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**State Merit-based Financial Aid Programs and College
Attainment**

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State Merit-based Financial Aid Programs and College Attainment

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Abstract

This paper examines the effects of state merit-based student aid programs on college attendance and degree completion. Our primary analysis uses microdata from the 2000 Census and 2001-2010 American Community Survey to estimate the effects of exposure to merit programs on educational outcomes for 25 states that adopted such programs by 2004. We also utilize administrative data for the University System of Georgia to look more in depth at the effects of exposure to the HOPE Scholarship on degree completion. We find strong consistent evidence that exposure to state merit aid programs has no meaningfully positive effect on college completion.

JEL Codes: H75, I23, J24, R28

Keywords: merit aid; HOPE; college attainment; degree completion; higher education

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1. INTRODUCTION

Higher education attainment has become increasingly important for the well-being of both individuals and the areas in which they live since it increases knowledge and skills that make individuals more productive in the labor market (Faggian and McCann, 2009a; Bauer, Schweitzer, and Shane, 2012). There is a large research literature showing that higher education increases individual wages and the probability of employment (see Psacharopoulos and Patrinos, 2004 and Dickson and Harmon, 2011 for reviews). But education also creates considerable positive externalities. For starters, more educated individuals create a fiscal surplus by paying higher taxes and consuming fewer public services (Trostel, 2010). They are also less likely to commit crimes and more likely to vote (Lochner and Moretti, 2004; Milligan, Moretti and Oreopoulos, 2004). Perhaps most important are the positive effects that highly educated individuals have on regional economic development. An increased local proportion of college educated workers is associated with increased wages and employment probabilities for other individuals, including those who never attended college themselves (Rauch, 1993; Moretti, 2004a, 2004b; Dalmazzo and de Blasio, 2007; Glaeser and Resseger, 2010; Iranzo and Peri, 2009; Abel, Dey and Gabe, 2012; Winters, 2013). These human capital externalities are thought to result for several reasons, including increased technological innovation by the higher educated, knowledge spillovers from highly skilled workers to their neighbors and coworkers, and production complementarities between high-skill and low-skill workers (Moretti, 2004a, 2004b). Furthermore, the local stock of human capital has been found to increase the quality of life in an area (Shapiro, 2006; Winters, 2011a) and lead to future population and employment growth (Glaeser, Scheinkman and Shleifer, 1995; Simon, 1998, 2004; Simon and Nardinelli, 2002; Winters, 2011b).

Given the regional benefits of higher education, increasing the percentage of young people with a college education is an important goal for policymakers, but there is little consensus on how that is best achieved (McHenry, 2014).¹ A number of studies have examined the migration decisions of high human capital workers, often trying to assess the relative effects of amenities versus employment opportunities (Gottlieb and Joseph, 2006; Ferguson et al., 2007; Chen and Rosenthal, 2008; Whisler et al., 2008; Partridge, 2010; Partridge et al., 2010).² The evidence suggests that both amenities and employment opportunities are important, but findings on their relative importance are mixed.

Another approach to growing the local stock of human capital is to increase the percentage of young people from the area who attend and complete college and remain in the area after college. Many states pursuing this approach have turned to merit-based financial aid programs (Groen, 2011). These programs award scholarships to in-state students who meet some merit requirement based on high school GPA (for example a 3.0 in Georgia) and sometimes SAT or ACT scores. State merit programs also require students to maintain a certain GPA in college in order to renew the award for subsequent years. Some states also have requirements that students be continuously enrolled and take a minimum number of credits. These merit scholarship programs have a number of goals and the relative importance of the goals likely differs across states and even across stakeholders within states, but increasing higher educational attainment among the state's young people is a widely agreed upon goal that has been used to justify the costs of these programs.³ Financial aid is expected to increase college attendance and completion rates because it lowers the costs of college for students and their families. However, it is unclear empirically if merit-based aid will actually increase higher education outcomes. The objective of this paper is to explore whether merit-aid programs do

increase college attainment and completion among young people from the state. There is a related line of research that explores the effect of merit-aid programs on the in-state retention of students post-college.⁴

The merit aid literature is relatively small with only a handful of studies examining the effects of merit aid on college attainment, and the few that do offer mixed results. The four published papers we identified consider merit programs in just three states (Arkansas, Georgia, and West Virginia). Two of the papers rely on 2000 Census public use microdata samples (PUMS) to measure merit aid effects in Arkansas and Georgia (Dynarski, 2008; Sjoquist and Winters, 2012) and two use state agency data to explore effects of programs in Georgia (Henry, Rubenstein, and Bugler (HRB), 2004) and West Virginia (Scott-Clayton, 2011). Dynarski, HRB and Scott-Clayton find a positive and statistically significant effect on the college graduation rate, while Sjoquist and Winters (2012) find a small and statistically insignificant effect.⁵

We identified 25 states that implemented a merit-based student aid program between 1991 and 2004 (see Table 1), although the characteristics of these programs differ substantially.⁶ The last two columns of Table 1 contain the percent of enrolled students receiving a merit award and size of the merit award per recipient for 2009-2010, and as can be seen, there are nine programs (Panel A) that have significantly larger participation rates and larger average awards; we refer to these as strong merit aid programs. The other 16 programs are classified as weak merit aid programs.⁷ In this paper we first employ the same basic approach used by Dynarski (2008) and Sjoquist and Winters (2012) but consider all 25 merit-adopting states, though we focus on the nine stronger programs. We combine the 2000 Census with the 2001-2010 American Community Survey to explore the effects of these merit aid programs on both college enrollment and completion, but our interest is primarily on completion. Previewing the results

using the Census/ACS we find strong consistent evidence that state merit aid programs had no meaningfully positive effect on the higher education attainment of young people in their states. We also estimate the effect separately for each of the 25 states and find no statistically significant positive effect of merit aid on college attainment in any of the states.

We also follow a second approach using administrative data for the University System of Georgia (USG).⁸ We use student records for two pre- and two post-HOPE cohorts of first time USG freshmen to conduct a pre- and post-HOPE cohort analysis. Controlling for sex, race, ethnicity, high school attended and SAT scores, we find no significant difference in degree completion between pre- and post-HOPE cohorts. Our results using USG data, therefore, reinforce our results using Census/ACS data, but are contrary to the findings of HRB (we discuss reasons for the different results below).

This paper provides several improvements over Dynarski (2008) and Sjoquist and Winters (2012) [jointly referenced as D-SW]. We combine the 2000 Census with the 2001-2010 American Community Survey, which allows us to estimate treatment effects for all 25 states with merit-based aid programs, both jointly and separately, compared to just the two states considered by D-SW. However, as noted above, some of these programs are stronger than others, and thus our preferred specification estimates merit effects for the nine states with strong merit aid programs. The longer data series also allows us to examine effects for more post-merit aid cohorts and at older ages. Our primary sample examines the effects of state merit programs on education outcomes of persons ages 24-30. We believe that examining many different treatment states and many different birth cohorts makes our analysis a considerable improvement over D-SW. Our approach allows us to estimate merit effects much more precisely than D-SW and

helps resolve the current uncertainty over the effects of state merit aid programs on educational outcomes.

Our analysis of USG data addresses some of the limitations to HRB's analysis, which compares a sample of HOPE students with high school GPAs just above 3.0 to a sample of non-HOPE students with GPAs below 3.0. While HOPE recipients in HRB have an overall GPA close to 3.0, HRB do not observe the overall GPA for non-recipients and do not know how close their overall GPAs are to 3.0. They match the two samples to have equal GPAs in core classes (HOPE eligibility was based on the overall GPA), but differences in academic ability remain. The mean SAT score for non-recipients is 47 points lower, and half of non-recipients are required to take remedial course work but only 34 percent of recipients are. We believe our approach to address the endogeneity issue is stronger than that used by HRB, although we estimate intent-to-treat effects of HOPE, while HRB try to estimate the effect of the treatment.⁹

2. CONCEPTUAL FRAMEWORK

Individual decisions regarding college education are often explained using the human capital model, which assumes that students maximize expected net present value; see Manski (1993). In such a framework, financial aid reduces student expectations regarding the cost of college, which economists argue should increase college matriculation, persistence, and completion. Empirical evidence is generally supportive of this position, with studies finding that general financial aid increases the likelihood of attending college, student persistence, and completion rates (DesJardins, Ahlburg, and McCall, 2002; Dynarski, 2003; Heller, 1997; Kane, 2003, 2007; Bettinger, 2004; Singell, 2004; Titus, 2006).

In principle, merit aid should have a similar directional effect on the likelihood of enrolling and graduating as does need-based aid. However, there are important reasons why we might expect that the size of the effect of state merit-based aid programs on access and completion will be smaller, and perhaps even non-existent. First, merit aid recipients are a select group of students who may be very likely to attend college with or without the existence of a state merit scholarship program (Ellwood and Kane, 2000). Most students who are at the margin of whether to enroll in college are likely below the merit-aid eligibility requirement and thus would be unaffected by the existence of the merit program. Along another important margin are students who lose their scholarship. Merit aid eligibility and renewal requirements are often sufficiently stringent that a relatively small fraction of students receive the award for four years of college (Dee and Jackson, 1999). Carruthers and Ozek (2012) find that students who lose their merit-aid are much less likely to persist in college than those who keep the award. Those who lose the award are more likely to be at the margin of staying in college vs. dropping out. Ultimately, merit programs may have minimal effects on overall college enrollment and degree completion rates because they are not targeted to students making marginal enrollment and completion decisions.¹⁰

Other responses may further reduce the effects of merit aid on educational attainment. Long (2004), Steele (2007), and Jensen (2011) find that colleges and universities respond to merit aid programs by raising tuition levels. States and/or colleges have lower need-based aid in response to new merit aid (Heller, 2002). Merit programs, therefore, could actually increase the cost of higher education for more marginal students who do not receive a merit award, which might reduce enrollment and graduation rates for such students. We investigate the effect of exposure to a state merit

aid program on educational attainment, testing the null hypothesis that merit programs have no effect on educational attainment, as measured by either any college, an associate's degree, or a bachelor's degree.

3. EMPIRICAL FRAMEWORK FOR CENSUS/ACS ANALYSIS

Data

We first use public use microdata samples (PUMS) from the 2000 decennial census long form and 2001-2010 American Community Survey (ACS) available at IPUMS (Ruggles et al., 2010). For 2000 we combine the 1% and 5% PUMS, which are files released separately and include one percent and five percent of the U.S. population. Sjoquist and Winters (2012) find non-trivial differences in education levels in Arkansas and Georgia between the 1% and 5% PUMS for 2000, so we combine the two instead of using only one. However, results below are robust to using only the 1% or only the 5% PUMS for 2000 and are robust to excluding the year 2000 data from the analysis. The census long form was discontinued after 2000 and replaced with the annually conducted American Community Survey.¹¹ For years 2001-2004, the ACS is a roughly 0.4 percent sample of the population and for 2005-2010 the ACS is a one percent sample of the U.S. population. We use Census weights to give each year roughly equal weight.¹²

Our primary interest is in the effects of merit programs on college degree completion. However, we first consider the effect of merit programs on the likelihood of having ever completed any amount of college. Our second education outcome is whether an individual has completed any college degree including an associate's degree or higher.¹³ However, there is a considerable difference in both the private and social benefits between completing an associate's

degree and completing a bachelor's degree or higher (Kane and Rouse, 1995). Thus, our third outcome is whether an individual has completed a bachelor's degree or higher.¹⁴

The PUMS also include information on individual age, sex, race, Hispanic origin, and state of birth; we use these in our analysis. We restrict the sample to persons ages 24-30. We exclude persons below age 24 because we are primarily interested in longer run effects of merit programs on college attendance and completion and not on whether merit programs alter the timing of these outcomes. The upper age cutoff is more arbitrary but was chosen in part because only a few states adopted programs early enough that persons exposed to a merit program were older than age 30 during our sample period. The results are qualitatively robust to varying upper and lower age cutoffs. We also exclude from the sample individuals with imputed information for age, education or state of birth, but results are robust to including these individuals.

Similarly to Dynarski (2008) we also collect information on a few state level variables intended to control for macroeconomic conditions in an individual's state of birth for the year the individual was 18 years of age. First, previous researchers have found that larger cohorts of college age persons in a state have lower college enrollment and degree completion rates, likely because states face some supply constraints in providing higher education (Card and Lemieux, 2000; Bound and Turner, 2007). We obtain data from the U.S. Census Bureau on the number of 18 year olds in the state in a given year (measured for July) and convert to logs and use this to control for cohort size in a state and year.¹⁵ The unemployment rate might also affect decisions to attend college because a more difficult job market can reduce a young person's opportunity cost of attending college and make them more likely to enroll. We collect state by year unemployment rates from the Bureau of Labor Statistics (BLS) and include this as another control variable. We also control for median household income and the college wage premium

in an individual's state of birth the year the individual was 18 years old, both computed using the March Current Population Survey (CPS). Greater household income suggests greater parental resources that make college more attainable for marginal students. The college wage premium is computed as the logarithmic difference in annual wage and salary income between persons with only a bachelor's degree and persons with only a high school diploma, using linear regression to control for age, sex, race, and Hispanic origin; the college wage premium CPS sample is also restricted to paid employees ages 25-54 who work at least 35 hours per week and 35 weeks during the year. The college wage premium signals to young people the potential benefits of attending college with a higher wage premium making college more appealing.

Research Design

We follow an intent-to-treat and control research design implemented via a difference in differences regression framework. The intent-to-treat group consists of individuals who were exposed to a state merit aid program when they graduated high school and the control group consists of individuals who were not exposed to a merit aid program. Unfortunately, we do not know when and where individuals graduated high school. We follow previous literature (e.g. Dynarski, 2008, Hickman, 2009, Sjoquist and Winters, 2012, 2014) and assign individuals to the merit program intent-to-treat group based on state of birth and year of birth, where year of birth is computed as the year of the survey minus age at the time of the survey. If an individual was born in a state that adopted a merit program and turned 18 after the program was implemented, they are assigned to the intent-to-treat group; if not they are part of the (unexposed) control group. The control group, therefore, includes both individuals born in states that never adopted merit programs and individuals born in merit-adopting states but turning 18 before the program

was implemented. Because some individuals attend high school outside their state of birth and some do not graduate high school at age 18, our treatment assignment will have some degree of assignment error. We take up the issue of assignment error in more detail later.

As noted above we identified 25 states that implemented a merit scholarship program between 1991 and 2004 (Table 1).¹⁶ Eligibility requirements are typically based on high school GPA, but there is often a standardized test score requirement and occasionally an income/asset requirement. Some states allow students at in-state private colleges and universities to be eligible for a scholarship while others restrict eligibility to students enrolled at public colleges and universities in the state. Renewal requirements are typically based on college GPA but can also include minimum course load requirements as in West Virginia. Award values range from \$500 per year to full tuition and fees at in-state public colleges and universities plus a book stipend. A few states also have multiple award levels based on different eligibility criteria. Dynarski (2004), Heller (2004), and the Brookings Institution¹⁷ provide details for most states.

One might be interested in examining the effects of specific merit program characteristics on educational outcomes, but there are problems with doing so. First, there is a dimensionality problem since many programs have relatively unique characteristics. Similarly, there is a classification problem because converting program requirements to usable data requires considerable information and a fairly large number of arbitrary classification decisions to be made; this is greatly complicated by the fact that many states changed their program's characteristics several times since inception.

Instead of trying to isolate the effects of specific merit program characteristics we adopt a much simpler approach that uses a dummy variable equal to one if an individual was exposed to a merit program. However, we recognize that some of these programs are relatively small and

unlikely to have a sizable impact on education outcomes. Based on program characteristics we identify nine states that adopted “strong” merit programs: Florida, Georgia, Kentucky, Louisiana, Nevada, New Mexico, South Carolina, Tennessee, and West Virginia. The other 16 states are considered to have “weak” programs. The nine strong states are defined as such because they have large broad-based programs that provide relatively large awards. Most of the other merit states have relatively strict criteria that limit eligibility to either the very best students or students with relatively low income. Two of the 16 states, Michigan and Mississippi, also have broad-based programs but are excluded from the group of “strong” states because they offer relatively small awards to most students.¹⁸ Michigan’s program, which was discontinued after 2008, provided a one-time award of \$2500 and the Mississippi TAG program offered only \$500 for the first two years of college and \$1000 for the third and fourth years. The “strong” programs provide scholarships large enough to cover full tuition or nearly full tuition at public colleges and universities in the state. The results below are qualitatively robust to several alternative definitions of “strong” states such as including Michigan and Mississippi as well as including all 13 states listed in Dynarski (2004).

In results not shown, we did estimate a regression equation that included a dummy variable equal to one if the state required the student to take a full load to receive a merit scholarship and results were similar to those using the simple merit dummy, that is, a full load requirement has no statistically significant effect.¹⁹ Later, we estimate separate merit effects for each merit state; doing so could help shed light on which program characteristics matter, if any.

Given that we expect that the effect of exposure to a merit aid program will depend positively on the percentage of enrollees who earn merit aid and the size of the award, our preferred approach for assessing the effects of exposure to broad-based merit scholarship

programs on higher education outcomes is to compare states with strong merit programs to states with no merit program. Weak program states receive only a “partial treatment” compared to the “full treatment” of strong program states and are less likely to have large impacts on educational outcomes. Thus, including weak merit states in the intent-to-treat group is likely to attenuate coefficient estimates toward zero. However, they do receive some treatment so including weak merit states in the control group is also likely to attenuate coefficient estimates toward zero. The purest test is to exclude weak merit states from the analysis and estimate the effect of state merit programs based on states with strong programs and states with no program. We do, however, examine the robustness of our results to including weak merit states in the analysis.

Econometric Model

For each of the three binary educational outcomes that we consider as dependent variables, we estimate linear probability models (LPM) as follows:

$$P(Y_{isct} = 1) = \Gamma_s + \Pi_c + \beta X_{isct} + \delta Z_{sc} + \theta Merit_{sc} + \varepsilon_{isct},$$

where Γ_s includes state of birth fixed effects, Π_c includes year of birth cohort fixed effects, X includes dummy variables for individual characteristics including sex, race, Hispanic origin, and age, Z includes the state of birth characteristics at age 18 discussed previously, and $Merit$ is an indicator variable equal to one if the individual was exposed to a state merit program and zero otherwise. The state of birth and year of birth fixed effects allow the model to be interpreted as a difference-in-differences model identified by differences across birth states and across birth cohorts within birth states. The models are estimated via Ordinary Least Squares (OLS), but using probit or logit yields very similar average marginal effects; we use OLS because it makes reporting and interpreting the results easier and has been widely used in the program evaluation

literature, especially that related to the effects of merit aid, e.g., see Dynarski (2000, 2004, 2008). Summary statistics for the variables in this study are reported in Table 2 separately for strong merit, weak merit, and non-merit birth states. The strong merit birth states have much lower educational outcomes than both the weak merit and non-merit states. There are also meaningful differences in some of the explanatory variables, so these maybe important factors. We consider the effects of excluding and progressively including the individual and state characteristics.

Because we use individual level data and the *Merit* variable is defined based on state and year of birth, OLS standard errors should not be used because they do not account for intra-cluster correlation (Bertrand, Duflo, and Mullainathan, 2004; Donald and Lang, 2007; Cameron, Gelbach and Miller, 2008). Instead, we report both standard errors clustered by state of birth and 95 percent confidence intervals based on procedures suggested by Conley and Taber (2011). Clustered standard errors are typically preferred to OLS standard errors, but Conley and Taber (2011) show that clustered standard errors can be downwardly biased when the number of policy changes is small. They suggest a confidence interval procedure based on the distribution of residuals across the control states and show that their procedure outperforms conventional clustered standard errors when there are a small number of treatment groups and does no worse more generally.²⁰ Our preferred approach includes nine states with policy changes, so we report both clustered standard errors and Conley-Taber confidence intervals.

4. EMPIRICAL RESULTS FOR CENSUS/ACS ANALYSIS

Basic Results

Table 3 reports results that start with a limited set of control variables and then progressively add more detailed controls. Persons born in weak merit states are excluded. The

treatment group includes only persons born in strong merit states who were exposed to a strong merit program. The control group includes persons born in non-merit states and persons born in strong merit states but reaching age 18 before the merit program was implemented. The effects of state merit aid programs on each of the three educational outcomes are reported in Panels A, B, and C. The first column of Table 3 includes dummies for state of birth, year of birth, age, sex, race, and Hispanic origin. The second column adds the state level controls. The third column contains all of the controls in column 2 and also includes state of birth by year of birth time trends; however, this is not our preferred specification because merit program effects may increase over time causing them to be captured by the state of birth time trend instead of the merit dummy. The fourth column contains all of the controls in column 2 and also includes region of birth by year of birth fixed effects. This specification restricts the control states for each merit state to only include non-merit states in the same census region. However, because strong merit states are only in the South and West regions, this approach does not utilize information for the Northeast and Midwest regions. The reduced number of control states also prevents us from computing Conley-Taber confidence intervals in the fourth column. Our benchmark specification is in the second column, but establishing the robustness of our results to state-specific time trends and region-year effects in the third and fourth columns makes our results more credible.

The results in Table 3 tell a consistent story. The coefficient estimates are small and close to zero for every regression; 11 of the 12 coefficients are negative and only one is positive, but the magnitudes are all less than one percentage point. Altering the set of controls changes the coefficient estimates only slightly. Furthermore, all of the coefficient estimates are statistically insignificant using both clustered standard errors and Conley-Taber 95 percent

Confidence Intervals. The confidence intervals are also fairly narrow and allow us to reject hypotheses of large effects.²¹ The benchmark specification in column 2 yields coefficients of -.0026, -.0025 and -.0045 in Panels A, B, and C. These results imply that merit aid has no economically meaningful effect on college attendance or degree completion. This is not an unexpected result since merit-aid recipients are above average students and thus more likely to attend and complete college in the absence of merit aid; financial aid is unlikely to have much of an effect on the educational attainment of students with otherwise high attainment probabilities. Furthermore, given that a large percentage of students lose merit aid after one year, the actual size of the aid is relatively small for the students who might be most affected by it. The rest of this section presents several robustness checks and alternative specifications.

Different Definitions of Intent-to-Treat and Control Groups

Table 4 considers how the results are affected by altering the assignment of states to the intent-to-treat and control groups. The first column of Table 4 replicates the second column of Table 3, our benchmark specification. The second column of Table 4 includes weak merit states in the analysis and includes individuals exposed to weak merit programs as part of the intent-to-treat group. The third column of Table 4 includes all persons born in weak merit states as part of the control group. Although we do not necessarily agree, one might be concerned that the non-merit states are somehow different from merit states and make a poor control group. To address such concerns, the control group for the fourth and fifth columns of Table 4 only includes persons born in merit states but who are too old to have been exposed to a merit program in their state of birth. The fourth column limits the sample to persons born in strong merit states. The fifth column limits the sample to persons born in strong or weak merit states and includes

persons exposed to weak merit programs in the treatment group. However, having no states without policy changes in the control group prevents us from estimating Conley-Taber confidence intervals for the last two columns.

The results in Table 4 are quite consistent across equations and are consistent with the results in Table 3 as well. The coefficient estimates in columns 2 and 3 are slightly larger than in column 1 but still negative for five of the six regressions and in no case statistically significant. The coefficient estimates in the fourth and fifth columns are also small and statistically insignificant for five of the six regressions. The exception is for bachelor's degrees or higher in the fourth column, which has a coefficient of $-.0058$ and is significant at the ten percent level based on standard errors clustered by state of birth. However, this magnitude is relatively small and having only nine states suggests clustered standard errors are likely to be downwardly biased, so we do not interpret this as convincing evidence of a significant negative effect. Table 4 as a whole provides consistent and convincing evidence that exposure to merit programs had no meaningfully positive effects on college attainment.²²

Accounting for Assignment Error in Treatment Status

Table 5 presents results that account for assignment error in treatment status. The results in the rest of this paper assign an individual to the intent-to-treat group if they were born in a state that adopted a merit program and turned 18 after the program was implemented. However, some individuals attend high school outside their state of birth and some finish high school before or after age 18. To account for possible assignment error due to age when finishing high school, the first column of Table 5 excludes from the sample persons who were ages 18 or 19 when a merit program was implemented in their birth state.²³ The earlier analysis assigns those

who were 18 years old when the program was first implemented to the intent-to-treat group and those who were 19 years old to the control group. But some who were 18 when the program started could have finished high school a year earlier at age 17 and not been eligible. Similarly, some individuals who were 19 when the program started could have graduated high school at 19 and been eligible for the merit program. Column 1 of Table 5 excludes these “marginal” birth cohorts from the analysis to reduce assignment error. The coefficient estimates are now slightly more negative than the benchmark specification in Table 3 column 3, but the difference is slight. The coefficient for bachelor’s degrees or above is significant based on clustered standard errors but not significant using Conley-Taber 95 percent confidence intervals. These results continue to imply that merit aid has little to no effect on college attainment.

We next account for possible assignment error in treatment assignment due to persons attending high school outside their state of birth.²⁴ Following Dynarski (2008) we explore measuring merit exposure based on the predicted probability of going to high school in a merit state based on state of birth. Using the sample of 15-17 year olds in the 2000 Census and 2001-2010 ACS, for each merit state we regress the probability of living in that state during high school on a complete set of state of birth dummies. We then use the predicted values and year of birth to compute the probability that an individual was exposed to a merit program.²⁵ We then replace the merit dummy in our education outcomes LPM models with the probabilistic merit variable. Results are reported in column 2 of Table 5 and are very similar to those for the preferred specification. The coefficient estimates are small, negative, and statistically insignificant.

The third column of Table 5 combines the procedures in the first two columns to account for possible assignment error in treatment due to both state and year of birth. The results tell a

familiar story. The coefficients are small, negative, and insignificant. Thus accounting for assignment error in treatment does not change the basic result that there is no evidence of a meaningful positive effect of exposure to state merit aid programs on college attendance or completion.

Effects by Sex and Race/Ethnicity

The prior results include dummy variables for sex, race/ethnicity, and age, but it is also of interest whether the coefficients on merit aid differ across these groups. Table 6 presents results for merit program effects by sex and race/ethnicity. The first two columns reports results for white non-Hispanic males and white non-Hispanic females. The last two columns report results for non-white or Hispanic males and non-white or Hispanic females. The coefficient estimates are typically small and negative and are in no case statistically significant based on Conley-Taber confidence intervals. It is worth noting that the coefficient for college attendance for non-white or Hispanic males is $-.0199$ which is the largest coefficient in absolute value thus far. However, as seen by the Conley-Taber confidence intervals, restricting the analysis to this group produces fairly noisy estimates. The results in Table 6 suggest that there is no meaningful positive effect of exposure to state merit aid programs on college attainment for demographic subgroups.²⁶

Effects by Birth State

There are reasons to expect that the effect of merit aid on college attainment might differ across states. First, we might expect that the larger the pre-merit aid probability of attaining a given education level, the smaller the marginal effect of merit aid on that attainment outcome; i.e. merit aid might have larger effects in states with low attainment. Interstate differences in pre-merit aid attainment could be related to several state characteristics. For example, states with

higher quality high schools might produce students who are more academically prepared for college and thus more likely to complete college. Children are more likely to graduate if their parents have a college degree (Goldrick-Rab, Harris, and Trostel, 2009), and a larger adult population with a college degree provides more positive role models for students. On average, students from higher income families are more academically prepared and less resource constrained, and therefore more likely to graduate from college (Federman, 2007). This suggests that the larger the percentage of adults with a college degree and larger the average household income, the smaller the effect of merit aid at the margin. Second, the effect of a merit aid program might also be affected by the nature of the merit aid program. For example, the greater the percentage of high school graduates who are eligible and the larger the scholarship, the larger the expected effect on enrollment and graduation. A requirement that students be enrolled full time to maintain eligibility should reduce the time to graduate, and may increase the graduation rate, as suggested by Scott-Clayton (2011).

Table 7 contains estimates of separate effects for all 25 merit states to see if there are any meaningful merit effects for individual states that are not detectable from the average effect over several states. The analysis is the same as in the benchmark specification except that each regression includes only one treatment state; the other 24 merit states are excluded. We report Conley-Taber 90 percent confidence intervals because having only 27 total states (one merit state and 26 non-merit states) per regression prevents us from computing the 95 percent confidence intervals. Including only one treatment state also decreases the precision of the estimates and widens the confidence intervals, especially for states that have only a few birth cohorts exposed (or not exposed) to their merit program. Having only one merit state per regression also means

that clustered standard errors are severely biased so we do not report them in order to conserve space.

The coefficient estimates for individual states are relatively dispersed as expected, but they are generally small and not statistically significant, with a few exceptions. Only five of the 75 regression coefficients are significant at the 10 percent level; all five are negative and are for weak merit states. These include the any college coefficients for Alaska and Washington and the associate's or higher coefficients for Idaho, Utah, and Washington. The largest positive coefficients for degree completion were for Maryland, South Dakota, and West Virginia, all of which have bachelor's degree coefficients just above two percentage points. However, only West Virginia has a strong merit program and all of these were adopted fairly recently and have only a few intent-to-treat cohorts. Maryland and West Virginia have only three years of birth cohorts included in the intent-to-treat and South Dakota has only one cohort in the intent-to-treat group. Consequently, the coefficients are not precisely estimated and the confidence intervals are wide enough that none of these are statistically significant. Furthermore, when estimating regressions for many individual states we would expect the coefficient estimates to be distributed around the true coefficient. Finding a few coefficients that are slightly positive is not surprising. Looking at individual states provides further evidence that exposure to state merit aid programs have no meaningfully positive effect on college attendance and completion.

5. EMPIRICAL FRAMEWORK FOR UNIVERSITY SYSTEM OF GEORGIA ANALYSIS

We next explore the effects of the Georgia HOPE Scholarship on educational outcomes using administrative data for the University System of Georgia (USG). Henry, Rubenstein, and Bugler (HRB) (2004) use Georgia administrative records and found that the HOPE program

increased graduation rates, which is contrary to our results presented in Table 7. Thus, we revisit the use of Georgia administrative records using a different research design to see if we get similar results to HRB. The USG is a statewide higher education system that includes a total of 35 two- and four-year colleges and universities in Georgia. In 1990, the USG made up 72 percent of total undergraduate enrollment in the state (National Center for Education Statistics, 1995). We obtained individual student data for four cohorts of first-time freshmen from the USG Board of Regents.²⁷ The data include all Georgia residents who graduated high school in Georgia in 1990, 1991, 1995 and 1996 and matriculated in the USG in the summer or fall immediately after high school. Data were obtained for the 1995 and 1996 cohorts instead of the 1993 and 1994 cohorts because these first two post-HOPE cohorts were initially subject to an income cap for eligibility. The 1992 cohort was not included because of concerns that some students could have anticipated HOPE and changed their behavior in anticipation.

We examine the effects of exposure to the HOPE Scholarship on degree completion in the USG using a cohort analysis. We consider two main outcomes: 1) completion of an associate's or bachelor's degree and 2) completion of a bachelor's degree. We look at differences in these outcomes between the pre- and post-HOPE cohorts after four, five, six, and twelve years after graduating high school and enrolling in the USG. In addition to information on associate's and bachelor's degrees awarded, the USG data also include sex, race, Hispanic origin, high school attended, SAT score, and core course high school GPA.

A concern with the USG data is that HOPE could have, as implied by Dynarski (2000) and Cornwell, Mustard, and Sridhar (2006), affected the composition of the student body post-HOPE, in particular, that the HOPE program enticed students who would have gone to college out-of-state in the absence of HOPE to go to college in-state. This could result in a possible

endogeneity problem. If such migratory students are equivalent to the non-migratory students, then endogeneity should not be a problem. However, if such students are less inclined (more inclined) to graduate, then our estimate of the effect of HOPE on attainment will be negatively (positively) biased. While we don't know which of these three alternatives is correct, we believe that migratory students are not less likely to complete college, since these students are likely to be higher quality students, and thus more likely to complete college. This suggests that if we could control for the change in the student body, the effect of HOPE would be smaller (more negative) than we estimate. Since clearly we cannot know which students these are, we control for the quality of students using SAT scores, which should reduce the importance of the endogeneity, although there may be unmeasured differences.

The linear probability model is as follows:

$$P(Y_{it} = 1) = \beta X_{it} + \theta PostHOPE_t + \varepsilon_{it},$$

where X includes dummy variables for sex, race, Hispanic origin, dummy variables for high school attended, and in some regressions, SAT score and high school GPA.²⁸ $PostHOPE$ is a dummy equal to one for the 1995-96 cohorts and zero for the 1990-91 cohorts.²⁹ Therefore, θ measures the effect of exposure to the HOPE program on degree completion in the USG.³⁰ Table 8 provides summary statistics for selected variables. Interestingly, mean SAT scores, mean high school GPAs, and the number of observations increase after HOPE. While other explanations are possible, this is certainly consistent with the possibility that HOPE caused a net increase in the number of high ability Georgia residents enrolled in the USG. It is also possible that rising admission standards could have crowded out some more moderate ability students, but we are unable to identify such students or examine how they are affected.

Note that the *PostHOPE* dummy equals one for all students in the post-HOPE cohort and not just students who received the HOPE Scholarship. Thus, as with the Census data, we are measuring the effect of exposure to HOPE on college attainment. We do not have the HOPE GPA needed to determine if pre-HOPE students would have qualified for HOPE had it existed. Henry, Rubenstein, and Bugler (2004) limit their analysis to post-HOPE students and estimate the effects of actual HOPE receipt on four-year degree completion based on differences between students with similar core GPAs but different HOPE GPAs. However, even controlling for core GPA, post-HOPE students who qualify for HOPE are likely higher quality and more motivated than those who do not. For example, marginal students with more to gain from receiving a HOPE Scholarship are more likely to take actions to gain eligibility such as taking more electives courses to boost their GPA above a 3.0. Less motivated students are less likely to take such actions to earn a HOPE Scholarship but are also less likely to take the necessary actions to succeed in college.

6. EMPIRICAL RESULTS FOR UNIVERSITY SYSTEM OF GEORGIA ANALYSIS

Table 9 presents the results for the USG analysis. Panel A presents results for completing an associate's or bachelor's degree and Panel B presents results for completing a bachelor's degree. The first column contains no control variables. The second column includes dummies for sex, race, Hispanic origin, and high school attended, but not SAT or high school GPA. The third column adds SAT scores and the fourth adds high school GPA.

There are important caveats for the third and fourth columns. If HOPE caused Georgia high school students to improve their SAT scores, then including SAT scores in the regression may be inappropriate since including SAT score will attenuate the measured effect of exposure

to HOPE. However, if the observed increase in SAT scores is due to changes in who enrolls in the USG, then SAT score increases should be controlled for to account for the changing composition of students. Given that there was no incentive for a student to improve his SAT score, we are inclined to believe that increases in average SAT scores reflect high ability students enrolling in-state who would have enrolled out-of-state without HOPE.

High school GPAs also increased with the adoption of HOPE. Sjoquist and Winters (2013) argue that HOPE caused high school grade inflation for post-HOPE cohorts in Georgia, while Henry and Rubenstein (2002) argue that the higher grades reflected improved high school performance. If HOPE caused high school GPAs for post-HOPE students to be inflated, then one should be very cautious in interpreting results that control for high school GPA because looking at students with the same GPA compares lower quality post-HOPE students to higher quality pre-HOPE students. Since student quality is strongly positively correlated with degree completion, grade inflation will create a negative bias in θ when controlling for high school GPA. On the other hand, if increased high school GPAs reflect better high school performance due to HOPE, then including high school GPA would attenuate the measured effect of exposure to HOPE. For both possibilities, high school GPA should not be included, but we do report results that include it. Given these arguments, we believe that the regression in column 3 is the most appropriate specification in the table since it includes dummies for age, gender, race/ethnicity, and SAT, and does not include GPA.

The results with no controls in the first column suggest a relatively small increase in degree completion in Panel A but a larger increase in Panel B for bachelor's degrees. The effects for bachelor's degrees are statistically significant for four, five, six, and twelve years after starting college based on standard errors clustered by year, and the magnitudes increase slightly

over time. However, there are only four cohorts so clustered standard errors should be interpreted with caution. The Conley-Taber procedure is not feasible since we have administrative data for only one state. Adding dummies for sex, race, Hispanic origin and high school attended in the second column increases the coefficients slightly. The positive effect of HOPE on the probability of degree completion for these two specifications likely results from the changing composition of students in the USG, specifically the fact that the average quality of students has increased; this is an interesting result in itself.

Our primary interest is whether and how HOPE affected the likelihood that a given quality student would complete a degree. Controlling for student quality by adding the SAT score dummies in the third column reduces the coefficient estimates to roughly zero and makes them statistically insignificant. Adding high school GPA dummies in the fourth column causes the coefficient estimates to be negative and significant. The results for the preferred specification in column 3 suggest that controlling for changes in student quality using SAT scores exposure to HOPE had no meaningful effect on degree completion rates in the USG. These results reinforce our findings using the Census/ACS data. Of course, it is possible that factors other than HOPE could have affected attainment, but we are unaware of any other policies that were adopted by Georgia at that time that might have affected attainment.

7. CONCLUSION

Increasing the percentage of young people with a college education is an important goal for both nations and regions but there is little consensus on how that can be best achieved. Providing financial aid, both need-based and merit-based, is often advocated as a useful policy tool. We examine the effects of recently adopted state merit-based financial aid programs on

college attendance and degree completion. The small literature that exists has only looked at one or two states at a time and has provided mixed results. Our main analysis utilizes public use microdata samples from the 2000 Census and 2001-2010 American Community Survey (ACS), which allows us to estimate the effects of exposure to merit programs on educational outcomes for nine states with strong merit programs and 16 other states with weaker merit programs. We also utilize administrative student records for the University System of Georgia (USG) to take a deeper look at the effects of exposure to the HOPE Scholarship on degree completion in Georgia.

We find strong consistent evidence that exposure to state merit aid programs had no meaningfully positive effect on individual college attendance or degree completion. The coefficient estimates for our benchmark Census/ACS specification are small, negative, and statistically insignificant. We also consider a number of robustness checks including varying the states included in the analysis and estimating separate merit effects for each of the 25 merit states. Coefficient estimates for the robustness checks are typically small and statistically insignificant, and more frequently negative than positive. Our benchmark specification for the USG analysis also yields small and insignificant effects of exposure to the HOPE Scholarship on degree completion.

To the extent that students are responsible for paying part of the cost of college, merit aid reduces the potential need to work while in school. This would allow more time to study and reduce the need to drop out. This implies that merit aid should increase college attainment. However, merit aid students are higher quality students, and perhaps more inclined to go to college and to graduate. Thus, at the margin merit aid may have little effect on college attainment. Furthermore, even for strong merit aid states merit aid generally doesn't cover more than tuition. Thus, merit aid may be too small to motivate marginal students to enroll or

graduate. In addition, to the extent that students lose merit aid, any effect will be reduced. Conceptually, merit aid programs may have minimal effects on college degree completion because they are not targeted to students at the margin of graduating or not.

There are policies that might be considered. Requiring students to repay part of the aid if they don't graduate would provide an incentive to complete college. Targeting funding to lower-income students, and perhaps increasing the size of such awards, might increase the effect on attainment.

While state merit aid programs may produce other benefits for their states, they do not appear to be effective at increasing the likelihood that a young person from the state earns a college degree. State policymakers should be aware of these results when evaluating the overall benefits of merit aid programs and when trying to build the stock of college-educated persons in their states.

ENDNOTES

¹ To the extent that various policies alter the location decisions of those with higher education and not the educational decisions of young people, then these policies may offer minimal social benefits at the national level.

² While much of the literature examines the U.S., researchers interested in migration of college graduates have also examined other countries including Canada (Brown and Scott 2012), the UK (Faggian, McCann, and Sheppard, 2007a, 2007b; Faggian and McCann, 2009b; Abreu, Faggian, and McCann, forthcoming), the Netherlands (Venhorst, Van Dijk and Van Wissen, 2011; Venhorst, 2013), Finland (Haapanen and Tervo, 2012), and Australia (Corcoran, Faggian, and McCann, 2010).

³ A related goal of merit aid programs is to build the stock of college-educated workers in the state by affecting the college and post-college location decisions of people who would attend college with or without the merit scholarship program. Researchers estimating the effects of state merit aid programs on college enrollment include Dynarski (2000, 2004); Cornwell, Mustard, and Sridhar (2006); Singell, Waddell and Curs (2006); Goodman (2008); Orsuwan and Heck (2009); Farrell and Kienzl, (2009); Zhang and Ness (2010); Winters (2012); and Hawley and Rork (2013). This literature typically finds a significantly positive effect on the probability of attending college in-state but inconsistent effects on the overall probability of attending college.

⁴ Hickman (2009), Sjoquist and Winters (2014) and Fitzpatrick and Jones (2012) find that merit programs also affect post-college location decisions and increase the probability that young college attendees reside in their native state during their immediate post-college years (e.g. ages 24-30). However, the effects on post-college location decisions are not especially large, likely

because while merit programs induce a large number of students to stay in-state for college, many of them leave the state after college (Sjoquist and Winters, 2013; Leguizamon and Hammond, 2013).

⁵ Recent working papers by Castleman (2012) and Fitzpatrick and Jones (2012) also examine the effects of merit programs on college degree completion. Castleman uses administrative data for Florida and finds that receiving a full merit scholarship has a positive effect on degree completion but receiving only a partial scholarship has no effect. Fitzpatrick and Jones use census data and find a slight negative effect on degree completion.

⁶ There are discrepancies in how various authors classify scholarship programs. We used various sources to develop our list; see the sources listed in Table 1. We also identified four merit aid programs that have been adopted since 2004: Delaware, Massachusetts, Montana, and Wyoming. Because these programs are so new, we do not include them in our analysis.

⁷ West Virginia was included as a strong merit aid state because it has a very high average award, despite a somewhat lower participation rate. California has the highest average award among the weak merit aid states, but is not classified as a strong merit aid state because its participation rate is very low. In addition, California has a low minimum GPA for eligibility and an income limit. Mississippi has a relatively high participation rate, but has a very low average award, and thus is classified as a weak merit aid state.

⁸ A detailed history of HOPE is provided by Sjoquist and Walker (2010).

⁹ Both census data and state administrative data have strengths and weaknesses for examining the effects of merit programs on educational outcomes. State administrative datasets typically have detailed information on student characteristics, but they only include students who enroll in the public higher education system for a single state. This can result in significant selection

problems if state merit aid programs incentivize higher quality students to stay in-state for college instead of attending college out-of-state; the college attainment of such students would be recorded as being due to the merit aid, while they only changed their state of enrollment. Researchers can control for observable characteristics but selection on unobservables can still threaten the validity of using administrative data. Furthermore, the ability to estimate difference-in-difference or regression discontinuity models is sometimes hampered by the lack of information necessary to accurately determine which students would have been eligible for merit-aid prior to the adoption of the merit-aid program or to identify sharp discontinuities. The census PUMS do not have as detailed information on student characteristics but they do include the total population regardless of where they attend college. Census data also allow us to examine the effects of merit programs in multiple states instead of looking at only one. However, census data do not identify who actually received merit aid. Since both census and administrative data have their merits and have been used in previous studies, we examine the effects of merit programs on educational attainment using both census data for the U.S. and administrative data for Georgia.

¹⁰ Merit scholarship programs may also affect student decisions in other ways. Cornwell, Lee, and Mustard (2005) find that HOPE students at the University of Georgia reduced course taking, presumably to spend more time per course or to delay losing HOPE. The first renewal decision in Georgia is made after 30 credit hours are completed, so a student could take 29 credit hours during the first two semesters and then another 15 before the GPA is calculated. However, these effects may affect when an individual earns a college degree without actually affecting whether the individual eventually earns a college degree. Furthermore, merit aid may be primarily a windfall gain to middle-income families. For example, Cornwell and Mustard (2007) find that

car purchases in higher income Georgia counties are positively correlated with the number of HOPE Scholarship awards in the county.

¹¹ One subtle difference is that while the Census was conducted for a single point in time (April 1), the ACS is administered continuously throughout the year.

¹² The weights in each sample sum to the total population in that year. We also reweight the 2000 PUMS to avoid giving that year double weight since we use two samples. Results are qualitatively robust to not using weights.

¹³ Until very recently associate's degrees were miscoded in the 2001-2002 ACS on IPUMS, so our analysis for associate's degrees excludes these years. This error did not affect the other two educational outcomes, so we include the full set of years for those outcomes.

¹⁴ One might also be interested in whether state merit aid programs affected graduate degree completion. In results not shown, we do consider the effects of merit programs on graduate degrees and find small insignificant effects. However, since merit programs cannot be used for graduate education we do not focus on advanced degrees.

¹⁵ Cohort size at age 18 could be endogenous if parents move their kids to merit-adopting states. However, using cohort size at birth gives very similar results.

¹⁶ Delaware, Massachusetts, Montana, and Wyoming implemented programs in 2005 and 2006 but these do not affect our main sample because persons 18 years old in 2005 and 2006 are only 23 and 22 years old in 2010, the last year of our sample.

¹⁷ Brookings Institution data file is available at <http://www.brookings.edu/research/reports/2012/05/08-grants-chingos-whitehurst>

¹⁸ Dynarski (2004) lists 13 states with eligibility criteria such that “at least 30 percent of high school students hav[e] grades and test scores high enough to qualify for a scholarship” (p. 65).

These include the nine strong states, Michigan, Mississippi, Arkansas, and Maryland. However, Arkansas and Maryland enforced income limits that rendered them considerably less broad-based than the other 11.

¹⁹ Scott-Clayton (2011) argues that her results for West Virginia are driven by the requirement that to retain eligibility students must complete 30 credit hours each year. In addition to West Virginia, New Mexico and South Carolina have enrollment requirements.

²⁰ Their procedure first estimates mean residuals by state and year and then uses the empirical distribution of the mean residuals in the control states to estimate the cumulative distribution function for the treatment effect estimator. This involves hundreds (or even thousands) of iterations. With N treatment states, each iteration randomly draws N pseudo-treatment states from the combined set of treatment and control states and then estimates a pseudo-treatment effect. The empirical distribution of the pseudo-treatment effect estimates is then used to define a confidence interval for the actual treatment effect estimate. We can reject the null hypothesis that the actual treatment effect is zero if the actual treatment effect estimate is outside the bounds of the confidence interval.

²¹ Moderate size effects often cannot be ruled out, but the coefficient point estimates provide the single best estimate about the size of any possible effect and these are usually quite close to zero. Note also that the Conley-Taber confidence intervals are by nature not perfectly symmetric around the coefficient estimates because we have a moderately small number of treatment and control states. The confidence intervals would become more symmetric as the number of both treatment and control states became very large.

²² In results not shown we also experiment with limiting the set of states to only those in the South and West regions, which is similar to using region of birth by year of birth fixed effects in the fifth column of Table 3. Results are qualitatively similar to those above.

²³ Results are also robust to excluding those who were ages 17 and 20 when the merit program in their state began.

²⁴ 76 percent of 18 year olds in the Census/ACS live in their birth state.

²⁵ Predicted probabilities by year and state of birth are available from the authors by request.

²⁶ We also conducted formal two-sample t-tests using the clustered standard errors for whether the coefficient estimates for each outcome are statistically significantly different across the subgroups. The only pair of coefficients that is different at the five percent level of significance is the difference between the any college coefficients for white females and non-white males.

This is primarily driven by the large negative coefficient on attendance for non-white males.

²⁷ Our agreement with the Board of Regents limited our data request to four cohorts of students.

²⁸ Our SAT controls include 19 group dummies. The first is for missing SAT score and the other 18 are 400-590, 600-640, 650-690, 700-740, 750-790, 800-840, 850-890, 900-940, 950-990, 1000-1040, 1050-1090, 1100-1140, 1150-1190, 1200-1240, 1250-1290, 1300-1340, 1350-1390, and 1400-1600. Our high school GPA controls include 27 group dummies. The first is for missing high school GPA and the other 26 are 0-1.54, 1.55-1.64, 1.65-1.74, 1.75-1.84, 1.85-1.94, 1.95-2.04, 2.05-2.14, 2.15-2.24, 2.25-2.34, 2.35-2.44, 2.45-2.54, 2.55-2.64, 2.65-2.74, 2.75-2.84, 2.85-2.94, 2.95-3.04, 3.05-3.14, 3.15-3.24, 3.25-3.34, 3.35-3.44, 3.45-3.54, 3.55-3.64, 3.65-3.74, 3.75-3.84, 3.85-3.94, and 3.95-4.00.

²⁹ We experimented with controlling for USG institution attended; this does not meaningfully change the results.

³⁰ Because we only have administrative data for Georgia we are unable to estimate a difference-in-differences treatment effect as we did with the Census/ACS microdata. Instead we estimate a pre- and post-HOPE time difference within the USG. This assumes that the USG would have experienced no time trend between the two periods in the absence of HOPE. While we cannot completely rule this out, we did some exploratory analysis using Census/ACS data for other states. Specifically, we created a Census/ACS sample of persons reaching age 18 in 1990-1991 or 1995-1996 and born in Southern states that did not adopt a merit program between 1990 and 1996. We then separately regressed each of the degree completion dummies on dummies for state of birth, age, and a dummy for being born in 1995-1996 that is intended to measure if the latter cohorts differed from the earlier ones. For both degree completion dummies, the 1995-1996 cohort dummy coefficient was small and insignificant, suggesting that there was no trend in degree completion in similar states. Again, this does not completely rule out the possibility of trends in the USG data, but if there were broader macroeconomic trends, we would have expected them to show up in this Census/ACS analysis.

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TABLE 1: States with Strong and Weak Merit Programs Implemented 1991-2004

State	First Cohort	Program Name	Awardees as a Percentage of FTE Students, 2010	Grant Expenditures per FTE Student, 2010
<u>A. Strong Merit Programs</u>				
Florida	1997	Florida Bright Futures Scholarship	24.25%	\$580.50
Georgia	1993	Georgia HOPE Scholarship	30.71%	\$1,191.08
Kentucky	1999	Kentucky Educational Excellence Scholarship	35.71%	\$493.25
Louisiana	1998	Louisiana TOPS Scholarship	23.23%	\$708.57
Nevada	2000	Nevada Millennium Scholarship	25.55%	\$326.88
New Mexico	1997	New Mexico Lottery Success Scholarship	20.71%	\$494.67
South Carolina	1998	South Carolina LIFE Scholarship	18.35%	\$887.69
Tennessee	2003	Tennessee HOPE Scholarship	26.86%	\$919.57
West Virginia	2002	West Virginia PROMISE Scholarship	9.81%	\$484.78
<u>B. Weak Merit Programs</u>				
Alaska	1999	Alaska Scholars	4.46%	\$43.88
Arkansas	1991	Arkansas Academic Challenge Scholarship	1.63%	\$55.45
California	2001	Competitive Cal Grant Program	3.56%	\$254.00
Idaho	2001	Robert R. Lee Promise Scholarship	9.07%	\$54.41
Illinois	1999-2004	Illinois Merit Recognition Scholarship	NA	9.12 ^a
Maryland	2002-2005	Maryland HOPE Scholarship	NA	21.08 ^a
Michigan	2000-2008	Michigan Merit & Promise Scholarship	0.20%	181.06 ^a
Mississippi	1996	Mississippi TAG and ESG	18.73%	\$138.11
Missouri	1997	Missouri Bright Flight Scholarship	6.64%	\$136.89
New Jersey	1997 (2004)	New Jersey OSRP (STARS)	1.15%	\$35.79
New York	1997	NY Scholarships for Academic Excellence	1.90%	\$12.93
North Dakota	1994	North Dakota Scholars Program	0.38%	\$22.96
Oklahoma	1996	Oklahoma PROMISE Scholarship	11.89%	\$58.33
South Dakota	2004	South Dakota Opportunity Scholarship	9.26%	\$100.71
Utah	1999	New Century Scholarship	0.73%	\$18.26
Washington	1999-2006	Washington PROMISE Scholarship	0.15%	\$9.94

Sources: Dynarski (2004), Heller (2004), Hawley and Rork (2012), Passty (2012), the Brookings Institution, and state agency websites. FTE and Expenditure are from NASSGAP Annual Reports; Awardees are from the Brookings Institution.

a. 2003-04 data since more recent data are not available. NA: Not Available.

TABLE 2: Summary Statistics for Strong, Weak and Non-Merit Birth States, 2000-2010 Census/ACS

	Strong Merit		Weak Merit		Non-Merit	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Any College Attendance	.562	.496	.656	.475	.635	.481
Associate's Degree +	.315	.464	.392	.488	.388	.487
Bachelor's Degree +	.234	.424	.302	.459	.300	.458
Merit	.415	.493	.323	.468	.000	.000
Age	26.967	1.998	26.965	2.004	26.978	2.001
Female	.511	.500	.504	.500	.505	.500
White	.696	.460	.677	.467	.767	.422
Black	.229	.420	.131	.338	.108	.310
Hispanic	.050	.218	.136	.343	.090	.286
Asian	.005	.072	.024	.154	.012	.108
Other	.020	.140	.031	.172	.024	.152
Log Cohort Size	11.267	0.588	11.917	0.920	11.392	0.875
Unemployment Rate	5.576	1.271	5.918	1.488	4.969	1.306
Log Median Household Income	10.310	0.179	10.488	0.205	10.449	0.205
Returns to BA Degree	0.260	0.138	0.245	0.097	0.240	0.130
Total Observations	373,890		1,027,030		1,100,230	

TABLE 3: Merit Program Effects with and without Additional Demographic and State Variables

	(1)	(2)	(3)	(4)
A. Any College Attendance	-.0018 (.0055) [-.0085 .0154]	-.0026 (.0044) [-.0096 .0126]	-.0053 (.0064) [-.0119 .0021]	-.0059 (.0038) [N/A]
B. Associate's Degree or Higher	-.0016 (.0029) [-.0070 .0117]	-.0025 (.0027) [-.0074 .0115]	.0011 (.0037) [-.0036 .0070]	-.0012 (.0024) [N/A]
C. Bachelor's Degree or Higher	-.0036 (.0039) [-.0124 .0101]	-.0045 (.0034) [-.0127 .0067]	.0025 (.0033) [-.0082 .0046]	-.0034 (.0032) [N/A]
State of Birth Dummies	Yes	Yes	Yes	Yes
Year of Birth Dummies	Yes	Yes	Yes	Yes
Age Dummies	Yes	Yes	Yes	Yes
Sex, Race/Ethnicity Dummies	Yes	Yes	Yes	Yes
Cohort Size, Unemployment, Med. HH Income, Returns to BA	No	Yes	Yes	Yes
State of Birth*Year of Birth Trends	No	No	Yes	No
Region*Year of Birth Fixed Effects	No	No	No	Yes
Strong Merit States	Treatment	Treatment	Treatment	Treatment
Weak Merit States	Excluded	Excluded	Excluded	Excluded
Non-Merit States	Control	Control	Control	Control
Ages Included	24-30	24-30	24-30	24-30
Years Included	2000-2010	2000-2010	2000-2010	2000-2010

Notes: Standard errors in parentheses are clustered by state of birth. Conley-Taber 95% confidence intervals are in brackets; these are not computed for column 4 because there are too few non-merit states as controls.

TABLE 4: Merit Program Effects for Different Treatment and Control States

	(1)	(2)	(3)	(4)	(5)
A. Any College Attendance	-.0026 (.0044) [-.0096 .0126]	-.0025 (.0028) [-.0040 .0095]	.0012 (.0039) [-.0050 .0147]	-.0081 (.0052) [N/A]	.0005 (.0029) [N/A]
B. Associate's Degree or Higher	-.0025 (.0027) [-.0074 .0115]	-.0008 (.0028) [-.0028 .0120]	-.0005 (.0025) [-.0060 .0142]	-.0013 (.0034) [N/A]	.0009 (.0031) [N/A]
C. Bachelor's Degree or Higher	-.0045 (.0034) [-.0127 .0067]	-.0033 (.0024) [-.0063 .0068]	-.0011 (.0034) [-.0086 .0122]	-.0058 (.0030)* [N/A]	.0004 (.0025) [N/A]
State of Birth Dummies	Yes	Yes	Yes	Yes	Yes
Year of Birth Dummies	Yes	Yes	Yes	Yes	Yes
Age Dummies	Yes	Yes	Yes	Yes	Yes
Sex, Race/Ethnicity Dummies	Yes	Yes	Yes	Yes	Yes
Cohort Size, Unemployment, Med. HH Income, Returns to BA	Yes	Yes	Yes	Yes	Yes
Strong Merit States	Treatment	Treatment	Treatment	Treatment	Treatment
Weak Merit States	Excluded	Treatment	Control	Excluded	Treatment
Non-Merit States	Control	Control	Control	Excluded	Excluded
Total States	35	51	51	9	25
Ages Included	24-30	24-30	24-30	24-30	24-30
Years Included	2000-2010	2000-2010	2000-2010	2000-2010	2000-2010

Notes: Standard errors in parentheses are clustered by state of birth. Conley-Taber 95% confidence intervals are in brackets; these are not computed for columns 4 and 5 because there are no states included that did not implement a policy change.

*Significant at 10% based on standard errors clustered by state of birth.

TABLE 5: Merit Program Effects Accounting for Measurement Error in Treatment Status

	(1) Excluding "Marginal" Birth Cohorts	(2) Using Probability of Living in Birth State	(3) Accounting for Both
A. Any College Attendance	-.0032 (.0058) [-.0116 .0156]	-.0025 (.0058) [-.0112 .0166]	-.0028 (.0075) [-.0129 .0211]
B. Associate's Degree or Higher	-.0054 (.0043) [-.0120 .0141]	-.0031 (.0035) [-.0100 .0140]	-.0065 (.0056) [-.0150 .0187]
C. Bachelor's Degree or Higher	-.0067 (.0037)* [-.0176 .0082]	-.0056 (.0045) [-.0171 .0109]	-.0082 (.0049) [-.0213 .0109]
State of Birth Dummies	Yes	Yes	Yes
Year of Birth Dummies	Yes	Yes	Yes
Age Dummies	Yes	Yes	Yes
Sex, Race, and Ethnicity Dummies	Yes	Yes	Yes
Cohort Size, Unemployment, Median Household Income, Returns to BA	Yes	Yes	Yes
Strong Merit States	Treatment	Treatment	Treatment
Weak Merit States	Excluded	Excluded	Excluded
Non-Merit States	Control	Control	Control
Ages Included	24-30	24-30	24-30
Years Included	2000-2010	2000-2010	2000-2010

Notes: Standard errors in parentheses are clustered by state of birth. Conley-Taber 95% confidence intervals are in brackets. Column (1) excludes from the sample persons who were age 18 in the year of or year before the merit program was implemented in their birth state. Column (2) measures merit exposure by the predicted probability of going to high school in a merit state based on state of birth. See text for further details.

*Significant at 10% based on standard errors clustered by state of birth.

TABLE 6: Merit Program Effects by Sex and Race/Ethnicity

	(1) White Non-Hispanic Men	(2) White Non-Hispanic Women	(3) Non-White or Hispanic Men	(4) Non-White or Hispanic Women
A. Any College Attendance	-0.0059 (.0101) [-.0200 .0126]	.0066 (.0061) [-.0048 .0279]	-0.0199 (.0063)*** [-.0548 .0261]	-.0097 (.0067) [-.0387 .0534]
B. Associate's Degree or Higher	-.0021 (.0066) [-.0122 .0204]	-.0100 (.0052)* [-.0239 .0105]	.0066 (.0087) [-.0202 .0359]	-.0045 (.0071) [-.0338 .0299]
C. Bachelor's Degree or Higher	-.0084 (.0056) [-.0229 .0170]	-.0085 (.0048)* [-.0242 .0101]	-.0040 (.0086) [-.0188 .0301]	.0023 (.0048) [-.0306 .0412]
State of Birth Dummies	Yes	Yes	Yes	Yes
Year of Birth Dummies	Yes	Yes	Yes	Yes
Age Dummies	Yes	Yes	Yes	Yes
Race and Ethnicity Dummies	N/A	N/A	Yes	Yes
Cohort Size, Unemployment, Med. HH Income, Returns to BA	Yes	Yes	Yes	Yes
Strong Merit States	Treatment	Treatment	Treatment	Treatment
Weak Merit States	Excluded	Excluded	Excluded	Excluded
Non-Merit States	Control	Control	Control	Control
Ages Included	24-30	24-30	24-30	24-30
Years Included	2000-2010	2000-2010	2000-2010	2000-2010

Notes: Standard errors in parentheses are clustered by state of birth. Conley-Taber 95% confidence intervals are in brackets.

*Significant at 10% based on standard errors clustered by state of birth; ***Significant at 1%.

TABLE 7: Merit Effects by State of Birth

	Any College Attendance	Associate's Degree +	Bachelor's Degree +
Alaska	-.0281 [-.0497 -.0050]	-.0060 [-.0228 .0109]	.0095 [-.0079 .0389]
Arkansas	-.0162 [-.0852 .0181]	-.0098 [-.0406 .0201]	.0051 [-.0728 .0365]
California	.0019 [-.0314 .0305]	-.0088 [-.0330 .0241]	-.0143 [-.0416 .0299]
Florida†	-.0105 [-.0282 .0172]	-.0074 [-.0196 .0271]	-.0109 [-.0341 .0240]
Georgia†	.0089 [-.0468 .0331]	.0030 [-.0224 .0359]	.0135 [-.0446 .0400]
Idaho	-.0057 [-.0355 .0234]	-.0434 [-.0684 -.012]	-.0325 [-.0578 .0112]
Illinois	.0048 [-.0185 .0260]	.0089 [-.0055 .0281]	.0056 [-.0106 .0368]
Kentucky†	.0147 [-.0075 .0378]	.0047 [-.0092 .0245]	-.0090 [-.0251 .0218]
Louisiana†	.0012 [-.0296 .0286]	-.0012 [-.0181 .0234]	-.0021 [-.0234 .0236]
Maryland	.0022 [-.0487 .0423]	.0256 [-.0108 .0701]	.0215 [-.0096 .0752]
Michigan	-.0077 [-.0347 .0152]	.0059 [-.0244 .0260]	.0042 [-.0396 .0389]
Mississippi	-.0051 [-.0168 .0326]	.0031 [-.0204 .0559]	-.0100 [-.0333 .0273]
Missouri	-.0080 [-.0248 .0205]	-.0080 [-.0204 .0263]	-.0150 [-.0394 .0190]
Nevada†	-.0093 [-.0356 .0136]	.0064 [-.0309 .0200]	-.0038 [-.0534 .0242]
New Jersey	.0004 [-.0178 .0277]	.0074 [-.0069 .0399]	.0007 [-.0252 .0339]
New Mexico†	-.0146 [-.0178 .0277]	-.0126 [-.0278 .0187]	-.0076 [-.0330 .0253]
New York	-.0087 [-.0251 .0199]	.0026 [-.0113 .0367]	-.0020 [-.0262 .0327]
North Dakota	-.0095 [-.0255 .0119]	.0165 [-.0073 .0511]	.0059 [-.0203 .0474]
Oklahoma	.0025 [-.0115 .0386]	.0044 [-.0178 .0590]	-.0041 [-.0273 .0330]
South Carolina†	-.0053 [-.0341 .0239]	-.0106 [-.0264 .0148]	-.0109 [-.0309 .0146]
South Dakota	-.0254 [-.1724 .0308]	.0494 [-.0242 .1399]	.0213 [-.0350 .1166]
Tennessee†	-.0002 [-.0855 .0428]	-.0042 [-.0393 .0448]	-.0015 [-.0409 .0756]
Utah	-.0120 [-.0366 .0088]	-.0325 [-.0485 -.0148]	-.0255 [-.0432 .0039]
Washington	-.0228 [-.0450 -.0001]	-.0244 [-.0387 -.0050]	-.0202 [-.0378 .0092]
West Virginia†	.0017 [-.0462 .0461]	.0303 [-.0037 .0778]	.0235 [-.0076 .0797]

Notes: Regressions include dummies for state of birth, year of birth, age, sex, race, and ethnicity and the additional state controls. Conley-Taber 90% confidence intervals are in brackets. †denotes states with strong merit aid programs.

TABLE 8: Summary Statistics for Selected Variables in USG Data

	1990-1991 Cohort		1995-1996 Cohort	
	Mean	S.D.	Mean	S.D.
Assoc. or Bach. Degree within 4 Years	.307	.461	.312	.463
Assoc. or Bach. Degree within 5 Years	.391	.488	.399	.490
Assoc. or Bach. Degree within 6 Years	.432	.495	.440	.496
Assoc. or Bach. Degree within 12 Years	.492	.500	.506	.500
Bachelor's Degree within 4 Years	.249	.432	.266	.442
Bachelor's Degree within 5 Years	.334	.472	.355	.478
Bachelor's Degree within 6 Years	.374	.484	.395	.489
Bachelor's Degree within 12 Years	.432	.495	.458	.498
Female	.537	.499	.563	.496
Black	.207	.405	.250	.433
Hispanic	.008	.087	.013	.112
Asian	.020	.140	.024	.154
Native American	.002	.045	.002	.044
SAT (Verbal + Math)	960.1	178.3	982.5	182.8
High School GPA	2.671	0.664	2.866	0.642
Number of Observations	43,642		49,741	

TABLE 9: Effects of Post-HOPE Dummy on Degree Completion in the USG

	(1)	(2)	(3)	(4)
<u>A. Associate's or Bachelor's Degree</u>				
By Four Years After High School	.0047 (.0048)	.0102 (.0034)*	-.0093 (.0042)	-.0388 (.0040)***
By Five Years After High School	.0079 (.0054)	.0127 (.0037)**	-.0085 (.0053)	-.0421 (.0055)***
By Six Years After High School	.0081 (.0061)	.0126 (.0052)*	-.0087 (.0060)	-.0426 (.0049)***
By Twelve Years After High School	.0137 (.0055)*	.0180 (.0046)**	-.0033 (.0058)	-.0349 (.0054)***
<u>B. Bachelor's Degree</u>				
By Four Years After High School	.0168 (.0052)**	.0197 (.0030)***	-.0005 (.0042)	-.0269 (.0044)***
By Five Years After High School	.0207 (.0061)**	.0231 (.0038)***	.0012 (.0059)	-.0300 (.0064)**
By Six Years After High School	.0212 (.0062)**	.0236 (.0048)**	.0015 (.0059)	-.0303 (.0052)**
By Twelve Years After High School	.0258 (.0068)**	.0280 (.0053)**	.0057 (.0067)	-.0243 (.0062)**
Sex, Race, Ethnicity Dummies	No	Yes	Yes	Yes
High School Dummies	No	Yes	Yes	Yes
SAT Dummies	No	No	Yes	Yes
High School GPA Dummies	No	No	No	Yes

Notes: Regressions include 93,383 total observations for four cohorts of recent high school graduate first time freshmen. Standard errors in parentheses are clustered by high school graduation year.

*Significant at 10% based on small sample t-distribution; **Significant at 5%; ***Significant at 1%.