications are then rerun, generating a Wald statistic. After many draws, our original t-statistic is compared 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 et al. (2008).

Randomization inference involves a permutation method proposed by Bertrand et al. (2002). 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 9 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 (5) in Tables 3 – 6. 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. Even in our most conservative case, randomization inference only renders one of our effects, that of BA attainment, insignificant. These results assure us that our estimates are not overly reliant on a failure to account for a small number of clusters.

5.9 Additional Evidence on College Enrollment Using Postsecondary Institution Data

In previous sections, we showed merit aid programs had a negative effect on BA degree receipt. One hypothesis for this is that increased retention of students at colleges within the state leads to crowding out of resources (e.g. required courses are full), which in turn makes it more difficult for students on the margin of graduating to do so. Unfortunately, the Census data limit our ability to test this hypothesis. In particular, we know more people are living in their state of birth during the ages of 24 to 32 but are not sure whether these individuals attended college in their state of birth. In order to provide suggestive evidence, we now turn to analyses of data from the Integrated Postsecondary Education Data System Residence and Migration Surveys from 1992 to 2008 for corroborating evidence.

Using these data, we compute state-by-year level measures of first-time enrollment at all postsecondary institutions and in-state enrollment in those institutions. We conduct difference-in-difference analyses using log enrollment as the dependent variable and including state and year fixed effects. The coefficient of interest is on a variable measuring the introduction of merit aid by the year of observation and its coefficient can be interpreted as the effects of merit aid programs on enrollment. We also conduct the analyses separately for two-year and four-year institutions of higher education. As before, the standard errors are clustered at the state level. The results are presented in Table A.1 with the sample of states that introduced merit aid programs.

The estimates in Table A.1 suggest that first-time enrollments of institutions may grow faster when merit aid is available, however none of the estimates are statistically significant. The point estimates suggest first-time enrollment increases by 2.7 percent when merit aid programs are available to residents. Enrollment of in-state residents has an even larger point estimate, 5.6 percent, but this estimate is also not statistically significant. These point estimates suggest that merit aid programs are successful at retaining some students in the state for college attendance who otherwise would have gone out of state. Looking separately at four-year versus two-year institutions, we see that increases in overall enrollment and in-state enrollment are concentrated at four-year institutions. Enrollment at two-year institutions contracts when merit aid is made available to residents. Again, however, with only 135 observations, we have limited power and our estimates here are speculative at best. However, the increased retention of students who otherwise would have migrated out of state combined with the likely decrease in college attainment is at least consistent with a crowd out story.

6 Conclusion

We have shown that merit aid programs increase the probability that residents live in their state of birth after they are old enough to have graduated from college. Of particular interest is that the results indicate some residents who obtain their college degrees but would have lived elsewhere are induced by their merit aid eligibility to live in-state. The fact that merit aid programs induce some residents to live in-state who otherwise would have left is suggestive that there is a location specific component of human or social capital. It also may point to a broader effect of going to college in a state on migration, though we cannot say whether the effects of merit aid generalize to other types of in-state college attendance.

This retention of high skilled workers might come at a price, however, as overall rates of degree attainment among residents 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. 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. Perhaps the combination of decreased attainment and increased enrollment of older residents is indicative that the supply of education is inelastic. 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.

More importantly, the magnitude of our results suggest that only a small fraction of the eligible population responds to merit aid by changing educational or migration decisions. We find that programs targeted to at least 30 percent of a cohort alter the behavior of at most 2 percent of a cohort at the margin. Even after adjusting for the fact we obtain intent-to-treat estimates, merit aid programs appear to alter outcomes for less than 3 percent of a cohort.[27] Indeed, we include results in Appendix B that exclude data from 1990. In this case, the merit programs appear to be even weaker, though the estimates in that sample tend to rely more heavily on later-adopting states. In any event, nearly all of the $1.4 billion in spending among our broad-based merit states appears to represent a transfer to inframarginal college-going residents.

Our results present a mixed picture regarding the evaluation of merit-based scholarships. In most cases, the 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 these programs do retain more residents in-state, 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 how the characteristics of the marginal degree recipient have been

[27] The 1 percent of individuals who have altered their migration, the 0.7 percent who have decreased BA attainment and the 0.4 percent delaying graduation comprise at most 2 percent of the population, in the case where they are mutually exclusive sets of individuals. Furthermore, 70% of a cohort born in a given state is predicted to still live in that state at age 18 in our sample. A crude adjustment scales up the 2 percent of the population to $2/0.7 = 2.86$.

affected. Finally, more work is needed to determine whether the increased retention of high skilled workers leads to increased economic growth for the state. Even if it does, further research will also be required to determine how distorting migration between states effects the economic growth of the country as a whole.

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Table 1: Timing of Merit Aid Program Introduction

State	Year Program Introduced	Funding Cap at Public Institutions
Arkansas	1991	\$5,000
Georgia	1993	Full Tuition*
Mississippi	1996	\$500-\$1,000*
Florida	1997	\$1,100-\$1,600
New Mexico	1997	Full Tuition
Louisiana	1998	Full Tuition^{+}
South Carolina	1998	\$5,000
Kentucky	1999	\$100-\$1,000^{+}
Nevada	2000	\$1,440*
Michigan	2000	\$1,000
West Virginia	2002	\$4,750
Maryland	2003†	\$3,000
South Dakota	2004	\$1,000*
Tennessee	2004††	\$4,000
Wyoming	2006	\$1,600-\$3,200^{+}

Note: Only includes merit aid programs where the merit-based scholarship program was lenient enough to include at least 30 percent of high school students (Dynarksi 2004). * 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 one year. ††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.

Table 2: Descriptive Statistics, Census and American Community Survey, 1990 – 2010, Ages 24 to 32

	(1)	(2)	(3)
	All States	Merit & Southern States	Merit States
Female	0.50	0.50	0.50
Black	0.14	0.21	0.22
Hispanic	0.14	0.13	0.08
Current Unemployment	0.06	0.06	0.07
Lagged Unemployment	0.05	0.06	0.06
Living In-State	0.66	0.68	0.66
BA Attainment	0.27	0.23	0.23
Living In-State w/ BA	0.15	0.13	0.12
Currently Enrolled	0.13	0.12	0.12
Currently Enrolled In-State	0.08	0.08	0.07
N	5,508	2,376	1,620

Note: Includes 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data 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 3: Effects of Merit Aid Eligibility on Probability of Living in One's State of Birth, Ages 24 to 32

	(1)	(2)	(3)	(4)	(5)
	All States		Merit & Southern States		Merit States
Panel A. No Age Fixed Effects					
Merit Eligible	0.009	0.004	0.019**	0.015**	0.010**
	(0.008)	(0.007)	(0.008)	(0.007)	(0.004)
Panel B. Age Fixed Effects					
Merit Eligible	0.009	0.004	0.019**	0.014**	0.010**
	(0.008)	(0.007)	(0.008)	(0.007)	(0.004)
N	5,508	5,508	2,376	2,376	1,620
Trend	N	Y	N	Y	N

Note: Includes 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year-level. The dependent variable is the share of a state's residents ages 24 to 32 that are living in their state of birth. Each regression includes state, year and cohort fixed effects. Panel B also includes age 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.

Table 4: Effects of Merit Aid Eligibility on Probability of Living in One's State of Birth with a BA Degree, Ages 24 to 32

	(1)	(2)	(3)	(4)	(5)
	All States		Merit & Southern States		Merit States
Merit Eligible	-0.006*	-0.001	0.002	-0.001	-0.001
	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)
N	5,508	5,508	2,376	2,376	1,620
Trend	N	Y	N	Y	N

Note: Includes 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 a BA degree. 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 Educational Attainment, Ages 24 to 32

	(1)	(2)	(3)	(4)	(5)
	All States		Merit & Southern States		Merit States
Panel A. BA Degree					
Merit Eligible	-0.008**	-0.003	-0.003	-0.007*	-0.007*
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
Panel B. Some College Attendance					
Merit Eligible	0.002	-0.001	0.002	-0.003	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
N	5,508	5,508	2,376	2,376	1,620
Trend	N	Y	N	Y	N

Note: Includes 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 degree or some college. 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 6: Effects of Merit Aid Eligibility on Current College Attendance, Ages 24 to 32

	(1)	(2)	(3)	(4)	(5)
	All States		Merit & Southern States		Merit States
Panel A. Currently Enrolled in College					
Merit Eligible	0.006***	0.002	0.004**	0.002	0.004***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Panel B. Currently Enrolled in College In-State					
Merit Eligible	0.007***	0.003	0.005**	0.004*	0.005***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
N	5,508	5,508	2,376	2,376	1,620
Trend	N	Y	N	Y	N

Note: Includes 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 and are currently enrolled in school 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 7: Placebo Treatment Effects

	(1)	(2)	(3)	(4)
	Living In-State	BA Attainment	Currently Enrolled in College	Currently Enrolled in College In-State
Panel A. Placebo 3 Years Before Treatment				
Merit Eligible	0.006**	-0.002	0.004	0.003
	(0.002)	(0.004)	(0.004)	(0.003)
$N = 569$				
Panel B. Placebo 4 Years Before Treatment				
Merit Eligible	0.002	-0.003	0.000	-0.000
	(0.003)	(0.003)	(0.003)	(0.002)
$N = 745$				
Panel C. Placebo 5 Years Before Treatment				
Merit Eligible	-0.005	0.002	-0.000	-0.000
	(0.004)	(0.003)	(0.004)	(0.002)
$N = 892$				
Panel D. Placebo 6 Years Before Treatment				
Merit Eligible	0.001	-0.001	0.002	0.002
	(0.005)	(0.003)	(0.003)	(0.002)
$N = 949$				

Note: Based on the authors' calculations using the 1990 and 200 Decennial Census and 2001-2010 ACS 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 is 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 (5) of Tables 3 – 6. 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 8: Sensitivity Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Omitted State	Arkansas	Florida	Georgia	Kentucky	Louisiana	Maryland	Michigan
Panel A. Living In-State							
Merit Eligible	0.007*	0.007*	0.012*	0.011**	0.012**	0.010**	0.011**
	(0.004)	(0.003)	(0.006)	(0.004)	(0.005)	(0.004)	(0.005)
Panel B. BA Attainment							
Merit Eligible	-0.007*	-0.007	-0.007	-0.007*	-0.008**	-0.004	-0.008*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
N = 1,512							

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Omitted State	Mississippi	Nevada	New Mexico	S. Carolina	S. Dakota	Tennessee	W. Virgina
Panel A. Living In-State							
Merit Eligible	0.009*	0.009**	0.009*	0.010**	0.010**	0.009**	0.010**
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Panel B. BA Attainment							
Merit Eligible	-0.007*	-0.007*	-0.006	-0.007	-0.007*	-0.008	-0.005
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
N = 1,512							

Note: Based on the authors' calculations using the 1990 and 200 Decennial Census and 2001-2010 ACS at the state-cohort-year level. The sample includes all of the merit aid states other than the state indicated in the column header. 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.

Table 9: Robustness Check: Intra-Cluster Correlation of Errors

	(1)	(2)	(3)	(4)	(5)	(6)
	Living In-State	BA Attainment	Some College	Living In-State w/ BA	Currently Enrolled in College	Currently Enrolled in College In-State
Merit Eligible	0.01	-0.007	-0.001	-0.001	0.004	0.005
p-values:						
Clustered SE	0.018**	0.064*	0.659	0.521	0.006***	0.002***
Nonparametric Bootstrap-t	0.044**	0.036**	0.868	0.630	0.008***	0.006***
Wild Bootstrap-t	0.050*	0.079*	0.900	0.744	0.008***	0.018**
Randomization Inference	0.078*	0.180	0.932	0.754	0.014**	0.016**

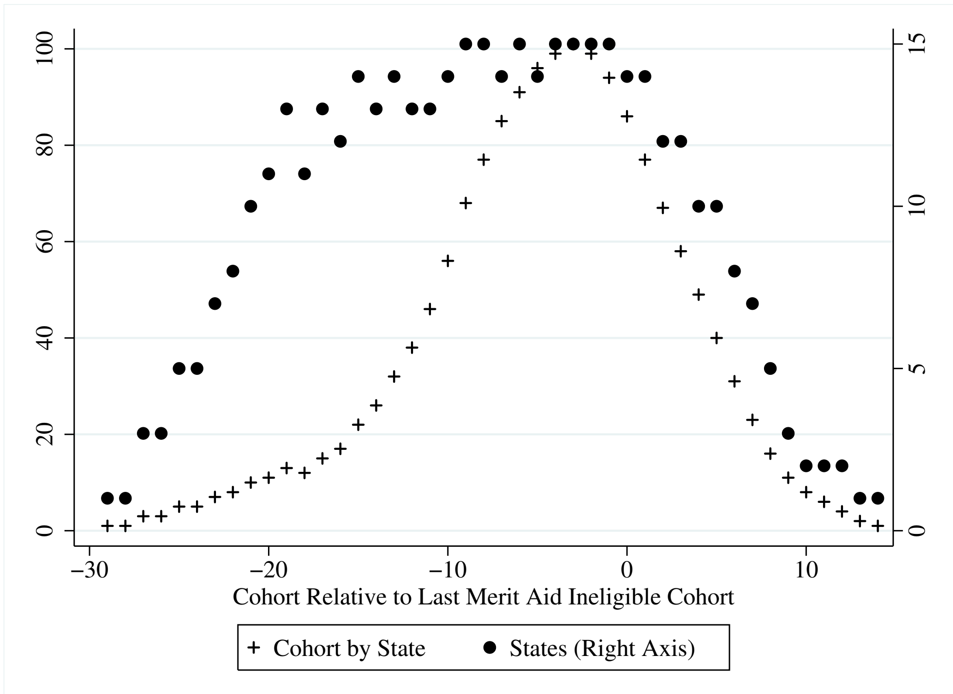
Note: Based on the authors' calculations using the 1990 and 200 Decennial Census and 2001-2010 ACS at the state-cohort-year level. Coefficients correspond to original treatment effects estimated column (5) Tables 3 – 6. Each regression includes state, year, cohort and age fixed effects. Regressions are 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 retained from the Tables above. The Nonparametric Bootstrap-t 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

Table 10: Effects of Merit Aid Program Introduction on First-Time Enrollments in Postsecondary Institutions, 1992-1998

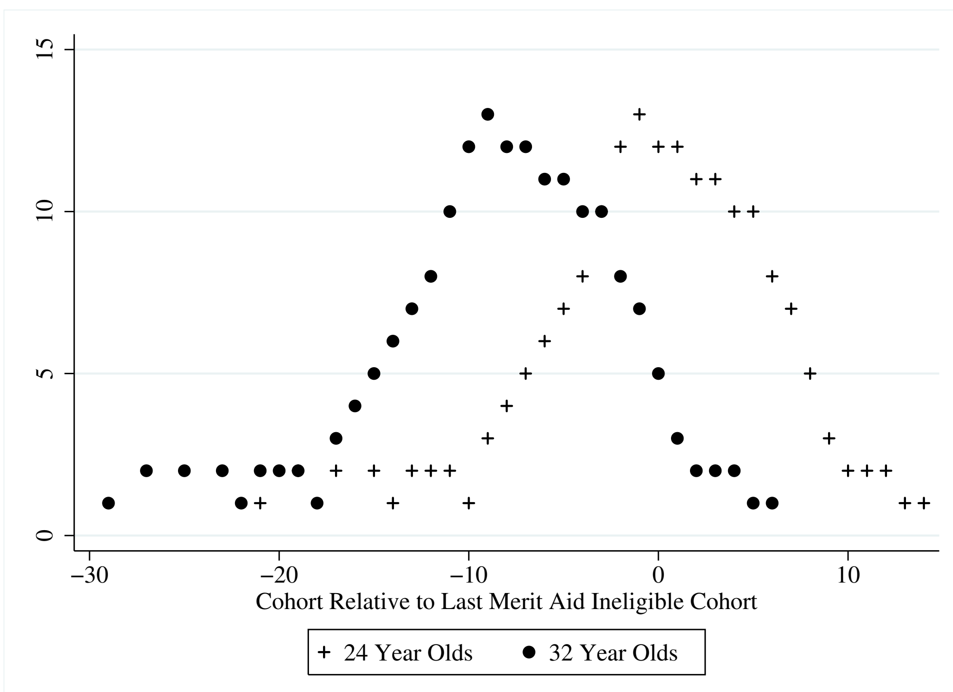
	(1)	(2)	(3)
	All Institutions	Four–Year Institutions	Two–Year Institutions
Outcome: Log Enrollment of First Time First Year Students			
Merit	0.027	0.058	-0.097
	(0.027)	(0.065)	(0.172)
Outcome: Log Enrollment of In-State First Time First Year Students			
Merit	0.056	0.062	-0.049
	(0.040)	(0.083)	(0.178)
N	135	135	135

Note: Based on the authors' calculations using the Integrated Postsecondary Education Data System, Residence and Migration of First-Time Students, 1992 to 2008. The sample includes only states that introduce merit aid programs. Specification is the difference-in-difference specification outlined in the text with state-year-level data. Dependent variables are the log of enrollment of first-time students as indicated by the row headers. Standard errors are clustered at the state level.

Figure 1: Number of Cells in the Data, by Cohort Relative to Last Merit Aid Eligible Cohort

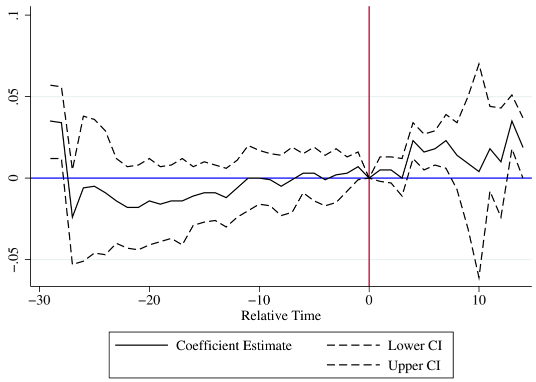


(a) Number of States and Number of Age by State Cells

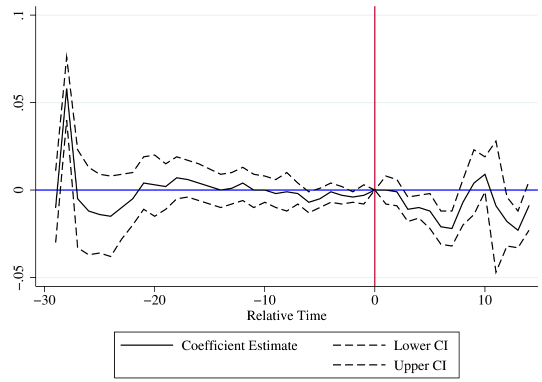


(b) Number of States in Each Cohort Relative to the Last Merit Aid Ineligible Cohort

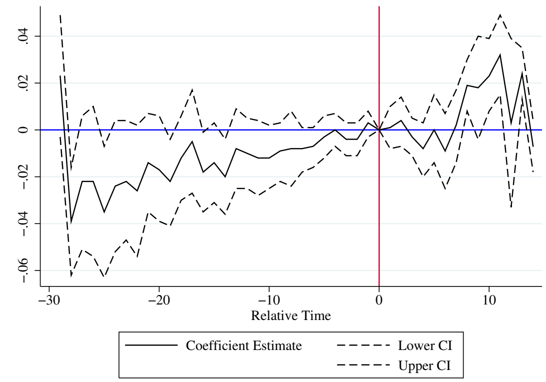
Figure 2: Event Study Figures



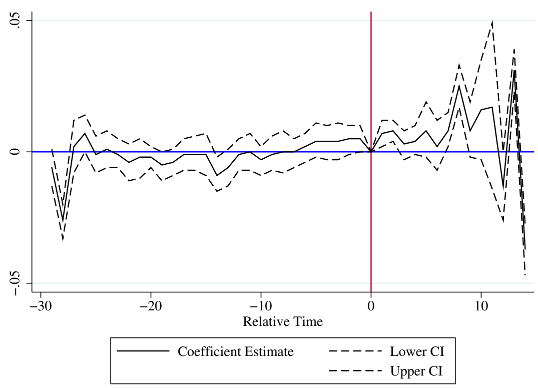
(a) Living In-State



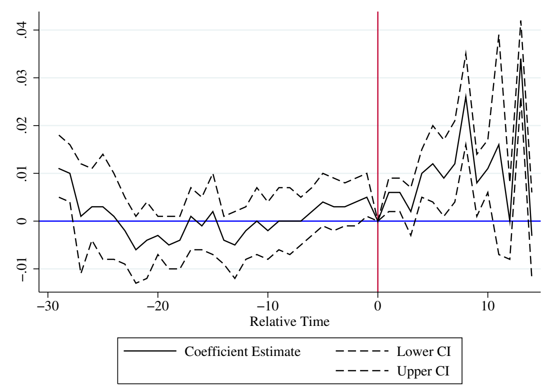
(b) BA Attainment



(c) Some College Attendance



(d) Currently Enrolled



(e) Currently Enrolled In-State

Note: Based on the authors' calculations using the 1990 and 2000 Decennial Censuses and the 2001-2010 ACS. 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

A Additional Results

Table A.1: Comparison of Results for BA Attainment Across Samples Based on Years Set of Controls Included, Ages 22 to 34

	(1)	(2)	(3)	(4)
Arkansas and Georgia Only	0.008*	0.002	0.000	-0.004
	(0.004)	(0.006)	(0.003)	(0.005)
All Merit Aid States			0.000	-0.004
			(0.004)	(0.005)
Years	2000	1990-2000	2000-2010	1990-2010
Age FE	Y	N	N	N
State FE	Y	Y	Y	Y
Cohort FE	N	Y	Y	Y
Year FE	N	Y	Y	Y

Note: Based on the authors' calculations using the 1990 and 2000 Decennial Censuses and 2001-2010 ACS, as indicated, at the state-cohort-year level. The dependent variable is the share of a cohort born in a state and aged 22 to 34 at the time of the survey who have obtained a BA degree. Each regression includes state, year, cohort and age fixed effects as indicated. 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. Note that these data do not include observations with imputed information on birthplace, place of residence, age or degree attainment, while the data elsewhere in the paper do contain observations with imputed data.

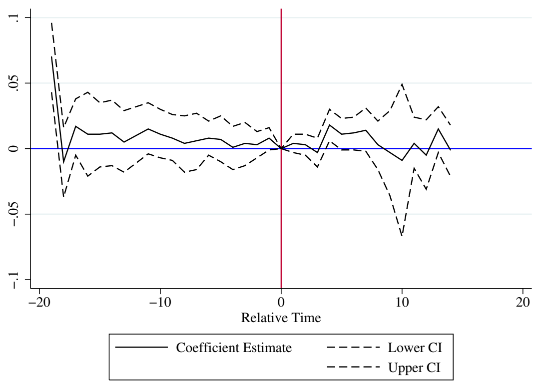
B Omitting the 1990 Census

Table B.2: Effects of Merit Aid Eligibility, Ages 24 to 32, Omitting 1990 Census

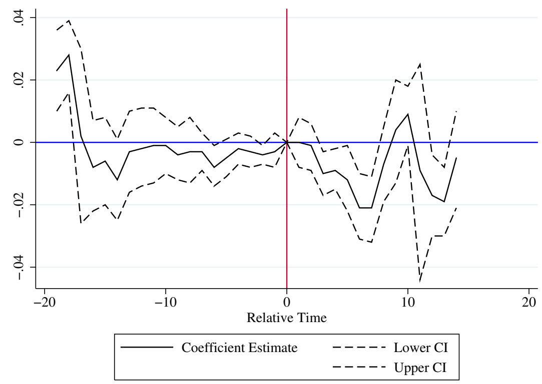
	(1)	(2)	(3)	(4)	(5)
Outcome	All States		Merit & Southern States		Merit States
Living In-State	0.001	0.005	0.011	0.007	0.004
	(0.007)	(0.005)	(0.007)	(0.005)	(0.005)
Living In-State w/ BA	-0.005*	-0.002	0.000	-0.001	-0.002
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
BA Degree	-0.004	-0.003	-0.002	-0.004	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Some College Attendance	-0.001	-0.002	-0.003	-0.003	-0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Currently Enrolled in College	0.003	0.001	0.001	0.000	0.001
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Currently Enrolled in College In-State	0.003	0.003	0.001	0.003*	0.003*
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
N	5,049	5,049	2,178	2,178	1,485
Trend	N	Y	N	Y	N

Note: Includes 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.

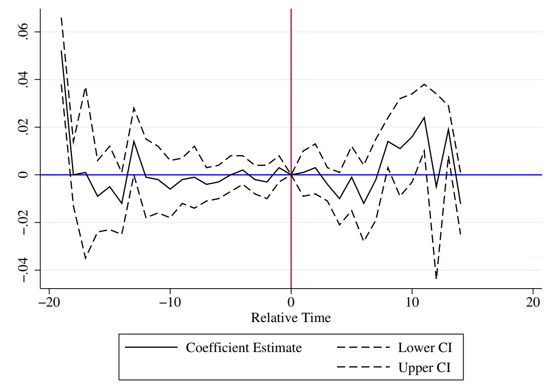
Figure B.1: Event Study Figures, Omitting 1990 Census



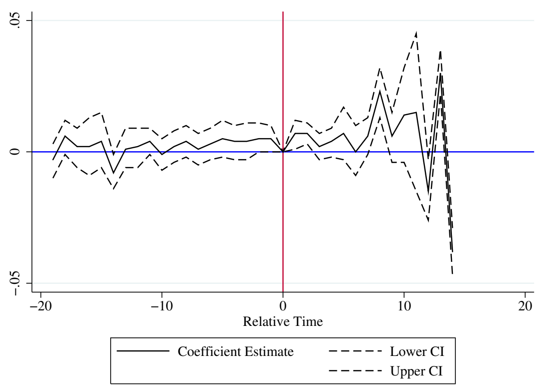
(a) Living In-State



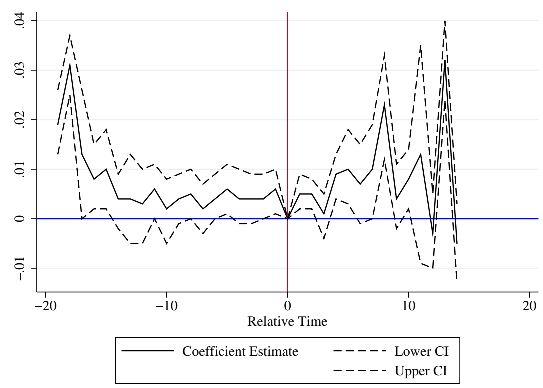
(b) BA Attainment



(c) Some College Attendance



(d) Currently Enrolled



(e) Currently Enrolled In-State

Note: Based on the authors' calculations using the 1990 and 2000 Decennial Censuses and the 2001-2010 ACS. 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.