NBER WORKING PAPER SERIES

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

Maria D. Fitzpatrick
Damon Jones

Working Paper 18530
http://www.nber.org/papers/w18530

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
November 2012

We would like to thank the Population Research Center at the University of Chicago (grant # R24 HD051152-05 from the National Institute of Child Health and Human Development) for their support. Rebecca Hinze-Pifer provided excellent research assistance. We are extremely grateful to Jeff Groen, Jeffrey Grogger, Kirabo Jackson, Michael Lovenheim, James Sallee, Sarah Turner and to participants at Cornell University, the University of Chicago, the New York Federal Reserve Board, the 2012 CESIfo Area Conference on the Economics of Education and 2012 SIEPR Young Scholars Conference at Stanford University for helpful comments and discussion. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Higher Education, Merit-Based Scholarships and Post-Baccalaureate Migration
Maria D. Fitzpatrick and Damon Jones
NBER Working Paper No. 18530
November 2012, Revised May 2013
JEL No. H7,I2,R23

ABSTRACT

We present new evidence on the effects of merit aid scholarship programs on residential migration and educational attainment using Census data on 24 to 32 year olds in the U.S. from 1990 to 2010. 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. 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.

Maria D. Fitzpatrick
Department of Policy and Management
Cornell University
103 Martha Van Rensselaer Hall
Ithaca, NY 14853
and 4853
and also NBER
maria.d.fitzpatrick@cornell.edu

Damon Jones
Harris School of Public Policy
University of Chicago
1155 East 60th Street
Chicago, IL 60637
and NBER
damonjones@uchicago.edu

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 at 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, Mustard and Sridhar, 2006; Orsuwan and Heck, 2008; Zhang and Ness, 2010). Since the time of the 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 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 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 merit-based scholarships before and after the programs were

[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 of Dynarski (2000, 2004).

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, Mustard and Sridhar, 2006) or if tuition and fees increase with merit scholarship introduction (Long, 2004), then enrollment increases may be offset, particularly of low-income college attendees. That is, 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 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, 2000; Cornwell, Mustard and Sridhar, 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 in part on 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 (Cohodes and Goodman, 2012) 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, may 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, 2012*a*; Cohodes and Goodman, 2012). Most of these studies have focused on one or two states, making it difficult to know whether the results generalize to other settings.[3]

Another important 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.[4] Finally, educational attainment, regardless of location, has been shown to directly increase mobility (Malamud and Wozniak, 2012). Thus, the ambiguous effects of merit aid on attainment discussed above likewise renders the net effect of merit aid on migration ambiguous. In two recent exceptions, Sjoquist and Winters (2012*c*) find little effect of the Georgia HOPE scholarship on migration using administrative data on enrollment and employment in Georgia, while Hickman (2009) finds a significant positive effect of merit aid on the retention of college graduates in Florida as a result of the Florida's Bright Futures Scholarship Program. [5] In both cases, the analysis is limited to the context

[3]One recent exception is a contemporaneous working paper by Sjoquist and Winters (2012*b*) who look at educational attainment using census data in a fashion similar to our current research design. They similarly find no positive effect of merit aid programs on college completion.

[4]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. Also, see Groen (2011) for a review of how this evidence might speak to the effect of merit aid on migration.

[5]In Sjoquist and Winters (2012*c*) 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. In Hickman (2009), the comparison is of migration patterns in the Census and ACS among Florida-born college graduates before and after merit aid introduction.

of a particular state. To the extent that the Georgia or Florida programs differ from other broad-based merit aid programs, these results may not generalize.

Our contribution to this literature is threefold. First, the long horizon over which some of these programs have been operating allows us to focus our analysis on a larger set of life-cycle 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 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 drive our difference-in-difference results.

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 ever attended college or to have received a bachelor's degree. In fact, our point estimates for BA attainment are negative, though not consistently significant. 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 degree. 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, aged 24 to 32 year olds.

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 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; 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 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 (≤ 3 percent) is of an order of magnitude smaller than the target population (≥ 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 and additional robustness checks in Section 6. Finally, we conclude in Section 7 with a synthesis of the programs' overall effects and a discussion of their economic significance.

2 Merit Aid Programs

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 is unable to meet them, they are unlikely to change educational choices. 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 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 students (Dynarski, 2004).[6] This leads us to focus on the 15 states 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.[7]

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 affect the residential migration, college enrollment, or college completion of eligible residents, we estimate the following:

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

[6]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 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, 2012*c*). In Appendix 6.5, we examine the pattern of results using just these early adopters and find that the patterns described here hold.

[7]These 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

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, current enrollment in college, and combinations of the 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 year she would most likely have entered college.

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.[8] 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, Gelbach and Miller, 2008) as well as randomization inference (Bertrand, Duflo and Mullainathan, 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 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.

[8] 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.

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 also conduct placebo tests using only pre-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).[9] Finally, we include time-varying demographic controls to mitigate concerns about other possible changes occurring simultaneously with program introduction.

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, there would be an upward bias the effect of merit aid on college attendance. 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, a point we return to in the conclusion.

[9] Since 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 zero.

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 (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 a 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, which ensures that at least three treatment states are contributed to estimates for all age groups.[10]

In each of the surveys, the Census Bureau collects information on place of residence, place of birth, current enrollment in school and educational attainment, which we use to create our three sets of dependent variables of interest.[11] Specifically, we are interested in i) residential migration, ii) degree completion, and iii) current college attendance. First, we investigate whether merit aid eligibility makes it more likely for residents to remain in the state in which they were born and whether high skilled people (those with who have completed at least some college attendance and those with BAs) 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 college degree (BA) or has at least some college attendance, regardless of their place of residence.[12] In addition, we examine both whether individuals

[10]In 2010, 32 year olds in Arkanas, 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 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 D.

[11]In 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 C.

[12]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 2003 ACS questionnaire included a rewording of the measurement of Associate Degree attainment

have ever attended any college and whether they are enrolled in college at the time of the survey. That is, we measure college enrollment during the ages of 24 to 32 in any state and, furthermore, in the state of birth. We define college enrollment as enrollment at any level, including both undergraduate and graduate enrollment.[13]

The Decennial Census and ACS also include gender and race information that we use to control for demographic differences across observations. 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, 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.[14]

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 indicates that 15 states in our sample 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

in way that precludes consistent comparisons across time.

[13] Our estimates of the effects of the program on enrollment therefore include any effects on either undergraduate or graduate enrollment. However, in results not reported here, we have separately estimated the effects of merit aid on graduate enrollment and they were zero. So, we attribute any merit aid effect on enrollment to its effect on undergraduate enrollment. Results are available from the authors upon request.

[14] 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.

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, separately for ages 24 and 32. 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. It is for this reason that we restrict analysis to 24 to 32 year olds in order to minimize the problem of too few treatment clusters (Conley and Taber, 2011).[15]

In Table 2, we present descriptive statistics that summarize the characteristics of our state-cohort-year cells.[16] 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 degree is between 23 and 27 percent. Of these, about half, or 12 to 15 percent of individuals have a BA degree and are living in their state of birth. Meanwhile, current college enrollment 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

[15] Results using a wider range of ages are available in Appendix D,

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

probability that people ages 24 to 32 live 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.[17] 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.

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.[18] 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.[19] The estimate is similar in magnitude to that in the first column, though it is

[17]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.

[18]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.

[19]In these specifications we do not estimate a model with a trend because with only merit states in the

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.[20] 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 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.

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 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 or at least some college attendance. These results are presented in Table 4, which has a similar structure to Table 3. In Panel A, the dependent variable represents the outcome of both living in one's state of birth and having a BA degree; in Panel B, it corresponds to living in one's state of birth and having attended at least some college.

Looking across the columns of Panel A of Table 4, 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 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 and the standard error suggests the estimate is between -0.3 and 0.1 percentage points.

sample the trend is not separately identified from time fixed effects.

[20]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 five models discussed here; generally, they produce a similar pattern of results.

Overall, the results provide little evidence of an effect on the retention of BA degree holders 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 at least some college by ages 24 to 32. In column (5), the estimate suggests that the probability increased by 0.7 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 2 percent increase.

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 it did little to change the probability of both living in the state and having a BA degree. 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 5 where the dependent variable of interest is attainment of at least a BA degree. 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 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 3 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 home state.[21] Unfortunately, we cannot determine with this data whom the marginal degree recipient is nor where the marginal degree is earned.[22] The estimates 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. 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, 2012) or general features of attending an in-state school, e.g. distractions stemming from a closer proximity to one's home.

To further understand a potential decrease in BA degree attainment, we present results in Panel B of Table 5 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. To the extent that BA attainment decreases, the reduction appears to occur somewhere between enrollment and graduation.[23]

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 graduation, particularly at in-state postsecondary institutions (Dynarski, 2004; Zhang and

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

[22]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.

[23]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 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.

Ness, 2010; Dynarski, 2008). 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 have no bearing on previous results regarding the location of enrollment. In fact, our finding of no change in having attended some college can be reconciled with finding increased in-state enrollment if the new in-state enrollment comes from students who would have attended college elsewhere.[24] Similarly, previous work has found increased enrollment in college in any location of merit-eligible 18 and 19 year olds (Dynarski, 2004). This finding would be consistent with ours if merit aid 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 are 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 the Dynarski (2008), who used the one-percent samples from the 1990 and 2000 Decennial 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 found that merit aid eligibility increased the probability of 22 to 34 year olds obtaining at least a Bachelor's degree by 2.5 percentage points (and at least an Associate's by 3 percentage points). In a replication exercise, Sjoquist and Winters (2012*a*) 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 make it possible to extend the previous analyses to additional merit aid states and to additional treated cohorts in Arkansas and Georgia. In Appendix Table A.1, we replicate this previous work in Columns 1 - 3, and show in Column 4 that when 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

[24]Indeed 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.

with that presented in our Table 5. We show in Columns 5 and 6 that the additional differences in our approach (e.g. expanding the sample to 2010, using new merit 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.[25]

5.4 College Attendance

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.[26] However, available data do not allow us to determine whether the people who are no longer holding degrees and those who are now enrolled at these ages are in fact one in the same.

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

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

6 Robustness Checks

6.1 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:

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

In equation (2), we include a set of new dummy variables, D^k_{sct}. 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 (2) we include a dummy variable for each of the possible cohorts before and 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.[27]

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 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 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. As indicated in Figure 1, these cohorts

[27]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.

comprise a relatively small share of our sample. Our analysis below in Section 6.2 provides additional evidence that the pre-treatment trends are not driving our results.[28]

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 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 2 reinforce those described earlier using the traditional difference-in-difference specification.

6.2 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 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. 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.[29]

[28]We present results in Appendix B 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 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.

[29]Note that even in cases where 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.

6.3 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.

6.4 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 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).[30] 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. In our most conservative case, randomization inference renders two of our effects, that of BA attainment and living in-state with some college attendance, insignificant. These results generally indicate that our estimates are not overly reliant on a failure to account for a small number of clusters.

6.5 Heterogeneous and Continuous Treatment Effects

In Table 10 we consider alternative specifications that explore differences in treatment effects by program type. We also present results that incorporate the variation in tuition generosity across merit aid programs. All of the samples include only merit aid states and the specifications correspond to column (5) of Tables 3 to 6. Columns (1) and (2) report coefficients from a regression with treatment indicator for merit aid eligibility and an interaction between this indicator and only allowing 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

[30]Note, this method is described in the working paper version of this paper, but not in the published version.

appear to be associated with a higher level of retention, but a lower likelihood of both living in-state and having attained a BA degree. With respect to general educational attainment, these states appear to experience a larger decrease in BA attainment and college attendance than other merit states. Finally, these programs do not appear to have any significantly different effects on current college enrollment.

Columns (3) and (4) report coefficients from a regression with a treatment indicator for merit aid eligibility and an interaction between this indicator and being an early adopting state, i.e. Georgia or Arkansas. In general, we do not find any significant differences in outcomes between the early adopting and later adopting states. Finally, in column (5), we report an alternative specification where instead of an indicator for merit aid eligibility, we regress outcomes on the fraction of the average tuition and fees at four-year public insitutions in the state that is covered by the merit aid scholarship. This fraction ranges from 0 for ineligible cohorts to 0.13 in Michigan to 1.11 in Arkansas. The coefficient should be 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 that only use merit aid eligibility as the treatment variable.

7 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 degree, however, does not appear to increase. Thus, merit aid programs have the potential to retain a 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. Although the statistical significance of this result is not consistent, an overall decrease in BA attainment combined with the aforementioned constant share of a cohort living in-state with a BA suggest 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.

There are at least three interpretations of the possible reduction or delay in BA degree 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 the 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. Alternatively, the marginal migrants and degree recipients may be one-in-the-same. That is, merit aid may cause price-sensitive students to remain in-state, resulting in mismatches in institution or location. Such a mismatch may be due to differences in quality (Cohodes and Goodman, 2012) or general location-specific factors. Note, these explanations require in-state enrollment during collage-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, those non-migrants now crowded out of a BA degree may experience a reduction in mobility, which would likewise increase the share of state-born 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. The case where the marginal out-migrant remains in-state to take advantage of merit aid but in the process endures a mismatch might also generate a reduction in out-migration. In this case, the mis-matched student may experience a reduction in mobility due to less educational attainment. Similarly, the case where would-be 2-year students substitute to 4-year institutions prematurely and as a result do not graduate may likewise generate a reduction in mobility for this group and likewise requires the retention of out-migrants with BAs to hold constant the share of the cohort both living in-state and having a BA.[31]

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. 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.[32] Indeed, when we exclude data from 1990 (see Appendix B), the merit program effects appear to be even smaller, 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

[31] Technically, 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.

[32] 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 2.1 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 2.1 percent of the population to $2.1/0.7 = 3$.

residents.

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 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 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 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 affects economic growth at a national level.

References

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2002. "How Much Should We Trust Differences-In-Differences Estimates?" NBER Working Paper No. 8841.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-In-Differences Estimates?" *The Quarterly Journal of Economics*, 119(1): 249–275.

Bound, John, and Sarah Turner. 2007. "Cohort crowding: How resources affect collegiate attainment." *Journal of Public Economics*, 91(5–6): 877–899.

Cameron, A Colin, Jonah B Gelbach, and Douglas L Miller. 2008. "Bootstrap-Based Improvements for Inference with Clustered Errors." *The Review of Economics and Statistics*, 90(3): 414–427.

Cohodes, Sarah, and Joshua Goodman. 2012. "First Degree Earns: The Impact of College Quality on College Completion Rates." *Harvard Kennedy School Faculty Research Working Paper Series.*

Conley, Timothy G, and Christopher R Taber. 2011. "Inference with 'Difference in Differences' with a Small Number of Policy Changes." *The Review of Economics and Statistics*, 93(1): 113–125.

Cornwell, Christopher, David B Mustard, and Deepa J Sridhar. 2006. "The Enrollment Effects of MeritBased Financial Aid: Evidence from Georgia's HOPE Program." *Journal of Labor Economics*, 24(4): 761–786.

Dynarski, Susan. 2000. "Hope for Whom? Financial Aid for the Middle Class and Its Impact on College Attendance." *National Tax Journal*, LIII(3): 629–662.

Dynarski, Susan. 2004. "The new merit aid." In *College choices: The economics of where to go.* , ed. Caroline M Hoxby. University of Chicago Press.

Dynarski, Susan. 2008. "Building the Stock of College-Educated Labor." *Journal of Human Resources*, 43(3): 576–610.

Fitzpatrick, Maria D., and Sarah Turner. 2007. "Blurring the Boundary: Changes in the Transition from College Participation to Adulthood." In *The Price of Independence: The Economics of Early Adulthood.* , ed. Sheldon Danziger and Cecilia Rouse. New York:Russell Sage Foundation.

Goodman, Joshua. 2008. "Who merits financial aid?: Massachusetts' Adams Scholarship." *Journal of Public Economics*, 92(10–11): 2121–2131.

Groen, Jeffrey A. 2004. "The effect of college location on migration of college-educated labor." *Higher education (Annals issue)*, 121(1–2): 125–142.

Groen, Jeffrey A. 2011. "Building Knowledge Stocks Locally: Consequences of Geographic Mobility for the Effectiveness of State Higher Education Policies." *Economic Development Quarterly*, 25(4): 316–329.

Hickman, Daniel C. 2009. "The effects of higher education policy on the location decision of individuals: Evidence from Florida's Bright Futures Scholarship Program." *Regional Science and Urban Economics*, 39(5): 553–562.

Long, Bridget Terry. 2004. "How do Financial Aid Policies Affect Colleges?" *Journal of Human Resources*, 39(4): 1045–1066.

Malamud, Ofer, and Abigail Wozniak. 2012. "The Impact of College on Migration: Evidence from the Vietnam Generation." *Journal of Human Resources*, 47(4): 913–950.

NASSGAP. 2011. "42nd Annual Survey Report on State-Sponsored Financial Aid." *National Association of State Student Grant Aid Programs*.

Orsuwan, Meechai, and Ronald H Heck. 2008. "Merit-Based Student Aid and Freshman Interstate College Migration: Testing a Dynamic Model of Policy Change." *Research in Higher Education*, 50(1): 24–51.

Ruggles, Steven, J Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B Schroeder, and Matthew Sobek. 2010. "Integrated Public Use Microdata Series: Version 5.0 [Machine-readable database]." *Minneapolis: University of Minnesota*.

Scott-Clayton, Judith. 2011. "On Money and Motivation A Quasi-Experimental Analysis of Financial Incentives for College Achievement." *Journal of Human Resources*, 46(3): 614–646.

Sjoquist, David L, and John V Winters. 2012*a*. "Building the Stock of College-Educated Labor Revisited." *Journal of Human Resources*, 47(1): 270–285.

Sjoquist, David L, and John V Winters. 2012*b*. "State Merit-based Financial Aid Programs and College Attainment." *IZA Discussion Paper No. 6801*.

Sjoquist, David L, and John V Winters. 2012*c*. "The Effects of HOPE on Post-Schooling Retention in the Georgia Workforce." Mimeo.

Wolfers, Justin. 2006. "Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results." *American Economic Review*, 96(5): 1802–1820.

Zhang, Liang, and Erik C Ness. 2010. "Does state merit-based aid stem brain drain?" *Educational Evaluation and Policy Analysis*, 32(2): 143–165.

Table 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*	Some	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 the merit-based scholarship program was lenient enough to include at least 30 percent of high school students (Dynarksi 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 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: 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 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: 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 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. 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 Probability of Living in One's State of Birth and Attending or Graduating from College, Ages 24 to 32

	(1)	(2)	(3)	(4)	(5)
	All States		Merit & Southern States		Merit States
Panel A. BA Degree Attainment					
Merit Eligible	-0.006*	-0.001	0.002	-0.001	-0.001
	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)
Panel B. Attended at Least Some College					
Merit Eligible	0.012**	0.007**	0.007**	0.007**	0.007**
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
N	5,508	5,508	2,376	2,376	1,620
Trend	N	Y	N	Y	N

Note: Estimates use1990 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 5: Effects of Merit Aid Eligibility on Educational Attainment, Regardless of Current Residence, Ages 24 to 32

	(1)	(2)	(3)	(4)	(5)
	All States		Merit & Southern States		Merit States
Panel A. BA Degree Attainment					
Merit Eligible	-0.008**	-0.003	-0.003	-0.007*	-0.007*
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
Panel B. Attended at Least Some College					
Merit Eligible	0.002	-0.001	0.002	-0.003	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Panel B. Graduated from High School					
Merit Eligible	0.006*	-0.007*	0.003	-0.004	-0.003
	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)
N	5,508	5,508	2,376	2,376	1,620
Trend	N	Y	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 degree, some college or 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 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: 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 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)	(5)
	Living In-State	Living In-state and Some College	BA Attainment	Currently Enrolled in College	Currently Enrolled in College In-State
Panel A. Placebo 3 Years Before Treatment					
Merit Eligible	0.003	0.003	-0.002	0.003	0.002
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
$N = 569$					
Panel B. Placebo 4 Years Before Treatment					
Merit Eligible	0.000	-0.010***	-0.006*	0.001	-0.000
	(0.004)	(0.002)	(0.003)	(0.003)	(0.002)
$N = 745$					
Panel C. Placebo 5 Years Before Treatment					
Merit Eligible	-0.005	-0.002	0.001	0.001	0.001
	(0.005)	(0.003)	(0.004)	(0.004)	(0.003)
$N = 892$					
Panel D. Placebo 6 Years Before Treatment					
Merit Eligible	0.001	0.000	-0.001	0.002	0.002
	(0.005)	(0.004)	(0.003)	(0.003)	(0.003)
$N = 949$					

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 (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: 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 5 of Tables 3 and 5. 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)	(7)
	Living In-State	BA Attainment	Some College	Living In-State w/ Some Col.	Living In-State w/ BA	Currently Enrolled in College	Currently Enrolled in College In-State
Merit Eligible	0.01	-0.007	-0.001	0.007	-0.001	0.004	0.005
p-values:							
Clustered SE	0.018**	0.064*	0.659	0.017**	0.521	0.006***	0.002***
Bootstrap-t	0.044**	0.036**	0.868	0.028**	0.630	0.008***	0.006***
Wild Bootstrap-t	0.050*	0.079*	0.900	0.035**	0.744	0.008***	0.018**
Rand. Inference	0.078*	0.180	0.932	0.110	0.754	0.014**	0.016**

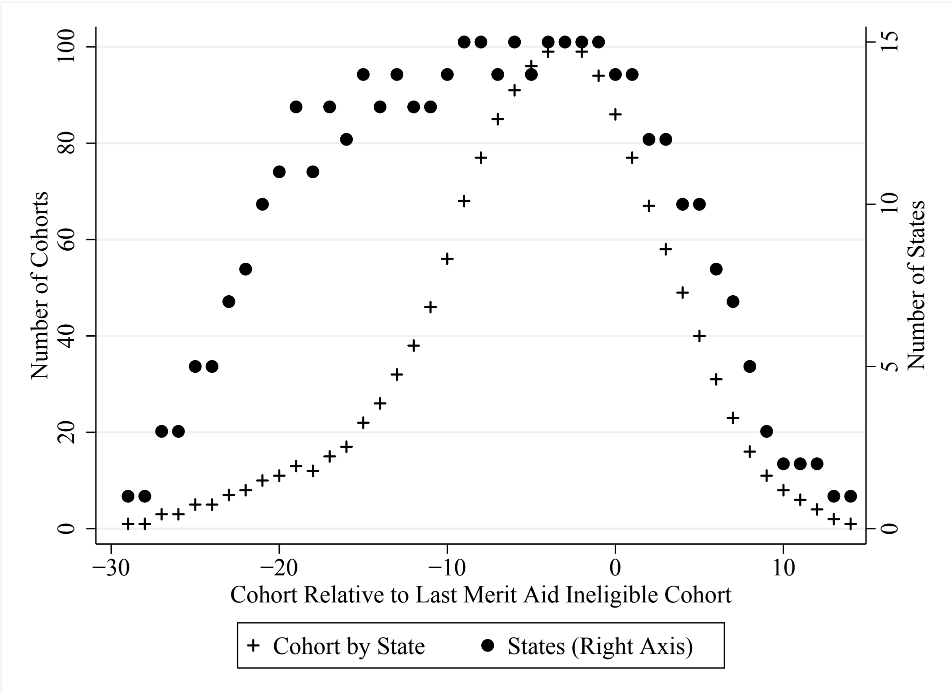
Note: Estimates use 1990 Decennial, 2000 Decennial and 2001-2010 ACS survey data at the state-cohort-year level. Coefficients in first row correspond to original treatment effects estimated column (5) Tables 3 – 6. Each regression includes state, year, cohort and age fixed effects. Regressions are weighted 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 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 (2004). ***, ** and * indicate statistically significant coefficients at the one, five and ten percent levels respective

Table 10: Effects of Merit Aid, Ages 24 to 32

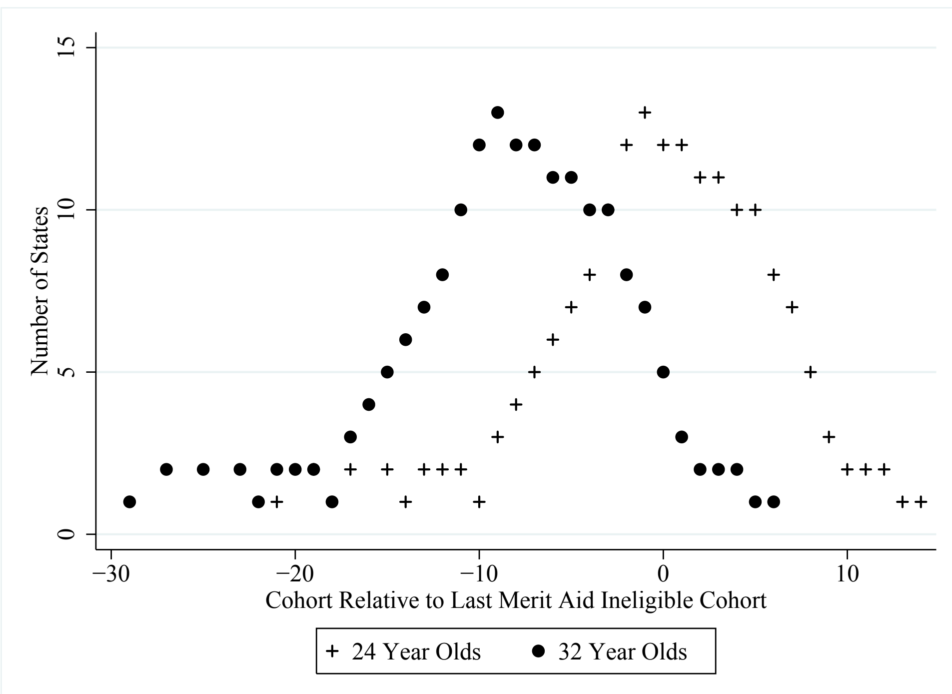
	(1)	(2)	(3)	(4)	(5)
Outcome	Merit Aid	Merit Aid x Public Only	Merit Aid	Merit Aid x AR & GA	Fraction of Tuition Covered
Living In-State	0.009*	0.024**	0.009	0.001	0.006
	(0.004)	(0.009)	(0.007)	(0.014)	(0.007)
Living In-State w/ BA	0.000	-0.015**	-0.002	0.001	-0.001
	(0.002)	(0.005)	(0.003)	(0.002)	(0.003)
Living In-State w/ Some College Attendance	0.007**	-0.008	0.004	0.007	0.008*
	(0.003)	(0.008)	(0.004)	(0.006)	(0.004)
BA Degree	-0.006	-0.019**	-0.007	0.001	-0.007*
	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)
Some College Attendance	0.001	-0.032***	-0.004	0.007	0.002
	(0.004)	(0.007)	(0.006)	(0.006)	(0.005)
Currently Enrolled in College	0.004**	-0.012	0.002	0.004	0.002
	(0.001)	(0.005)	(0.002)	(0.003)	(0.002)
Currently Enrolled in College In-State	0.006**	-0.009	0.003	0.004	0.003
	(0.002)	(0.007)	(0.002)	(0.003)	(0.003)
$N = 1,485$					

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 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 states allowing enrollment only at public institutions of higher education. Columns (3) and (4) present results from a specification with a general merit indicator and an indicator for just Arkansas and Georgia. 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. Each regression includes state, year, cohort and age fixed effects and 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 1: Number of Cells in the Data, by Cohort Relative to Last Merit Aid Eligible Cohort

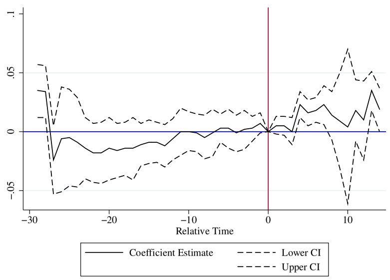


(a) Number of States and Number of Cohort by State Cells

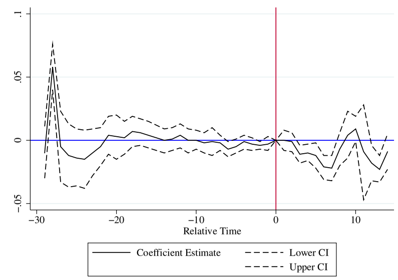


(b) Number of States in Each Cohort Relative to the Last Merit Aid Ineligible Cohort

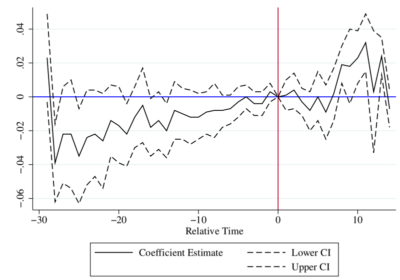
Figure 2: Event Study Figures



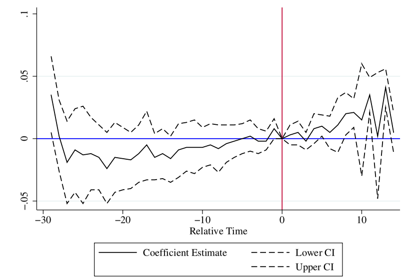
(a) Living In-State



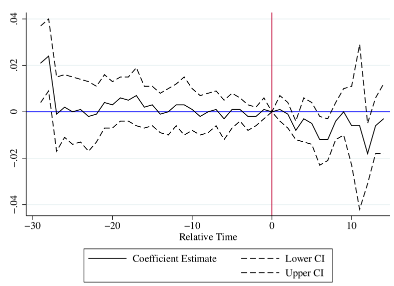
(b) BA Attainment



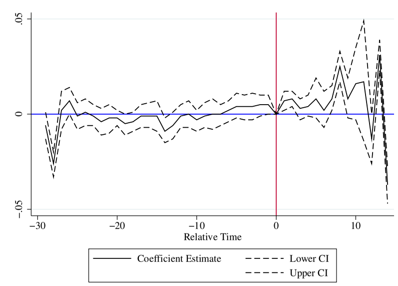
(c) Some College Attendance



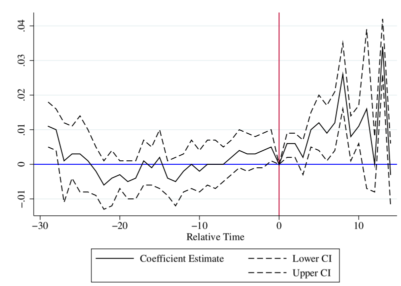
(d) Living In-State w/ Some College



(e) Living In-State w/ BA



(f) Currently Enrolled



(g) Currently Enrolled In-State

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 A: Additional Results

Table A.1: Comparison of BA Degree Results Across Different Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Arkansas and Georgia Only	0.025***	0.016**	0.008**	-0.007	-0.009	-0.007	-0.011***
	(0.004)	(0.006)	(0.004)	(0.009)	(0.005)	(0.004)	(0.003)
All Merit Aid States					-0.006	-0.010*	-0.007*
					(0.004)	(0.005)	(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	24-34

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 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.

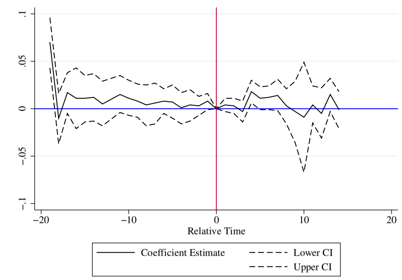
Appendix B: Omitting the 1990 Census

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

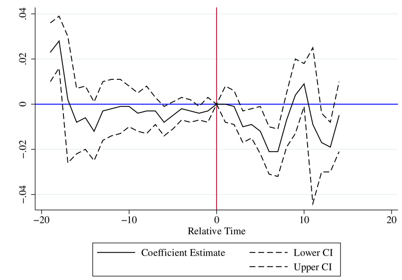
	(1)	(2)	(3)	(4)	(5)
Outcome	All States		Merit & Southern States		Merit States
Living In-State	0.001	0.005	0.011	0.007	0.004
	(0.007)	(0.005)	(0.007)	(0.005)	(0.005)
Living In-State w/ BA	-0.005*	-0.002	0.000	-0.001	-0.002
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
Living In-State w/ Some College Attendance	0.004	0.001	0.012*	0.008*	0.007*
	(0.005)	(0.005)	(0.005)	(0.004)	(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: 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.

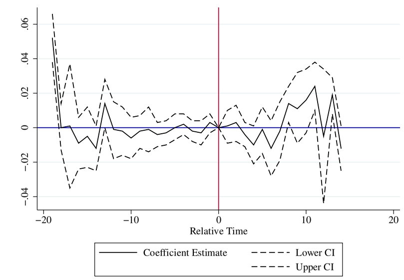
Figure B.1: Event Study Figures, Omitting 1990 Census



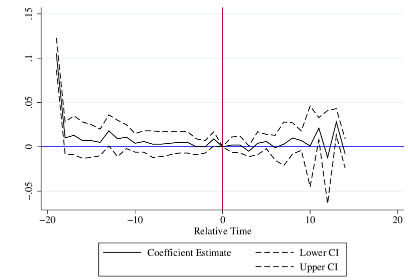
(a) Living In-State



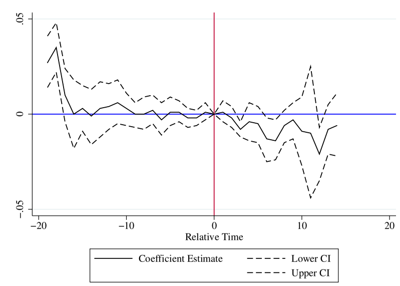
(b) BA Attainment



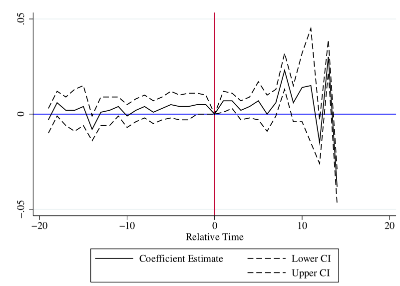
(c) Some College Attendance



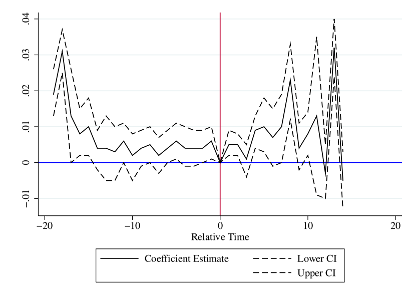
(d) Living In-State w/ Some College



(e) Living In-State w/ BA



(f) Currently Enrolled



(g) Currently Enrolled In-State

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 C: Using Unallocated Census Data

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

	(1)	(2)	(3)	(4)	(5)
Outcome			Merit &		
	All States		Southern States		Merit States
Living In-State	0.003	-0.004	0.016	0.011	0.008
	(0.010)	(0.011)	(0.010)	(0.009)	(0.006)
Living In-State w/ BA	-0.004	0.000	0.003	0.001	0.002
	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Living In-State w/ Some College Attendance	0.008	0.004	0.015**	0.011**	0.011**
	(0.006)	(0.007)	(0.006)	(0.005)	(0.004)
BA Degree	-0.007*	-0.001	-0.003	-0.006	-0.005
	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)
Some College Attendance	0.006	0.004	0.005	0.000	0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Currently Enrolled in College	0.008***	0.005**	0.006***	0.005***	0.006***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Currently Enrolled in College In-State	0.008***	0.005**	0.007***	0.006***	0.008***
	(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: 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 D: Using Different Age Ranges

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

	(1)	(2)	(3)	(4)	(5)
Outcome	All States		Merit & Southern States		Merit States
Living In-State	0.007	0.000	0.018*	0.013*	0.008
	(0.009)	(0.008)	(0.009)	(0.008)	(0.005)
Living In-State w/ BA	-0.006*	-0.002	0.002	-0.001	-0.001
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Living In-State w/ Some College Attendance	0.004	0.001	0.012*	0.008*	0.007*
	(0.005)	(0.005)	(0.005)	(0.004)	(0.003)
BA Degree	-0.007*	-0.001	-0.002	-0.006	-0.005
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Some College Attendance	0.003	0.000	0.003	-0.001	0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Currently Enrolled in College	0.004**	0.000	0.004**	0.002	0.003**
	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Currently Enrolled in College In-State	0.005**	0.002	0.004*	0.004**	0.005*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
N	8,313	8,313	3,586	3,586	2,445
Trend	N	Y	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.