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Abstract

One goal of state merit-based financial aid programs is to increase the stock of college-educated labor in the state by retaining college-educated persons in the state after college. However, there has been surprisingly little research on whether state merit aid programs are effective at this goal. This paper investigates the effect of state merit aid programs on the post-college location of 24-30 year olds. We use decennial census and American Community Survey microdata to consider post-college retention effects in the 25 states that implemented merit aid programs between 1991 and 2004. Our preferred specification implies that strong state merit aid programs on average increase the probability that a college attendee lives in his or her birth state during ages 24-30 by 2.76 percentage points. We also estimate the effect for individual states and find meaningful differences across states in the effect of merit aid programs on in-state post-college retention and explore explanations for these differences.

JEL Codes: I23, J24, R23

Keywords: college education; merit aid; migration; retention; brain drain

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1. Introduction

Since the early 1990s new merit-based student financial aid programs have been created in more than two dozen states. These programs award college scholarships to in-state students who meet a merit requirement based on high school GPA and sometimes SAT/ACT score. Several studies have investigated the effects of merit aid programs on education outcomes such as the probability that a high school student will attend college and the percentage of students who stay in-state to go to college; see Dynarski (2000, 2002, 2004, 2008) and Cornwell, Mustard, and Sridhar (2006). But there also appears to be a desire for merit aid programs to increase the quality of the workforce, in part by retaining recent college attendees in the state after they complete their college education (Groen 2011). If merit aid scholars leave the state upon completing their undergraduate studies, the state's return on its expenditure is reduced; higher educated workers pay more in taxes and impose less cost on the public sector for services (Trostel 2010), and attracting skilled workers may result in higher economic growth rates (Glaeser and Saiz 2004; Moretti 2004; Florida et al. 2008), lower unemployment rates (Winters 2013) and perhaps a better quality of life (Shapiro 2006; Winters 2011a).¹

Despite this interest in post-college retention of merit aid students, there has been little research conducted on the subject. This paper attempts to help fill this gap by exploring the effect of merit-based aid on post-college in-state retention using data from the 25 states that adopted merit aid programs between 1991 and 2004.

Studies of the location decisions of college graduates such as Perry (2001) find that students are more likely to live in the state in which they attend college. One explanation is that students may develop location-specific networks with friends and employers while in college and

¹ Additionally, parent-voters may be particularly interested in which college-educated workers reside in the state, i.e., they may want their college-educated children to stay in the state.

these may make them more likely to stay in the area after college.² However, students may choose to go to college in the state where they expect or hope to reside after completing college. Thus, measuring the effect of attending college in an area on a student's post-college residence requires controlling for this potential endogeneity. Groen (2004) controls for endogeneity of college decisions and finds that there is a modest magnitude causal effect of attending college in a state on the likelihood of living in that state after graduation. However, he does not examine the effects of state merit aid programs on post-college retention.³

Merit aid programs likely increase the probability that a student goes to college in-state rather than out-of-state (Dynarski 2004; Cornwell et al. 2006; Orsuwan and Heck 2009; Zhang and Ness 2010). The hope is that encouraging students to stay in-state for college will also encourage them to stay in-state after college as they begin their careers. However, it is possible that a student who attends college in-state because of a merit-based scholarship program might be almost or even just as likely to live out-of-state after college as she would have been had she gone to college out-of-state. Additionally, many students would have gone to college in-state even in the absence of a merit aid program, and for them there is no obvious link between being given a merit aid scholarship and living in-state post college. Thus, whether a merit aid program can increase the stock of college educated workers in a state by affecting post-college location decisions is ultimately an empirical question.

To the best of our knowledge Hickman (2009), Sjoquist and Winters (2013), and Hawley and Rork (2013) are the only published papers that address the effect of a merit-based

² Winters (2011b) suggests that the widely documented correlation between local human capital levels and future population growth of U.S. metropolitan areas is largely driven by recent college attendees staying in the area where they attended college.

³ Malamud and Wozniak (2012) examine the effects of college attendance in any state on the probability of out-migration from one's native state, instrumenting for college attendance using variation in the risk of induction through the military draft during the Vietnam War. They find causal evidence that college education increases out-migration, but their study does not focus on the effects of where the education was received.

scholarship program on post-college retention.⁴ Hickman (2009) investigates the effect of the introduction in 1997 of Florida's merit-based scholarship program on the post-college retention of students in Florida. He uses the 2000 Census of Population and the American Community Survey (ACS) for 2001 through 2006 to construct a treatment group and a control group. The treatment group consists of anyone born in Florida who was 18 years of age in 1997 or later, and thus are assumed to have been exposed to the treatment, while the control group consists of individuals born in Florida who were age 18 in 1996 or earlier. He considers individuals between 23 to 27 years of age who are not in school or the military. Hickman's dependent variable is whether the individual resides in Florida after college. His analysis is essentially a difference-in-differences model, comparing individuals with no college to those with at least some college, both pre- and post-merit aid program adoption. Thus, he includes both the treatment dummy and its interaction with a dummy measuring whether the individual has any post-secondary education. The coefficient on the treatment dummy is not statistically significant, but the coefficient on the interaction term is. He finds that the Florida scholarship program increased the probability that a 23 to 27 year old with some college located in Florida by 3.4 percentage points.

Sjoquist and Winters (2013) use student administrative records from the University System of Georgia (USG) matched with employment records from the Georgia Department of Labor to explore the effect of Georgia's HOPE Scholarship program on post-college retention of

⁴ In a recent working paper, Fitzpatrick and Jones (2012) investigate the effect of merit aid on post-college migration, as well as on educational attainment. They employ an approach that is similar to ours and find that merit aid increases the probability of living in one's state of birth by one percentage point for their sample that includes non-college attendees. There are important differences between their paper and the current paper, such as: we examine the set of 25 states with merit aid while Fitzpatrick and Jones use 15; we distinguish between strong and weak merit aid programs but Fitzpatrick and Jones do not; we provide estimates by state-of-birth, Fitzpatrick and Jones do not; both papers provide estimates unconditioned on education but our main estimates focus on college attendees; we address measurement error in exposure due to not graduating at age 18 and going to high school in a non-birth state, Fitzpatrick and Jones do not.

USG students in the Georgia workforce. They measure post-college retention by whether a student is employed in Georgia X years after first enrolling in college, where X varies from 4 to 12 years. They find that HOPE reduced the percentage of high ability students in the USG who are employed in Georgia several years after college, but there was no meaningful difference for low ability students. They interpret this to suggest that HOPE kept many high quality students in-state for college but many of these students left the state after college. Sjoquist and Winters (2013) also conduct a secondary analysis that uses census and ACS data for Georgia similar to Hickman's (2009) analysis for Florida. They find small and statistically insignificant effects for Georgia.

Hawley and Rork (2013) estimate the effect of merit aid programs on out-migration of college-educated adults. They use microdata from the decennial census to calculate five-year out-migration rates for each state for 1980, 1990, and 2000 and use the ACS to calculate one-year out-migration rates for each state for 2005-2009 which they combine into one migration rate to approximate the 5-year migration flow. They consider the migration rate for five groups: the entire population, the entire college educated population, and the college educated population aged 22-25, 22-34 and 35-65. Since a merit aid program could have been adopted during the five year migration period, they define merit exposure using "windows" based on time elapsed since the program was adopted. They find that younger adults are more likely to migrate from states with a merit aid program while older adults are less likely, resulting in no net effect of a merit aid program on the state out-migration rate.⁵

⁵ Our research is related to that of Hawley and Rork (2013), but there are several important differences. First, they focus on the recent flow of college educated persons while we focus on the probability of college-educated persons residing in their state of birth. Second, their analysis is complicated by having to combine five-year migration rates in the census with one-year migration rates in the ACS. Our retention variable is defined the same throughout our sample period. Third, they use all 21 merit aid programs that they identified regardless of how extensive the programs are, while our analysis focuses on 9 states with "strong" merit programs, although not exclusively. Fourth, we use annual data and allow for yearly variations in the merit variable, while Hawley and Rork use "windows" to

Hickman considers only one state, but 25 states adopted merit-based financial aid programs between 1991 and 2004 (see Table 1). In this paper we follow Hickman's basic approach but expand the analysis to all states with a merit aid program and include non-merit states in the comparison group. We seek to address whether the positive effects on post-college retention found by Hickman are unique to Florida or if they generalize to other merit-adopting states. This is an important question with implications for both researchers and policymakers.

Merit aid programs differ in many ways, including the size of the award and the number of students who are awarded scholarships. We identified 9 programs as being much more significant; we classify these as "strong" merit aid programs. 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. As can be seen, the 9 programs have significantly larger participation rates and larger average awards. The classification for three states perhaps needs some explanation. 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. Therefore, we examine the effects of the 16 weaker merit aid programs but focus on the 9 stronger programs.

Our baseline estimates suggest that strong state merit aid programs on average increase the probability that a college educated person resides in their state of birth after college by 2.76

account for merit aid programs that could have been adopted part way through the five-year period over which migration is measured. Finally, their treatment is whether the merit aid program existed during the period of migration, not whether the individual was eligible for merit aid. We compare individuals who were potentially eligible for merit aid to those who were not. So while our results could be similar to theirs, it would not be surprising if the results differed.

percentage points. However, our results suggest that there are heterogeneous effects across states even among those with strong merit aid program. Furthermore, the effects for the weak merit aid states are on average much smaller.

The rest of the paper proceeds as follows. In the next section we describe the data we use and our empirical approach. Section 3 presents our results. Section 4 contains a summary and some concluding remarks.

2. Empirical Framework

2.1. Data

We use public use microdata samples (PUMS) from the 2000 decennial census long form questionnaire and 2001-2010 American Community Survey (ACS) available at IPUMS (Ruggles et al. 2010). We use Census provided weights in our analysis, so that each year is given roughly equal weight.⁶ For our baseline analysis we restrict the sample to persons between ages 24 and 30 who have attended at least some college, are not currently in college or the military, and have unimputed information for age, education, and state of birth. Persons who have never attended college are excluded for our baseline model because our interest is in how merit aid programs affect post-college location decisions, and they by definition do not face a post-college location decision; however, we also explore dropping this restriction. Similarly, persons currently in college or the military face very different location decisions than civilians who have finished their education and are thus excluded. Imputed values for non-respondents are subject to considerable measurement error, and including those with imputed values would likely attenuate coefficient estimates toward zero.

⁶ The weights in each sample sum to the total population in that year. We use both the 5% and the 1% PUMS for 2000 and therefore reweight the 2000 PUMS to avoid giving that year double weight. The results below are qualitatively robust to not using weights.

We include several state of birth control variables measured at multiple points in time. These include three measures of state macroeconomic conditions in a given year computed using persons ages 38-54 in the March Current Population Survey (CPS): the unemployment rate, the college wage premium, and the percentage of jobs in white collar occupations. The age range is chosen to exclude older persons who may be nearing retirement and to exclude younger persons who may have been exposed to a state merit aid program. Our regressions include each of these variables measured at five points in time: the year an individual was age 18, the year an individual was age 22, the survey year, the year before the survey year, and the year two years before the survey year. Most individuals make initial college enrollment decisions at age 18, and state economic conditions could influence whether they enroll in-state, out-of-state, or not at all, which could affect post-college location decisions. Economic conditions at age 22 are intended to control for conditions a college attendee would likely have faced about the time they are finishing their undergraduate education. Strong economic conditions could encourage them to start careers in their native states and a weak state economy could do the opposite. Even after their initial location decisions, recent local economic conditions (e.g. during the survey year and the two previous years) could alter their location decisions, with better economic conditions making them more likely to live in their native state.

We also include two additional state of birth control variables measured only at age 18. We include the log of the cohort size to reflect the extent of peer competition. Larger cohorts create greater competition for both higher education and jobs and may encourage more of the cohort to leave their native state in search of better opportunities. Finally, we include a control for the percentage of an individual's age cohort at age 18 that is foreign born to allow foreign born competition to have a differing effect than native born competition reflected in the cohort

size variable. Importantly, both cohort size and the percentage of a cohort that is foreign born are potentially affected by college and post-college location decisions, so we do not measure these variables after age 18.

2.2. Research Design

We follow a treatment and control research design. The treatment group includes persons who were exposed to a state merit-based financial aid program when graduating high school and the control group consists of persons who were not. Thus, we measure the effect of the intent to treat. Unfortunately, we do not know at what age or in what state an individual graduated high school, and thus we follow previous literature (e.g. Dynarski 2008 and Hickman 2009) and assign an individual to the merit program treatment group if she was born in a state that adopted a merit program and turned 18 after the program was implemented; if not she is part of the control group. The control group, therefore, includes both individuals born in states that never adopted merit aid programs and individuals born in merit-adopting states but turning 18 before the program was implemented. Because some individuals do not graduate high school at age 18 and some attend high school outside their state of birth, our merit dummy variable will have some measurement error. We address measurement error due to mis-assigning individuals to treatment and control groups in more detail later.⁷

Our preferred specification for assessing the effects of state merit scholarship programs on post-college retention is to compare the 9 states with strong merit aid programs to states with no merit program. The 16 weak program states receive only a “partial treatment” compared to

⁷ These merit programs were initially targeted to recent high school graduates, but some programs did eventually allow non-traditional students to qualify if they met certain criteria. This creates an additional source of measurement error in our merit variable because some individuals in the pre-merit cohorts may have actually received merit aid. We believe the extent of measurement error from eligibility for non-traditional students is small, but it could further attenuate coefficient estimates toward zero.

the “full treatment” of strong program states and are less likely to have large impacts on educational outcomes. The purest test is to exclude weak merit aid states from the analysis and estimate the effect of state merit aid 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 aid states in the analysis. Additionally, we restrict the comparison states for each merit state to non-merit states in the same census region since states in different regions may experience very different migration patterns. We also exclude the District of Columbia given its small size and uniqueness, but results are robust to including it as a state.

We estimate a linear probability model (LPM) as follows:

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

Where Y_{isct} equals one if individual i born in birth cohort c lives in her state of birth s in year t , Γ_s includes state of birth fixed effects, Π_c includes region-specific 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 control variables discussed previously⁸, 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 birth cohort fixed effects allow the model to be interpreted as a difference-in-differences model identified by differences across birth states (within the same region) and across birth cohorts within birth states.⁹ Thus, our analysis is essentially based on comparing the differences in the pre- and post-merit means in the treatment states with the differences in means in the comparison states at the same time. The key identifying assumption

⁸ In results not shown, we also consider the robustness of our baseline specification to altering the state control variables and including state-of-birth-specific trends for year of birth. The results are qualitatively robust to several alternative specifications. These are available by request.

⁹ Our sample for the baseline model is restricted to those with at least some college, and thus we do not interact the merit aid dummy with a college education dummy as is done by Hickman (2009) because the college dummy equals one for all individuals in our sample; an interaction term would be perfectly collinear with the merit dummy.

is that the time series for the comparison states provides an accurate counterfactual for what would have occurred in the treatment states had they not adopted merit aid. The models are estimated via Ordinary Least Squares (OLS) but results are robust to using probit or logit. Summary statistics for the individual level variables are reported in Table 2 separately for strong merit, weak merit, and non-merit birth states. Appendix Table A provides state means for a few migration and education outcomes from the 1990 census to give an idea about what each state looked like before the spread of merit aid.

3. Empirical Results

3.1. *Basic Results*

The first column of Table 3 presents our baseline results for the effect of state merit-based financial aid programs on whether an individual resides in his or her state of birth post-college.¹⁰ This specification excludes persons born in weak merit aid states from the analysis, and also excludes persons born in the Northeast and Midwest census regions who are part of neither the treatment nor the control group since strong merit states are only in the South and West and the control-group is region-specific. The second column examines the robustness to including the Northeast and Midwest non-merit states in the analysis; these states have no direct effect on the merit coefficient, but can have an indirect effect on the merit coefficient by affecting the coefficients for the individual and state control variables. The third column again excludes Northeastern and Midwestern states but treats the South and West as a combined region by replacing the region-specific birth year dummies with a common set of birth year dummies. The fourth column alters the first column by including weak merit states in the South and West

¹⁰ We only report the coefficient on the merit aid dummy variable; full regression results are available from the authors.

in the control group. The fifth column assigns weak merit states to the treatment group and excludes strong merit states from the analysis; weak merit states are located in all four census regions, so the last column includes the 16 weak merit states and all 25 non-merit states.

The baseline specification (column 1) yields a coefficient of 0.0276 that is statistically significant at the 1% level based on standard errors clustered by state. Given that 64 percent of our sample lives in their birth state during ages 24-30, an increase of 2.76 percentage points is not incredibly large, but it is certainly non-trivial. The coefficient in column 2 increases slightly to 0.0286, but as expected the difference is small. The coefficient is slightly reduced to 0.0251 in column 3 and to 0.0225 in column 4, but there are important reasons discussed above why these are not the preferred specification; still, it is useful to know that these modifications do not substantially alter the merit coefficient.¹¹ The fifth column, which reports the effect of weak merit aid programs, does produce fundamentally different results. The weak merit coefficient is only 0.0059 and is not statistically significant. This confirms our earlier expectation that strong and weak merit aid programs have very different effects and should be considered separately.

3.2. Considering Selection Effects and Alternative Samples

One potential concern is that the state merit aid programs could affect the variables that we use to define our main sample and cause the pre- and post-policy composition of our sample to differ. In particular, one might be concerned that merit aid could affect the percentage of individuals who attend college. If merit aid increases attendance and the induced attendees have

¹¹ One potential concern with the analysis is that clustered standard errors could be downwardly biased because we use a relatively small number of treatment and control states (Bertrand et al. 2004; Donald and Lang 2007; Cameron et al. 2008). Conley and Taber (2012) suggest an alternative inference procedure for policy analyses based on the distribution of residuals across states that performs better than clustered standard errors when there are very few policy states. Our ability to use their procedure is complicated by our use of region-specific comparison groups. However, we estimated a modified version of their procedure for the specifications in Table 3 and found that the first four columns were statistically significant, while the fifth was not. We interpret this to suggest that the clustered standard errors in Table 3 are reasonably accurate.

unobserved characteristics that systematically differ from those who would have attended college either way, then the post-policy treatment group will differ from the pre-policy control group due to selection on unobservables. If the unobservables are correlated with the outcome variable, a simple analysis that ignores selection issues could produce biased results of the effects of merit aid on post-college retention.¹²

Our analysis and that in previous work confirm that selection is not a problem. First, Sjoquist and Winters (2012) conduct a similar analysis of college attainment and find that merit aid has no meaningful effect on the likelihood that an individual ever attends college.¹³ To consider selection effects more thoroughly, we also examined the effects of merit aid on our sample exclusion criteria individually and jointly in results not shown but available by request. The results are very consistent; merit aid has a small and statistically insignificant effect on each of the selection criteria. These results imply that our selection criteria do not affect the pre- and post-policy composition of our main sample.

We also explore how merit aid affects the probability of living in one's state of birth for alternative samples. Consider the "full sample" that includes both our main sample and the sample of excluded individuals ages 24-30. The first column of Table 4 examines the effect on the "full sample" and the second considers the effect on the sample of individuals excluded from the main sample; both are otherwise similar to the baseline specification. The "full sample" coefficient is 0.0142 and statistically significant at the 10% level, and the excluded sample coefficient is 0.0041 and not statistically significant. As expected, the results for our excluded

¹² However, merit-induced increases in college attendance could also increase the quality of the state's workforce, especially if the marginal attendees are otherwise more likely to stay in the state post-college. We find no effect on attendance and this effect is not the focus of our study, but given that others have found such an effect (e.g. Dynarski 2008) the possibility should certainly be considered when evaluating the overall benefits of merit aid programs.

¹³ This result may seem surprising, but the implication seems to be that merit aid has no effect on ever attending college because students meeting merit eligibility requirements are very likely to attend college anyway.

sample strongly differ from those of our main sample; merit aid does not affect the location decisions of those that we exclude. Combining the main and excluded samples gives a coefficient estimate for the “full sample” that is roughly half as large as that for our main sample. Our interest is in the effects of merit aid on post-college retention, and we believe that our exclusion criteria are warranted and justified.

We also consider whether the effects of merit aid on post-college retention differ between college attendees leaving school without a bachelor’s degree and those who complete a bachelor’s degree. The third and fourth columns of Table 4 examine the effects of merit aid on post-college retention for these two subsamples of our main sample. The coefficient estimates have very similar magnitudes of 0.0279 and 0.0255, respectively, and both are statistically significant. Thus, there is not much difference in the retention effects for college graduates and non-graduate attendees.

3.3. Accounting for Measurement Error in Merit Exposure

We next try to account for measurement error in our merit exposure variable. We previously assumed that a person was exposed to a state merit aid program if he or she was born in a merit aid state and reached age 18 after the merit program was implemented in the state. We assigned persons who were age 18 or younger when the merit program in their state of birth began to the treatment group and assigned persons who were age 19 or older when the program began to the control group. Age 18 was chosen as the cutoff because it is the age at which a typical student graduates high school, but not everyone graduates high school at 18. Many graduate at younger or older ages, so our merit program treatment variable will be measured with error for some persons graduating high school at an age other than 18 and around the time a

merit program was implemented in their state. Furthermore, some persons leave their birth state before graduating high school and attend high school in a different state, but we do not observe where they attended high school.

We first account for measurement error in merit exposure resulting from students finishing high school before or after age 18. In column 1 of Table 5 individuals ages 18 or 19 when the merit program was implemented in their birth state are excluded from the analysis since these “marginal” birth cohorts are the ones for which merit exposure is most likely to be measured with error. Results (not shown) that also exclude persons who were ages 17 or 20 are similar. The coefficient estimate in column 1 of Table 5 is 0.0308, which is slightly larger than the coefficient of 0.0276 for the baseline specification (column 1 of Table 3).

We next account for measurement error in our merit variable that results from students going to high school outside their birth state. We use a procedure similar to one suggested by Dynarski (2008) that measures merit exposure using the predicted probability of attending high school in one’s birth state. For each state we compute the predicted probability of living in one’s birth state for 15-17 year olds in the 1990 Census (reported in the first numerical column in Appendix Table A). The predicted probabilities are then multiplied by the merit dummy variable to predict the probability of being exposed to the state’s merit aid program. We then estimate our LPM equation for our sample of 24-30 year olds but replacing the merit dummy variable with the predicted merit variable. The second column of Table 5 reports the results. The coefficient estimate increases to 0.0349.

Column 3 of Table 5 accounts for measurement error in merit exposure combining the procedures adopted in the first two columns. Column 3 uses the predicted merit variable in column 2 and excludes individuals ages 18 or 19 when a merit program was implemented in their

state of birth. The coefficient is now 0.0390. Accounting for measurement error, therefore, increases the merit coefficient somewhat. Though the adjusted estimate is not statistically significantly different from the baseline coefficient estimate, this measurement error exercise suggests that the actual effect of merit aid on post-college retention is likely larger than that in the baseline specification.

3.4. Differing Effects by Time from Policy Adoption

We next follow an approach similar to an event-style analysis, but instead of using single years,¹⁴ we combine years before and after merit aid adoption into six groups: 1-4 years before adoption, 5-8 years before adoption, 9 or more years before adoption, 1-4 years after adoption, 5-8 years after adoption, and 9 or more years after adoption. We then interact each of these time periods with a dummy equal to one if the state ever adopted a strong merit program but excluding the period 1-4 years before adoption to make it the omitted based group. This analysis serves two main purposes. First, it allows us to examine whether the merit effects differ by the number of years since the policy was adopted. For example, the effects of merit aid on post-college retention might increase or decrease over time after the policy is adopted as students learn more about the program and alter their behavior in response. Second, this approach allows us to directly look for pre-policy trends. The results of this analysis are in Table 6.

We see that the coefficients for the post-policy year groups are fairly consistent at 0.0285, 0.0294, and 0.0257, respectively, suggesting that the effect of merit aid on post-college retention does not depend on the number of years since policy adoption. Importantly, we also find that the coefficients for the pre-policy year groups are small and not statistically significant. This confirms that pre-policy trends are relatively unimportant. One can also use this analysis to

¹⁴ We also examined effects by single year but these were noisier and more difficult to interpret.

conduct a placebo test by pretending as if the merit programs were actually adopted four years earlier and comparing the differences between the 1-4 years before the policy to the 5-8 years before the policy. Since there was no merit aid during the pre-policy years, we should see no meaningful difference, which is exactly what we find with the small and statistically insignificant coefficient for 5-8 years before the policy.^{15,16}

3.5. Separate Effects by State of Birth

We next estimate separate merit coefficients for each of the 25 merit-adopting states to investigate whether there are important differences across states. We estimate a joint regression that uses interaction terms between the merit indicator variable and state dummies for each of the 25 merit states; the 25 interaction term coefficients report the separate effects for each merit aid state.¹⁷ The regression also includes the 25 non-merit control group states. The results for individual merit aid states are reported in Table 7, which shows that there is meaningful variation in coefficient estimates, both within and across the strong and the weak merit states. The effects are statistically significant using clustered standard errors for only 11 of the 25 states. The coefficient for Florida is 0.0456, which is slightly larger than the preferred estimate of 0.0339 in Hickman (2009), no doubt due to the fact that we use a different research design and include

¹⁵ In results not shown, we also experimented with placebo tests three, four, five, and six years before the actual policy and found no significant placebo effect.

¹⁶ A related issue we considered is whether the effects differ by age. In particular, merit aid might keep some people in their native state during their early post-college years, but they could end up moving from the state as they age; merit aid might delay migration without actually stopping it. We estimated separate effects by age for persons ages 22-31 by interacting age dummies with the merit dummy. In results not presented, but available by request, we find that the coefficients are fairly consistent across ages 22-27. However, for ages 28-31 the coefficients show no pattern and are generally statistically insignificant, although they are meaningfully smaller for ages 30 and 31. However, the number of exposed states and cohorts is significantly smaller for older ages, so we cannot be very confident that the effects do indeed attenuate with age. As cohorts age and more data become available, future research should revisit this issue.

¹⁷ We also estimate separate regressions for each merit state in results not shown. The resulting merit coefficients are similar to those reported in Table 7; the simple correlation coefficient between the two sets of coefficients is 0.96. We also estimate joint regressions that include the measurement error adjustments in Table 5; the correlation between these and the estimates in Table 7 is 0.81.

more years in our sample.¹⁸ Nineteen of the states have positive coefficients and six of those have coefficients greater than three percentage points: Florida, New Mexico, West Virginia, Alaska, Idaho, and Maryland. We estimate a fairly large negative effect for South Dakota, with a statistically significant coefficient of -0.1045. However, South Dakota has only one birth cohort exposed to its merit program and is a small state with relatively few observations per cohort. Michigan is another notable exception with a moderately large negative coefficient.

There are several possible explanations for the interstate differences in the measured effect of merit-based student aid on post-college in-state retention found in Table 7. We discuss these explanations below, and where possible provide empirical evidence. Some of the potential explanations are more statistical/econometric in nature, while others focus more on differential behavioral responses.

First, while the sample size of our data is very large the PUMS is still a random sample, and thus the interstate differences in the coefficients could be partially due to sampling error. However, we believe this is likely to explain only a small portion of the differences. Many of the coefficients are relatively large while others are relatively small, and these differences are often statistically significant. Below we find evidence of systematic differences due to observable factors, which we would not expect to find if differences were just from sampling error.

Second, it is always possible that states that adopt merit aid programs contemporaneously adopt other policies that affect the in-state retention rate. And, if there are variations across states in the policies that are adopted, the measured effect of the adoption of merit aid programs could differ. For example, the adoption of a merit aid program could have come at the expense of a reduction in the state's need-based aid program (Long 2004; Steele 2007). Or universities could have expanded their job placement efforts that might have increased the probability that

¹⁸ In results not shown, we also replicated Hickman's (2009) basic results for Florida using his approach.

students would remain in-state post-college. Or, some states might have added programs that made the states' colleges more attractive to merit aid eligible students, and thus increasing the percentage that remained in-state for college. Given that going to college in a state increases the probability of living in the state post-college (Groen 2004), such policies could affect the post-college retention rate. Our search of the literature did not reveal an analysis of other policies that might affect post-college in-state retention, and we were not able to identify such policies.

3.5.1 Bivariate Correlations with State-Level Factors

We turn now to the more behavioral factors, and note that we are attempting to explain why merit aid programs have differential effects on in-state post-college retention across states, and not just why retention differs across states in general. In the regressions reported above we control for individual characteristics and state economic conditions, and include state of birth dummies, which control for time-invariant state characteristics. Thus, we seek other possible explanations for the differences in the coefficients. The existing literature is generally silent regarding the mechanisms through which merit-based aid might affect post-college retention, so we considered a number of different state-level variables that might help explain the differences. We first compute correlation coefficients between the state-specific merit aid coefficients in Table 7 and various state characteristics, with the bivariate correlation coefficients being reported in column 1 of Table 8.¹⁹ Then in the next subsection we present a simple multivariate analysis. Because South Dakota is such an outlier, we exclude it from both analyses; results including South Dakota are available by request.²⁰

¹⁹ Since we do not have a time series for the variables, we cannot include them in the regressions reported above since we have state of birth dummies in the regressions.

²⁰ As would be expected including South Dakota substantially reduces the values of the correlation coefficients.

We first consider the possibility that the effect of a merit aid program on the retention rate depends on the pre-merit aid retention rate; for example, if a state has a very high pre-merit aid retention rate, merit aid might have a very small effect on the retention rate because there is less opportunity for the retention rate to increase. To explore this we calculated the correlation between the coefficients in Table 7 and the post-college retention rate in 1990 reported in the second numerical column in Appendix Table A. The correlation coefficient is -0.513, confirming that states with lower initial retention rates experienced much larger increases in retention due to merit aid.

Another possible linkage is through the effect of merit-based aid on in-state college enrollment, in particular, that as a result of merit-aid programs some students who would have gone to college out-of-state decide to stay in-state for college. Since there is a high probability of remaining in the state in which one goes to college, this could increase the percentage of individuals who live in their birth state post college. To the extent that the effect of merit aid on in-state college enrollment differs across states, the measured effect of merit aid on post-college retention could also differ.²¹ We used enrollment data from the Integrated Postsecondary Education Data System (IPEDS) to compute for each merit aid state the effect of merit aid on the percentage of college attendees from the state enrolling in-state for college; this was done using a regression framework similar to that in Table 7 except that we only observe a given cohort at the initial point of enrollment and therefore only use the state control variables observed at age 18.²² We then correlated the IPEDS in-state enrollment coefficients with the post-college retention coefficients in Table 7 and obtained a correlation coefficient of 0.187. The expectation is that if

²¹ Both Pallais (2009) and Bruce and Carruthers (2013) find that Tennessee's HOPE Scholarship had no effect on in-state enrollment, in contrast to findings for other states. We find no effect of Tennessee's HOPE on post-college retention in the state, which is consistent with the premise that change in enrollment affects the level of retention.

²² These results are available from the authors by request.

merit aid keeps more college attendees in the state for college it would lead to more college attendees living in the state after college. The positive correlation is consistent with this view, but the magnitude of the correlation is fairly moderate, consistent with previous literature that finds that the production and stock of college-educated persons in an area are only modestly correlated (Bound et al. 2004; Abel and Deitz 2012).

A related issue is whether the effect of merit aid on post-college retention in a state depends on the effect of merit aid on college completion in the state. Sjoquist and Winters (2012) find that merit aid on average has a small insignificant effect on college completion but they report possible differences in the effects across states. Increased education has been shown to increase out-migration (Malamud and Wozniak 2012), so merit-induced increased degree completion might be expected to decrease post-college retention. However, if merit aid increases degree completion by increasing education in one's home state, then the effects could be positive because getting an education in one's home state decreases out-migration (Groen 2004). The correlation between the retention coefficients in Table 7 and the degree completion coefficients in Sjoquist and Winters (2012) is 0.181, suggesting that college completion due to merit aid may have a small positive effect on the effect of merit aid on retention. However, this correlation is very moderate and the average effect of merit aid on degree completion in Sjoquist and Winters (2012) is small, so we cannot draw strong inferences.²³

We next consider the effects of the main features of the merit aid programs. For starters, our earlier results in Table 3 suggest that the average effect of merit aid on post-college retention is larger for programs that we classify as strong than for those that we classify as weak. The unweighted average of the Table 7 coefficients is 0.0178 for strong merit programs and 0.0105

²³ Sjoquist and Winters (2012) also compute state-specific coefficients for the effect of merit aid on having attended any college. The correlation between those coefficients and the retention coefficients in Table 9 is -0.019.

for weak merit programs, excluding South Dakota. Thus, the effect is still larger for strong merit programs but the difference is not as pronounced as in Table 3.²⁴ More generally, the effects of merit-aid programs could depend on the criteria that we used to define merit programs as strong or weak, namely, aid per student and the percentage of college students who receive aid. We expect that more significant programs would lead to larger effects on retention. The correlation coefficient between the Table 7 coefficients and aid per student is only 0.090, but the correlation with recipients per student is 0.206, again providing modest support for the hypothesis that larger programs have a greater effect on post-college retention. The correlation between the retention coefficient and a dummy variable equal to one for strong merit aid states and zero otherwise is 0.166.

It is also possible that the effect of merit aid on post-college retention could differ between earlier and later adopting merit aid states. For example, first movers might experience larger effects from greater publicity surrounding the introduction of the program. To explore the effect of the timing of adoption, we calculated the correlation between the Table 7 coefficients and the year the merit program was adopted. The coefficient was -0.093, which may weakly suggest that more recently adopted merit aid programs have smaller effects on retention rates; the small effect is consistent with results in Table 6.

The effect of merit aid on post-college retention could depend on the quality of the state's colleges (and universities), and in particular, through the effect of college quality on the decision to go to college in-state or out-of-state. Higher quality colleges may result in fewer students going to college out-of-state in the absence of merit aid, so the number of students who could switch from out-of-state to in-state due to merit aid would be smaller. On the other hand, states

²⁴ Note that the results throughout the rest of the paper implicitly give greater weight to more populous states, which gives meaningfully different weighted averages than the unweighted averages discussed here.

with less selective flagship institutions may be better able to increase post-college retention via merit aid because they are better able to accommodate more moderate quality students who in the absence of merit aid would have gone to college out-of-state. However, lower quality colleges might also make it less attractive to switch from going to college out-of-state to going in-state due to merit aid, which would reduce the effects of merit aid on retention. We use three admittedly crude measures of the quality of a state's public colleges: the Peterson's ranking of the highest ranked public college in the state, the percent of students in the state's top ranked college scoring 700 or higher on the SAT math exam, and the percent scoring 700 or higher on the SAT reading exam. Interestingly, higher quality colleges are correlated with smaller effects of merit aid on post-college retention. The correlation with the Peterson's ranking was 0.254,²⁵ and the correlations with the two SAT score measures were -0.311 and -0.329, respectively, implying that increased college quality reduces the effect of merit aid on post-college retention. However, the correlation between these three measures of college quality and the 1990 retention rate are -0.87, 0.71, and 0.64, respectively, so the observed correlation between college quality and the Table 7 coefficients could be driven by their strong mutual correlation with the 1990 post-college retention rate, and thus one must be careful in drawing conclusions about the effects of college quality.

The effects of merit aid on retention could also depend on the structure of higher education institutions in a state. We next used IPEDS and state population data to compute for each state the number of public institutions per capita, the number of 4-year public institutions per capita, and the percentage of undergraduates in public institutions that are enrolled in 4-year schools. The correlations between these variables and the Table 7 coefficients are 0.281, 0.324, and 0.341, respectively. These correlations suggest that having a high density of institutions and

²⁵ Note that the top college is ranked first so that a larger ranking number indicates a lower quality college.

especially a high density of 4-year institutions might make a state's higher education system more desirable and increase the effects of merit aid on post-college retention.

Higher income households may be less responsive to the price of higher education and less responsive to financial aid. To test this we obtain median family income in 1989 for each state. The correlation between it and the Table 7 coefficients is -0.080, which is small, but the sign is consistent with the hypothesis that higher income households are less responsive to financial aid. The Table 7 coefficients are also negatively correlated with the state population in 1990 (-0.213) and the percent of the population that is urban (-0.169), but the correlations are not especially strong.

Finally, the effects of merit aid on post-college in-state retention could be affected by the quality of life in a state. For example, a high quality of life might make students less likely to leave the state for college, and thus there would be fewer students to entice to stay with merit aid. Alternatively, states with nicer location-specific amenities may be able to better leverage merit aid to keep recent college attendees in the state post-college. Merit aid may entice some students to stay in-state for college, but a poor quality of life could quickly drive them out of the state after college. However, individuals are likely more reluctant to leave states with amenities that they highly value. Keeping young people in-state for college may translate into keeping them in-state after college if the state offers desirable amenities and staying in-state for college strengthens an individual's preferences for the state's amenities. It seems reasonable that most new high school graduates have had limited opportunity and freedom to explore their home state's quality of life and thus may not fully appreciate it. Going to college gives them greater freedom and staying in-state for college increases their exposure to the state's amenities, which if desirable, could increase their appreciation for them and attachment to the state. We calculate

the correlation between our Table 7 coefficients and the 1990 state quality of life rankings reported in Table 3 of Gabriel, Matthey and Wascher (2003). The correlation coefficient is -0.429, which suggests that a better quality of life increases the effects of merit aid on post-college retention (since a smaller number indicates a higher ranking and better quality of life).²⁶

3.5.2 *Partial Correlation Analysis of State-Level Factors*

We next extend the bivariate correlation analysis in the previous sub-section to a multivariate framework, estimating partial correlation coefficients for the coefficients in Table 7 and multiple state-level factors simultaneously.²⁷ Partial correlations allow us to net out the effects of other explanatory variables and thus may provide important insights missed by the bivariate analysis. However, we are limited by the fact that we have only 24 state coefficient observations (since we exclude South Dakota for being such an outlier—results with South Dakota are available upon request). A model with very many explanatory variables (there are 17 in Table 8) would have too few degrees of freedom and likely suffer from a high degree of multicollinearity since many of the variables listed in Table 8 are highly correlated, which would hinder our ability to interpret the results. In the previous subsection we posited several possible explanations for the inter-state differences in the retention coefficients, but we do not have a theoretical model that would tell us which variables to include in the multivariate analysis. Thus, we use empirical criteria to narrow the variables to be included, but with the objective of identifying variables that are reflective of the more significant factors discussed in the previous

²⁶ We note that there are many quality of life rankings and they often differ significantly. So, it would be very possible to find a ranking that yielded a much different correlation coefficient. For example, using the ranking of metropolitan areas by Albouy (2012) to infer state rankings yields a correlation coefficient of -0.112.

²⁷ We estimate partial correlation coefficients rather than regression coefficients since the latter are hard to interpret given that the explanatory variables are scaled very differently and to facilitate comparison with the bivariate correlation analysis.

subsection and at the same time excluding redundant variables. However, this approach may not yield the true model, so the results should be interpreted with some caution.

For several of the factors we have alternative variables to measure the factor. We first select one variable for each factor, and thus eliminate 5 variables that are redundant. We select the variable that we think is the best measure of the factor among the alternatives. Column 2 of Table 8 presents the partial correlation coefficients for the 12 remaining variables. Next we exclude the four variables with partial correlation coefficients smaller than 0.2; the resulting partial correlation coefficients for the remaining 8 variables are shown in column 3 of Table 8. Finally, we drop the three variables from column 3 with the smallest partial correlation coefficients in column 3, leaving 5 variables, for which the correlation coefficients are reported in column 4 of Table 8.

As the number of variables included is reduced, the magnitude of the partial correlation coefficients change a little and more partial correlation coefficients become statistically significant, which is not unexpected given the correlations between the explanatory variables. Thus, we focus the discussion on the results in column 4 of Table 8.

Three of the variables' correlation coefficients are consistent with the bivariate analysis. The 1990 pre-merit aid post-college retention rate has a statistically significant negative effect with a partial correlation coefficient of -0.677. The Gabriel et al. (2003) quality of life ranking (with higher values indicating lower quality) has a coefficient of -0.393 that is significant at the ten percent level, which again suggests that better amenities make a state more attractive and increase the positive effect of merit aid programs on post-college retention in the state. The strong merit dummy has a coefficient of 0.295 that is marginally insignificant at conventional

levels but certainly consistent with expectations that strong merit aid programs have stronger effects on post-college retention.

Two of the five variables in column 4 of Table 8 have effects that contradict the bivariate analysis. First, the percent of the population that was urban in 1990 has a positive partial correlation coefficient that is not quite statistically significant; the expected effect is largely ambiguous and it is not very surprising to see its sign differ from the bivariate analysis. Second, and more importantly, the college quality measure based on the top ranked college according to Peterson's is significantly negative with a coefficient of -0.582. Recall that a larger ranking number implies lower quality, so this implies that for states with higher quality colleges merit aid increases post-college retention in the state.

4. Conclusion

One of the goals of state merit-based financial aid programs is to increase the stock of college-educated labor in the state. However, there has been surprisingly little research on whether state merit aid programs are effective at keeping college-educated persons in the state. This paper examines the effects of state merit aid programs on post-college location decisions for 25 states implementing merit aid programs between 1991 and 2004. Specifically, we investigate whether exposure to state merit aid programs affected whether a person resides in their state of birth during ages 24-30. Some of these states adopted much more significant merit aid programs than others and our preferred specification excludes states adopting weak merit aid programs from the analysis. We also estimate separate effects for each merit aid state.

We find that exposure to merit aid programs on average increases in-state post-college retention. Our baseline specification yields a coefficient of 0.0276, which suggests that strong

state merit aid programs increase the probability that a college attendee lives in his or her birth state during ages 24-30 by 2.8 percentage points. Procedures that account for measurement error suggest that the effect could be as large as 3.9 percentage points. While the magnitude is not incredibly large, it is certainly not trivial. Increasing the stock of college-educated workers in a state, even by a few percentage points, can have important benefits for a state, and supporters of merit programs should be pleased to know that strong merit aid programs do appear to have a meaningfully positive effect on in-state post-college retention.

However, there is an important caveat. We find meaningful differences in the effects of merit aid programs across states, both across and within states with strong and with weak merit programs. Florida, New Mexico, West Virginia, Alaska, Maryland, and Idaho experience relatively large effects, while other states experience smaller effects or even no effect on post-college retention. We examine the differences in the state-specific coefficients and offer some important insights into why the effectiveness might differ across states. For one, the largest merit-induced increase in post-college retention occurred in states with initially low retention rates. These states had the most opportunity to reduce the percentage of college attendees leaving the state and merit aid programs were an effective tool to do so. However, we also find that using merit programs to keep more students in-state for college does not necessarily translate into increased post-college retention. Merit programs might keep students in-state for college, but something else is needed to keep them in the state after college. We find evidence that having a desirable quality of life in a state is positively correlated with the ability of merit programs to increase in-state post-college retention. Thus merit aid alone is not very effective at keeping college-educated persons in the state post-college, but combining merit aid with

amenities that make a state desirable to live in can be effective at increasing post-college retention.

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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*
Maryland	2002-2005	Maryland HOPE Scholarship	NA	21.08*
Michigan	2000-2008	Michigan Merit & Promise Scholarship	0.20%	181.06*
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), the Brookings Institution, and state agency websites.

FTE and Expenditure are from NASSGAP Annual Reports; Awardees are from the Brookings Institution

*2003-04 data since more recent data are not available

NA: Not Available

Table 2: Main Sample Summary Statistics for Strong, Weak and Non-Merit Birth States

	Strong Merit		Weak Merit		Non-Merit	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Living in Birth State	0.637	0.481	0.646	0.478	0.642	0.479
Merit	0.397	0.489	0.312	0.463	0.000	0.000
Age	27.172	1.978	27.166	1.986	27.151	1.984
Female	0.555	0.497	0.537	0.499	0.538	0.499
White	0.748	0.434	0.735	0.441	0.822	0.383
Black	0.180	0.384	0.102	0.303	0.082	0.274
Hispanic	0.046	0.211	0.105	0.307	0.063	0.242
Asian	0.007	0.083	0.031	0.172	0.014	0.117
Other	0.018	0.134	0.027	0.162	0.020	0.139
Total Observations	163,939		523,941		557,179	

Table 3: Merit Program Effects on Living in Birth State for Different Treatment and Control Groups

	(1)	(2)	(3)	(4)	(5)
Merit	0.0276 (0.0085)***	0.0286 (0.0085)***	0.0251 (0.0090)**	0.0225 (0.0087)**	0.0059 (0.0051)
Individual Controls	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes
Region*Year Dummies	Yes	Yes	No	Yes	Yes
Strong Merit States	Treatment	Treatment	Treatment	Treatment	Excluded
Weak Merit States	Excluded	Excluded	Excluded	Control	Treatment
Non-Merit States	Control	Control	Control	Control	Control
Northeast & Midwest	Excluded	Included	Excluded	Excluded	Included
Total States	20	34	20	29	41

Notes: The main sample includes only persons who have attended at least some college. Individual controls include dummy variables for state of birth, year of birth, survey year, age, sex, race, and Hispanic origin. State controls include the unemployment rate, the college wage premium, and the percentage of white collar jobs measured at several points in time and the log cohort size and the percent of the cohort foreign born at age 18. Standard errors in parentheses are clustered by state of birth.

Significant at 5% based on standard errors clustered by state of birth; *Significant at 1%.

Table 4: Merit Program Effects on Living in State of Birth for Alternative Sample Selections

	(1) "Full" Sample	(2) Individuals Excluded from Main Sample	(3) Included with Some College, No Bachelor's	(4) Included with Bachelor's or Higher
Merit	0.0142 (0.0075)*	0.0041 (0.0070)	0.0279 (0.0093)***	0.0255 (0.0101)**
Individual Controls	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
Region*Year Dummies	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
Northeast & Midwest	Excluded	Excluded	Excluded	Excluded
Total States	20	20	20	20

Notes: The main sample includes only persons who have attended at least some college. Individual controls include dummy variables for state of birth, year of birth, survey year, age, sex, race, and Hispanic origin. State controls include the unemployment rate, the college wage premium, and the percentage of white collar jobs measured at several points in time and the log cohort size and the percent of the cohort foreign born at age 18. Standard errors in parentheses are clustered by state of birth.

*Significant at 10% based on standard errors clustered by state of birth; **Significant at 5%; ***Significant at 1%.

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
Merit	0.0308 (0.0090)***	0.0349 (0.0111)***	0.0390 (0.0119)***
Individual Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Region*Year Dummies	Yes	Yes	Yes
Strong Merit States	Treatment	Treatment	Treatment
Weak Merit States	Excluded	Excluded	Excluded
Non-Merit States	Control	Control	Control
Northeast & Midwest	Excluded	Excluded	Excluded
Total States	20	20	20

Notes: The main sample includes only persons who have attended at least some college. Individual controls include dummy variables for state of birth, year of birth, survey year, age, sex, race, and Hispanic origin. State controls include the unemployment rate, the college wage premium, and the percentage of white collar jobs measured at several points in time and the log cohort size and the percent of the cohort foreign born at age 18. Standard errors in parentheses are clustered by state of birth.

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

Table 6: Merit Effects by Cohort Years Before and After Policy

	Coefficient	St. Error
Merit State*9+ Cohort Years Before Policy	0.0077	(0.0114)
Merit State*5-8 Cohort Years Before Policy	0.0057	(0.0073)
Merit State*1-4 Cohort Years Before Policy		Base Years
Merit State*1st-4th Cohort Years of Policy	0.0285	(0.0083)***
Merit State*5th-8th Cohort Years of Policy	0.0294	(0.0120)**
Merit State*9th+ Cohort Years of Policy	0.0257	(0.0142)*

Notes: Cohort year refers to the year age18. Coefficients are estimated by a joint regression consistent with the baseline specification in Column 1 of Table 3.

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

Table 7: Merit Effects by State of Birth

	Coefficient	St. Error
<u>A. Strong Merit States</u>		
Merit*Florida	0.0456	(0.0081)***
Merit*Georgia	0.0185	(0.0114)
Merit*Kentucky	0.0213	(0.0118)*
Merit*Louisiana	0.0157	(0.0110)
Merit*Nevada	-0.0194	(0.0167)
Merit*New Mexico	0.0389	(0.0152)**
Merit*South Carolina	0.0036	(0.0088)
Merit*Tennessee	-0.0034	(0.0111)
Merit*West Virginia	0.0395	(0.0201)**
<u>B. Weak Merit States</u>		
Merit*Alaska	0.0374	(0.0326)
Merit*Arkansas	0.0160	(0.0226)
Merit*California	-0.0122	(0.0074)*
Merit*Idaho	0.0418	(0.0093)***
Merit*Illinois	0.0093	(0.0063)
Merit*Maryland	0.0412	(0.0233)*
Merit*Michigan	-0.0430	(0.0063)***
Merit*Mississippi	0.0094	(0.0148)
Merit*Missouri	0.0158	(0.0088)*
Merit*New Jersey	0.0103	(0.0081)
Merit*New York	0.0211	(0.0060)***
Merit*North Dakota	0.0114	(0.0128)
Merit*Oklahoma	0.0104	(0.0162)
Merit*South Dakota	-0.1045	(0.0148)***
Merit*Utah	-0.0135	(0.0109)
Merit*Washington	0.0025	(0.0097)

Notes: Coefficients are estimated by a joint regression including all 50 states that is otherwise consistent with the baseline specification in Column 1 of Table 3.

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

Significant at 5%; *Significant at 1%.

Table 8: Correlations Between Retention Coefficients and Selected Variables

Variable	(1) Bivariate Correlation Coefficients	(2)	(3)	(4)
Pre-Merit Aid Retention Rate	-0.513**	-0.607**	-0.612***	-0.677***
In-State Enrollment Coefficient	0.187	-0.102		
Degree Completion Coefficient	0.181	0.231	0.151	
Merit Aid per Recipient	0.090			
Aid Recipients per Student	0.206			
Strong Merit Aid State Dummy	0.166	0.226	0.283	0.295
Year of Adoption	-0.093	0.030		
Peterson's Ranking	0.254	-0.458	-0.585**	-0.582***
Percent of Students with High Math SAT	-0.311			
Percent of Students with High Verbal SAT	-0.329			
Public Institutions per Capita	0.281	-0.160		
4-Year Public Institutions per Capita	0.324			
Percent Enrolled in 4-Year Schools	0.341	0.261	0.202	
Median Income	-0.080	-0.225	-0.190	
State Population	-0.213	0.086		
Percent Urban	-0.169	0.400	0.400	0.319
Quality of Life Ranking	-0.429**	-0.284	-0.245	-0.393*

Note: See text for explanation of variables.

*Significant at 10%; **Significant at 5%; ***Significant at 1%.

Appendix Table A: State of Birth Characteristics in 1990

Birth State	% of Cohort Living in Birth State, Ages 15-17	% of College Attendees Living in Birth State, Ages 24-30	% of College Graduates Living in Birth State, Ages 24-30	% of Cohort with at Least Some College, Ages 24-30	% of Cohort with a Bachelor's Degree or Higher, Ages 24-30
Alabama	0.813	0.596	0.538	0.485	0.178
Alaska‡	0.532	0.268	0.211	0.517	0.176
Arizona	0.712	0.551	0.442	0.510	0.150
Arkansas‡	0.758	0.515	0.466	0.446	0.160
California‡	0.807	0.735	0.710	0.586	0.209
Colorado	0.655	0.492	0.433	0.569	0.229
Connecticut	0.770	0.577	0.510	0.597	0.304
Delaware	0.658	0.427	0.331	0.526	0.240
Florida†	0.744	0.577	0.510	0.497	0.181
Georgia†	0.807	0.614	0.567	0.432	0.169
Hawaii	0.646	0.505	0.460	0.599	0.244
Idaho‡	0.670	0.371	0.268	0.560	0.175
Illinois‡	0.768	0.607	0.556	0.599	0.259
Indiana	0.791	0.563	0.476	0.492	0.201
Iowa	0.762	0.475	0.371	0.591	0.244
Kansas	0.708	0.496	0.449	0.582	0.242
Kentucky†	0.778	0.578	0.510	0.420	0.160
Louisiana†	0.819	0.571	0.506	0.445	0.174
Maine	0.812	0.512	0.413	0.466	0.170
Maryland‡	0.737	0.581	0.517	0.503	0.210
Massachusetts	0.780	0.631	0.603	0.612	0.305
Michigan‡	0.837	0.655	0.564	0.564	0.209
Minnesota	0.832	0.643	0.567	0.627	0.253
Mississippi‡	0.769	0.518	0.469	0.480	0.167
Missouri‡	0.759	0.564	0.501	0.524	0.223
Montana	0.690	0.380	0.311	0.584	0.222
Nebraska	0.717	0.496	0.404	0.621	0.251
Nevada†	0.615	0.431	0.354	0.546	0.167
New Hampshire	0.680	0.505	0.425	0.509	0.219
New Jersey‡	0.764	0.563	0.515	0.594	0.307
New Mexico†	0.700	0.425	0.314	0.492	0.162
New York‡	0.773	0.591	0.547	0.627	0.308
North Carolina	0.816	0.672	0.614	0.482	0.185
North Dakota‡	0.653	0.420	0.317	0.645	0.263
Ohio	0.808	0.603	0.530	0.513	0.211
Oklahoma‡	0.772	0.530	0.458	0.533	0.210
Oregon	0.746	0.560	0.471	0.548	0.180
Pennsylvania	0.835	0.575	0.519	0.514	0.253
Rhode Island	0.702	0.554	0.489	0.576	0.255
South Carolina†	0.804	0.594	0.521	0.451	0.166
South Dakota‡	0.687	0.391	0.349	0.601	0.251
Tennessee†	0.791	0.606	0.548	0.460	0.175
Texas	0.844	0.740	0.705	0.507	0.179
Utah‡	0.763	0.631	0.533	0.616	0.193
Vermont	0.764	0.458	0.330	0.452	0.174
Virginia	0.736	0.552	0.483	0.493	0.201
Washington‡	0.772	0.611	0.535	0.576	0.213
West Virginia†	0.744	0.416	0.361	0.414	0.163
Wisconsin	0.852	0.605	0.495	0.557	0.239
Wyoming	0.589	0.302	0.209	0.584	0.220

†Strong merit state; ‡weak merit state.