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# State Merit Aid: Effects on College Enrollment, Labor Market Outcomes, and Government Revenue*

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April 2, 2026

**Abstract**

This paper evaluates long-run effects of state merit aid programs that subsidize in-state college attendance. Using national survey data on college enrollment and U.S. Census data, I exploit staggered program adoption across states. Merit aid shifts students from out-of-state to in-state institutions, which are on average relatively less selective. There is little evidence that these programs increase degree attainment or long-run in-state residence. For women, program exposure reduces post-college employment and earnings, while there is limited evidence of effects for men. Welfare analysis shows net losses for students and the implementing state, with additional negative fiscal spillovers to other jurisdictions.

Keywords: Merit Aid, College Enrollment, Labor Market Outcomes, Welfare Analysis

JEL Classification: I22, J24, H75

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# 1 Introduction

Over the past three decades, many U.S. states have adopted merit-based college scholarship programs as a strategy to invest in human capital and strengthen their future workforces. These programs provide state-funded merit aid to high-achieving resident students, with the explicit goal of encouraging college attendance within state borders and reducing the out-migration of college-educated workers (Sjoquist and Winters, 2014; Fitzpatrick and Jones, 2016). For state governments concerned about "brain drain", merit aid represents a highly visible policy tool: by lowering the cost of in-state colleges for talented students, states aim to attract and retain individuals who may contribute to local economies and generate fiscal returns over their lifetimes.

State merit aid programs operate in a higher education market characterized by considerable pricing complexity and uncertainty about long-run returns Levine (2022). College costs are often opaque, varying across institutions and students, while the economic returns to different colleges are uncertain, heterogeneous, and realized years later. In contrast, merit scholarships provide a clear and immediate reduction in tuition. As a result, students' enrollment decisions may be particularly sensitive to these salient price changes,[1] even though the differences in returns across college attendance decisions often exceed the differences in costs that students face (Mountjoy, 2026). It is therefore critical to understand whether state merit aid successfully promotes human capital development and strengthens state economies. While prior research examines merit aid's effects on educational outcomes with mixed findings,[2] there is limited evidence on longer-term labor market outcomes or on whether these

[1]Research suggests that complexity and uncertainty in financial aid lead students to value price certainty highly when choosing where to enroll, and that such certainty substantially influences college attendance decisions (Hoxby and Turner, 2015; Dynarski et al., 2021; Burland et al., 2023).

[2]Dynarski (2008) evaluates merit programs in Georgia and Arkansas and finds positive impacts on college completion, while Jia (2019) finds that programs with lenient eligibility requirements and generous aid raise educational attainment. In contrast, Sjoquist and Winters (2015b) finds little evidence of net effects on educational attainment using variation from all state merit aid programs. Additionally, Sjoquist and Winters (2016) and Cohodes and Goodman (2014) show that merit aid exposure shifts students from relatively more selective out-of-state colleges to relatively less selective in-state colleges, subsequently reducing college degree receipt.

programs generate net benefits for students or fiscal returns for governments.

This paper addresses this gap by providing the first national evidence on how state merit aid programs affect long-run labor market outcomes as well as the economic and fiscal welfare of students and governments. This study contributes to relevant literature and policy discussions in a few ways. First, it represents one of only a small handful of studies that assess the impact of financial aid on labor market outcomes. Research on need-based financial aid generally points toward positive effects on earnings (Denning et al., 2019; Black et al., 2023; Bettinger et al., 2019), but evaluations of state merit programs have not detected significant earnings impacts (Welch, 2014; Scott-Clayton and Zafar, 2019). Whereas these prior studies focus on variation from individual programs, this paper exploits variation across all major state merit aid programs in the U.S. Second, the findings provide new evidence on the role of state merit aid in shaping college enrollment and how changes in college selectivity influence long-term outcomes. Prior research suggests that attending a more selective college is particularly consequential for women's educational attainment, family formation, and labor market outcomes (Ge et al., 2022; Carlana et al., 2024; Kirkeboen et al., 2025). Third, I leverage new estimates of program impacts to conduct welfare analysis following the Hendren and Sprung-Keyser (2020) cost-benefit framework.[3] This approach is extended to examine revenue neutrality and net fiscal impacts across in-state, out-of-state, and federal governments, quantifying the broader economic consequences of state merit aid beyond aggregate government costs.

The analysis uses data from two main sources. First, college enrollment data are from the Integrated Postsecondary Education Data System (IPEDS) Residence and Migration Survey (NCES, 2023b). Second, data on longer-term outcomes are from the U.S. Census and American Community Survey (ACS) (Ruggles et al., 2022). The methodological approach exploits variation between states and across cohorts driven by the staggered rollout of state

[3]Hendren and Sprung-Keyser (2020) conduct a recent meta-analysis that uses existing estimates in the literature to develop a novel approach to welfare analysis, estimating the ratio of net benefits to recipients relative to net government costs.

merit aid programs. For analysis of college enrollment outcomes using IPEDS data, I use the state of permanent residence at the time of college entry and the enrollment year to identify merit program exposure. For outcomes from the Census data, I use birth state and birth cohort to create a proxy for merit program exposure.[4] I estimate the impact of being born in a merit aid state and reaching age 18 after program implementation by comparing the change in outcomes between those born in merit and non-merit states across exposed and unexposed cohorts.

The introduction of state merit programs shifts enrollment away from more selective out-of-state institutions toward in-state colleges that are, on average, less selective, consistent with prior evidence on merit aid (Sjoquist and Winters, 2016; Cohodes and Goodman, 2014). Enrollment in out-of-state colleges falls by 19 percent while in-state enrollment rises by 17 percent. These changes are statistically significant, though confidence intervals are wide, so the focus is on the direction and significance rather than precise magnitude. Colleges located in merit states are, on average, less selective across multiple measures, including lower student earnings 10 years after enrollment, lower graduation rates, and lower average SAT scores.[5] Despite these shifts in enrollment and differences in average institutional selectivity, estimated effects on educational attainment are close to zero and precisely estimated in the Census data. Overall, merit aid alters where students enroll without improving educational attainment and is associated with negative labor market effects for some groups.

Results for labor market outcomes and state residence indicate that program exposure harms women's labor market outcomes, while impacts for men are smaller and statistically insignificant, and there is no meaningful effect on long-term retention of in-state residents. For women, merit aid exposure reduces employment by 0.8 percentage points (p.p.) and earnings by $500 (1.9 percent decrease). Parameterized estimates show that a 10 p.p. increase

[4]This proxy has been used widely in the merit aid literature (Dynarski, 2008; Sjoquist and Winters, 2015a; Jia, 2019) and in other studies using Census survey data to proxy for program exposure across states and time (Goodman-Bacon, 2021).

[5]These selectivity measures reflect average differences across colleges in different states rather than changes in the specific institutions attended by marginal students, whose college choices are not directly observable in the data.

in the share of students receiving merit aid lowers female employment by about 0.3 p.p. and earnings by roughly $200, highlighting that broad program reach is central to shaping outcomes at scale. The earnings decline is largely driven by reductions in extensive margin employment, though decreases in hours worked among employed women suggest a small role for intensive margin labor supply. For men, there is a relatively precisely estimated zero effect on employment while the earnings impact is small and not significant. Although staggered program adoption may raise concerns about differential pre-trends, results are robust to controls for demographics, concurrent state-level economic conditions, and birth state by birth cohort trends, and are similar when using the Sun and Abraham (2021) heterogeneity-robust estimator. Placebo tests using individuals who do not attend college, who are unlikely to be eligible for merit aid, show no significant effects on labor market outcomes.

The analysis concludes by investigating effect heterogeneity by race, assessing mechanisms, and evaluating welfare implications. For White women, I find negative effects on postsecondary degree attainment and also find that effects on having a child and labor market outcomes are concentrated primarily among this group. The mediation analysis suggests that a decrease in college selectivity is a plausible mechanism that can explain a substantial portion of the negative effects on labor market outcomes for women, with effects on childbearing playing a modest secondary role. Welfare analysis highlights the limited economic returns of merit programs. The data indicate that negative estimated earnings effects outweigh reductions in educational costs for merit program recipients. I find that there is a $1.57 loss for every dollar of government spending on state merit aid programs. Revenue neutrality analysis shows that a state government implementing a merit aid program recoups only 67 cents of revenue that would have been collected in the absence of the programs, with additional negative spillovers for out-of-state and federal governments. Taken together, these findings indicate that state merit aid programs generate net welfare losses, calling into question their role as economic engines for building state workforces.

This paper proceeds as follows. I discuss background in Section 2. Section 3 presents the conceptual framework for assessing the potential impact of merit program rollout. Section 4 describes the data and Section 5 details the empirical strategy (5.1) and identifying assumptions (5.2). Section 6 presents main results (6.1 and 6.2), heterogeneity results and mediation analysis (6.3), and robustness tests (6.4). I discuss the welfare analysis in Section 7. Section 8 concludes the paper.

# 2 Background

## 2.1 State Merit Aid Programs Background

Since 1990, 26 states have adopted merit aid programs that award scholarships to high-achieving high school students for in-state college attendance.. These programs were introduced in part to retain high-achieving students and mitigate concerns about "brain drain" to other states (Joint Economic Committee, 2019). Merit aid has become a major feature of the higher education landscape in states with the largest programs. In 2019-2020, states spent nearly $3 billion on more than 750,000 recipients (NASSGAP, 2020). In states with the largest programs, a sizable share of college enrollees receive a merit award.

Programs differ widely in award generosity and coverage. To focus on programs most likely to affect students' college choices and long-term outcomes, I distinguish between 10 "strong" merit aid states with relatively generous and widely accessible awards, 15 "weak" merit aid states with smaller programs, and 25 "non-merit" states that never adopt such programs.[6] Program strength is is measured by aid per full-time equivalent (FTE) student, which captures both award generosity (aid per recipient) and program reach (the share of students receiving aid). By construction, all strong merit states have at least one program with aid per FTE student of $400 or more, while all programs in weak merit states have aid per FTE student below $300.[7] The "main" program in each state is defined as the program

[6]Alaska is considered a strong merit state, but is excluded from this classification due to its late adoption in 2011.

[7]These thresholds create a clear separation in program scale across states and generate a classification

with the highest aid per FTE. Figure 1 illustrates the geographic distribution of strong, weak, and non-merit aid states, highlighting how program intensity varies across regions, while Table 1 provides a summary of key features of the programs in strong merit states.[8]

Strong merit programs operate at meaningful scale, both in per-student award amounts and in the share of students receiving aid. Average awards among main programs in these states are about $4,000 per recipient, and in most strong merit states, more than 20 percent of in-state college students receive merit aid. A key feature common to all strong programs is that awards must be used at in-state institutions, directly linking academic eligibility to in-state college attendance. Eligibility is typically based on high school GPA, SAT or ACT scores, or state standardized test scores, and many states use tiered award structures that provide larger scholarships to higher-achieving students. Awards can generally be used at a broad set of public and private in-state institutions, including four-year universities and state colleges, two-year colleges, and technical or vocational programs. For additional details on merit aid program background, see Appendix A.

## 2.2 Financial Aid and Labor Market Outcomes

A nascent literature has evaluated the impact of merit- and need-based financial aid on labor market outcomes (Welch, 2014; Scott-Clayton and Zafar, 2019; Denning et al., 2019; Black et al., 2023; Bettinger et al., 2019; Carlson et al., 2022). Welch (2014) and Scott-Clayton and Zafar (2019) exploit merit aid eligibility cutoffs in Tennessee and West Virginia and find small, statistically insignificant effects on earnings. My study extends this literature by using variation from all major state merit aid programs, allowing estimation of average effects for all students exposed to merit aid rather than just those near an eligibility threshold.

Other studies examine the labor market effects of need-based aid and student loans

of states that is similar to prior classifications in the literature (Sjoquist and Winters, 2015a,b, 2016).

[8]The 10 strong merit aid states are concentrated in the Southern and Western Census regions while the 15 weak and 25 non-merit aid states are distributed across all four Census regions. Information on spending and eligibility for all states is reported in Tables A1 and A2.

(Denning et al., 2019; Black et al., 2023; Bettinger et al., 2019; Carlson et al., 2022). Evidence is mixed, but targeting is crucial: aid that reaches financially constrained students tends to increase long-term earnings, while less well-targeted aid can have weaker or even negative effects. For example, Denning et al. (2019) and Black et al. (2023) find positive impacts for financially constrained students, whereas Bettinger et al. (2019) show that California's Cal Grant increases earnings at GPA eligibility thresholds (financially constrained students) but has negligible or negative effects at family income thresholds. Carlson et al. (2022) exploit a randomized lottery for need-based aid in Wisconsin and find negative impacts on early-career earnings for four-year students, possibly due to reduced work incentives from lower student loan burdens. My findings complement this literature by showing that when aid is not targeted to financially constrained students and distorts college choices, it can have negative effects on labor market outcomes for some students.

## 2.3 College Selectivity and Labor Market Outcomes

Prior research shows that merit aid shifts enrollment from out-of-state to in-state colleges and reduces college selectivity (Sjoquist and Winters, 2016; Cohodes and Goodman, 2014). Using variation from major state merit aid programs, Sjoquist and Winters (2016) link enrollment data to selectivity measures for roughly 350 four-year institutions. I extend this work by examining enrollment across nearly 5,000 colleges, covering the full set of institutions participating in federal financial aid programs. While Sjoquist and Winters (2016) document average enrollment effects, Cohodes and Goodman (2014) show that being marginally eligible for a merit award reduces selectivity and college persistence, particularly for White students.

Related research suggests that college selectivity may be especially consequential for women's educational attainment, family formation, and labor market outcomes (Ge et al., 2022; Carlana et al., 2024; Kirkeboen et al., 2025). In contrast, Mountjoy (2026) estimates the marginal returns to public universities in Texas and finds modest earnings gains from marginal intensive-margin increases in selectivity, with similar effects for men and women.

However, these intensive-margin estimates do not speak to larger shifts in selectivity or to students on the margin of attending private or out-of-state colleges, margins that are central in my setting.

Largely consistent with this prior research, I find that merit aid shifts enrollment to in-state colleges that are less selective on average. I further show that negative effects on educational attainment, as well as impacts on having a child and labor market outcomes, are concentrated among White women.

### 2.4 Merit Aid and Welfare

Finally, a recent meta-analysis evaluates the welfare implications of a broad set of policies, including state merit aid programs (Hendren and Sprung-Keyser, 2020). Using estimated impacts on educational attainment to infer earnings effects and combining these with educational costs, this work finds benefit-cost ratios ranging from \$0.72 to \$4.00 per dollar of government spending. For example, Cornwell et al. (2006) and Bruce and Carruthers (2014) estimate positive effects on four-year enrollment that imply benefit-cost ratios of \$4.00 and \$1.86 in Georgia and Tennessee, respectively. In contrast, Cohodes and Goodman (2014) find negative effects on college completion, yielding a lower, but still positive, ratio of \$0.72, as reductions in educational costs offset declines in degree attainment. Building on this literature, I compute benefit-cost ratios using directly estimated earnings effects for all major state merit aid programs and evaluate fiscal impacts, including revenue neutrality and spillovers across state and federal governments.

## 3 Conceptual Framework

This section presents a stylized model of educational choice that illustrates how state merit aid influences college enrollment decisions and subsequent labor market outcomes. Students choose among three educational options: attending an out-of-state college ($e_o$), attending an in-state college ($e_i$), or not attending college ($e_{nc}$). Students select the option that maximizes expected utility:

$$E(V^{e_j}) - E(C^{e_j}) + u^{e_j}, \tag{1}$$

where $E(V^{e_j})$ is the expected present discounted value of lifetime earnings associated with educational choice $e_j$, $E(C^{e_j})$ is the expected present discounted value of educational costs borne by the student (including net tuition payments, room and board, and debt payments), and $u^{e_j}$ captures contemporaneous utility from the educational choice.

The introduction of state merit aid reduces the out-of-pocket cost of attending in-state colleges. In this framework, merit aid therefore affects educational choices along two margins.[9] First, for students who in the absence of state merit aid choose to attend an out-of-state college, but are on the margin of attending in-state college, their choice is represented with the following inequality:

$$E(V^{e_o}) - E(C^{e_o}) + u^{e_o} > E(V^{e_i}) - E(C^{e_i}) + u^{e_i}. \tag{2}$$

With the introduction of state merit aid, there is no change in the value of choosing out-of-state college, but for in-state college, there is a decrease in students' expected educational costs $E(C^{e_i})$. For the students on the margin, the above inequality reverses and these students switch their choice to in-state college:

$$E(V^{e_o}) - E(C^{e_o}) + u^{e_o} < E(V^{e_i}) - E(C^{e_i}) + u^{e_i}. \tag{3}$$

Second, for students who in the absence of state merit aid choose not to attend college, but are on the margin of attending in-state college, their choice is represented with the following inequality:

[9] I abstract from the third margin between no college and out-of-state college because there is no change in the value of these choices with the introduction of state merit aid in partial equilibrium.

$$E(V^{e_{nc}}) - E(C^{e_{nc}}) + u^{e_{nc}} > E(V^{e_i}) - E(C^{e_i}) + u^{e_i}. \quad (4)$$

With the introduction of state merit aid, there is no change in the value of choosing no college attendance, but for in-state college, again there is a decrease in expected educational costs $E(C^{e_i})$. For the students on the margin, the above inequality reverses and these students switch their choice to in-state college:

$$E(V^{e_{nc}}) - E(C^{e_{nc}}) + u^{e_{nc}} < E(V^{e_i}) - E(C^{e_i}) + u^{e_i}. \quad (5)$$

To generate predictions, I impose three simple assumptions consistent with prior research (Sjoquist and Winters, 2016) and the human capital framework of Becker (1964) and Mincer (1974): (i) college costs increase with institutional selectivity, (ii) out-of-state colleges are more expensive than in-state colleges, and (iii) attending college increases expected lifetime earnings relative to not attending.

Under the first two assumptions, lowering the cost of in-state college through merit aid induces marginal students to switch from higher-cost, more selective out-of-state colleges to lower-cost, less selective in-state colleges. In the absence of merit aid, these students attend out-of-state colleges because their greater selectivity is associated with higher expected earnings. When merit aid lowers the price of in-state options, some students substitute toward these lower-cost but less selective institutions, potentially reducing their expected earnings.

Under the third assumption, merit aid can also induce enrollment among students who would otherwise not attend college. Without merit aid, some students do not attend college because the expected costs exceed the expected gains in lifetime earnings. When merit aid lowers these costs, some students cross the enrollment margin and attend college, increasing their educational attainment and expected lifetime earnings.

Behavioral frictions may amplify the substitution toward in-state college attendance due to biased perceptions about educational costs relative to expected earnings. A large literature shows that students often lack information about differences in college costs and returns and place substantial weight on salient financial incentives when making enrollment decisions (Hoxby and Turner, 2015; Dynarski et al., 2021; Burland et al., 2023). As a result, a visible reduction in in-state tuition may disproportionately influence college attendance choices even when differences in expected earnings across these choices are larger.

The relative importance of the two margins depends in part on how merit aid is targeted. If merit aid primarily reduces costs for students who are already likely to attend college, such as higher-achieving or higher-income students, it may mainly affect where students enroll rather than whether they enroll in college at all. In this case, substitution from out-of-state to in-state colleges may dominate. If instead aid is targeted toward financially constrained students who are on the margin of attending college, increases in college attendance may play a larger role.

These mechanisms imply that the welfare consequences of merit aid depend on both changes in lifetime earnings and students' educational costs. Substitution from out-of-state to in-state colleges lowers educational costs but may reduce earnings if students attend less selective institutions. In contrast, increases in college attendance raise both educational costs and expected earnings. The overall impact of merit aid on student welfare therefore depends on the relative magnitude of these two margins and the extent to which merit aid primarily affects institutional selectivity versus college attendance.

# 4 Data

This paper uses data from two primary sources: (i) the Integrated Postsecondary Education Data System (IPEDS) Residence and Migration Survey (NCES, 2023b) and (ii) the U.S. Census and American Community Survey (ACS) (Ruggles et al., 2022). IPEDS data are used to examine the effects of merit aid program adoption on out-of-state and in-state college enrollment. Census and ACS data are used to evaluate longer-run impacts on educational attainment, employment, and earnings.

## 4.1 IPEDS Data

**Data Description and Outcomes.** Biannual college enrollment data are drawn from the IPEDS Residence and Migration Survey from 1986 to 2020 (NCES, 2023b). The survey is administered to all U.S. postsecondary institutions participating in federal student aid programs and requires reporting in even-numbered years (with the exception of 1990).[10] These data are panel since the same postsecondary institutions are followed over time. The survey reports the Fall enrollment of first-time degree-seeking freshmen who graduated from high school in the previous 12 months at each institution by state of residence. Because both students' residential state and the institution's state are observed, I can distinguish between out-of-state and in-state enrollment by residential state. This structure allows me to examine how merit aid adoption affects enrollment decisions among residents of adopting states. The IPEDS sample includes 4,951 postsecondary institutions.[11] I aggregate institution-level counts to the residential state-by-year level to construct measures of out-of-state, in-state, and total enrollment, and the primary outcomes take the logarithmic transformation of these enrollment measures.

**Defining Treatment and Sample Construction.** The treatment group consists of the 10 strong merit states, while the control group includes the 25 non-merit states and not-yet-treated cohorts in strong merit states. To maximize contrast between states with high-intensity merit aid programs and states without merit aid, the 15 weak merit states are excluded from the IPEDS sample.[12] The key explanatory variable is an indicator for residing in a strong merit state and enrolling in college after program introduction. This sample is restricted to up to six years before merit program adoption and up to seventeen years after

[10]Although data have been collected annually since 2001, reporting in odd-numbered years is voluntary and incomplete, so I restrict the analysis to even-numbered years (NCES, 2023b).

[11]Summary statistics for IPEDS institutions linked to Delta Cost Project tuition data (NCES, 2015) and College Scorecard student outcomes data (US Department of Education, 2024) are reported in Table C1.

[12]Alaska is excluded from the analysis because its late adoption of a strong merit program in 2011 leaves too few post-treatment cohorts to maintain a balanced panel.

merit program adoption, which is the widest range that allows me to maintain a balanced panel in all pre- and post-event periods.[13]

A key strength of the IPEDS data is that they cover the near universe of U.S. post-secondary institutions participating in federal student aid programs, allowing me to assess impacts on enrollment across the full distribution of colleges. This represents a substantial expansion relative to prior research that relied on data for roughly 350 four-year institutions included in the *USNWR* rankings (Sjoquist and Winters, 2016). A limitation of the Residence and Migration Survey, however, is that enrollment is not reported separately by gender, which precludes analysis of gender heterogeneity in the enrollment results.

## 4.2 Census and ACS Data

**Data Description and Outcomes.** Data on educational attainment, employment, and earnings were obtained from the 5 percent and 1 percent samples of the 2000 U.S. Census, as well as the ACS from 2000 to 2019 (Ruggles et al., 2022). These data are repeated cross-sections: a new random sample is drawn each year, so individuals are not tracked over time. Educational attainment outcomes include indicators for completing at least one year of college, earning an associate's degree or higher, a bachelor's degree or higher, and a master's degree or higher. The primary measure of employment is an indicator for having positive earnings over the previous year.[14] The main measure of earnings is earnings below the 90th percentile in 2019 dollars.[15]

[13]Due to the biannual structure of the IPEDS Residence and Migration Survey, odd-year adopting states are observed in odd-numbered event years, while even-year adopting states are observed in even-numbered event years. Arkansas was the earliest-adopting state in 1991, which allows for a maximum of six pre-event periods while keeping the panel balanced with data back to 1986. Tennessee was the latest-adopting state in 2003, which allows for 17 post-event periods while keeping the panel balanced with data through 2020.

[14]Employment defined as having positive earnings over the previous year is used as the main measure of extensive margin labor supply because it is more persistent and less noisy than measures of employment or labor force participation at the survey date.

[15]This measure includes individuals with zero earnings to capture both extensive and intensive margin effects. The sample is trimmed at the 90th percentile to address the right-skewed nature of the earnings distribution. Trimming earnings at this threshold has been used in prior research using ACS data to increase the precision of earnings estimates Pope (2016). Figure C1 shows that estimates are robust to alternative trimming percentiles, though precision declines in specifications without controls, which I discuss further in

I augment the analysis with additional outcomes for employment, state residence, family formation, and college majors and occupations. Other employment outcomes capture both extensive and intensive margin effects, including indicators for being employed or in the labor force at the survey date, weeks worked in the previous year, and usual hours worked per week. State residence is measured by an indicator for living in the state of birth. Family formation outcomes include indicators for ever being married and having children in the household, as well as a measure of the number of children. College major outcomes include indicators for having a STEM major or STEM occupation, as well as predicted earnings by major or occupation.[16]

**Defining Treatment and Sample Construction.** The treatment group consists of individuals born in the 10 strong merit states who reached age 18 after program implementation, while the control group consists of individuals born in the 25 non-merit states and not-yet-treated cohorts in strong merit states. Consistent with the college enrollment analysis, individuals from the 15 weak merit states are excluded from the main analysis to focus on the contrast between states with high-intensity programs and those without programs.[17] The key explanatory variable is strong merit program exposure, defined as being born in a strong merit state and reaching age 18 after program implementation. Birth state is a proxy for high school location, and age 18 is a proxy for high school graduation year.

Several restrictions are applied to the Census and ACS sample to refine the treatment measure and ensure valid comparisons across states and cohorts. First, the sample includes only individuals born in a U.S. state or the District of Columbia. Second, the sample is restricted to ages 23-34 to capture post-college outcomes while ensuring that at least one

Section 6.4. Earnings are converted to 2019 dollars using the CPI-U (Minneapolis Fed, 2023).

[16]STEM majors are classified following Sjoquist and Winters (2015a), and STEM occupations are classified following Langdon et al. (2011) and Vilorio (2014). Predicted earnings variables are based on coefficients from regressions of log earnings on major or occupation fixed effects, capturing relative differences in earnings across fields.

[17]These weak merit states are included in some parameterized specifications, and those results are described in Section 6.2. Alaska is excluded from the analysis because its late adoption of a strong merit program in 2011 leaves too few post-treatment cohorts to maintain a balanced panel.

treated cohort is observed at each age in all strong merit states.[18] Third, the sample includes up to seven cohorts before program adoption and up to eleven cohorts after adoption to maintain a balanced panel.[19] Fourth, the sample is restricted to individuals who attended at least some college, which sharpens identification by focusing on individuals likely to have been exposed to merit aid.[20] Fifth, observations with imputed values for key variables or residing in group quarters are excluded.[21] These restrictions define the analytic sample.[22]

There are three primary advantages of using the Census and ACS microdata. First, these data allow estimation of average effects of state merit aid at the national level using variation across all adopting states. Second, the large sample size spanning all U.S. states over two decades permits precise estimation. Third, the richness of the microdata allows examination of several long-term outcomes, including educational attainment, labor market outcomes, and state residence. The main limitation is that individual merit aid receipt cannot be directly observed. Consequently, exposure is proxied by birth in a strong merit aid state, and the estimates should be interpreted as intention-to-treat (ITT) effects.[23]

# 5 Empirical Strategy

This section begins by describing the identification strategy and main empirical specifications in Section 5.1 and then discussing identifying assumptions and the event study approach in Section 5.2.

[18] See Table C2 for earnings impacts by age; ages 19-22 are excluded as there is limited evidence of significant effects during college years.

[19] Arkansas was the earliest-adopting state (1991), allowing up to seven pre-event cohorts, while Tennessee was the latest adopter (2003), allowing up to eleven post-event cohorts.

[20] Prior research (Sjoquist and Winters, 2015b) and Table C3 show no significant effects on extensive margin college enrollment with Census and ACS data, mitigating concerns about sample selection bias.

[21] I drop observations with imputed information for age, birth state, education, employment status, or earnings because all of these variables are central to the analysis. Group quarters were not included in the ACS from 2000-2005.

[22] Summary statistics and balance tests between the 10 strong merit and 25 non-merit states are shown in Tables C4 and C5, respectively.

[23] This exposure measure introduces measurement error, as some individuals attend high school outside their birth state, which attenuates estimates toward zero. Using Census and ACS data for high school age individuals (ages 15-17), I estimate that approximately 80 percent reside in their birth state nationwide.

## 5.1 Main Empirical Specifications

The identification strategy exploits the staggered adoption of state merit aid programs, using variation across states and cohorts in program exposure.. This generalized difference-in-differences (DID) approach is estimated via a two-way fixed effects (TWFE) specification.

For the college enrollment outcomes from IPEDS, the specification is as follows:

$$log(E_{sc}) = \gamma_s + \pi_c + \phi Merit_{sc} + \epsilon_{sc}, \quad (6)$$

where $log(E_{sc})$ is the logarithmic transformation of the number of college enrollees from residential state $s$ in cohort $c$;[24] $\gamma_s$ represents the residential state fixed effects and controls for time-invariant fixed differences between states; $\pi_c$ represents the cohort fixed effects and controls for time-varying cohort effects common to all states; $Merit_{sc}$ is an indicator for residential state $s$ being a strong merit state and cohort $c$ enrolling in college after program adoption; and $\epsilon_{sc}$ is the error term, which is clustered at the residential state level. $\phi$ estimates the effect of merit program adoption on log out-of-state, log in-state, and log total enrollment for two-year, four-year, and all colleges.[25]

For long-term outcomes from the Census, the specification is as follows:

$$Y_{isct} = \gamma_s + \pi_c + \beta X_{isct} + \lambda E_{st} + \delta_s T_{sc} + \theta Merit_{sc} + \zeta_{isct}, \quad (7)$$

where $Y_{isct}$ is the outcome (e.g., employment, earnings) for individual $i$ in birth state $s$ and birth cohort $c$ at survey year time $t$; $\gamma_s$ represents the birth state fixed effects and controls for time-invariant fixed differences between birth states; $\pi_c$ represents the birth cohort fixed effects and controls for time-varying cohort effects common to all birth states; $X_{isct}$ is a set of demographic controls; $E_{st}$ is a set of economic controls; $T_{sc}$ are birth state by birth

[24] College enrollment is recorded for students who graduated high school in the previous 12 months and are first-time degree-seeking freshmen. Consequently, the Fall enrollment cohort year, high school graduation cohort year, and survey year all refer to the same year.

[25] I assess robustness using the Sun and Abraham (2021) heterogeneity-robust estimator, specifying never-treated non-merit states as the control group. Results are shown in Table C6 and discussed in Section 6.1.

cohort linear time trends; $Merit_{sc}$ is an indicator for being born in a strong merit state and reaching age 18 after program adoption; and $\zeta_{isct}$ is the error term, which is clustered at the birth state level. The demographic controls included in $X_{isct}$ are indicators for gender, race, gender-race interactions, and age. The economic controls included in $E_{st}$ are labor force participation and employment rates as well as the 25th, 50th, and 75th percentile of earnings measured at the state by survey year level.[26]

$\theta$ estimates the impact of being born in a strong merit aid state and reaching age 18 after merit program implementation. This represents an ITT exposure effect because only students meeting the eligibility criteria in strong merit states have the opportunity to receive merit aid and only some eligible students use their merit awards, which are conditional on attending college in-state. ITT is the policy-relevant parameter since this estimates the effect of making merit aid available to students in merit states. I also report implied treatment-on-the-treated (TOT) effects by scaling the ITT estimates by the share of college attendees in strong merit states receiving aid, assuming take-up rates are stable across cohorts. In addition to main ITT specifications, I estimate parameterized effects of merit aid programs using measures of program intensity, including the share of students receiving aid and average aid per recipient. These specifications allow me to explore dose-response relationships between program reach, generosity, and outcomes.

## 5.2 Event Study for Parallel Trends

In generalized DID settings with multiple treatment groups and staggered treatment timing, the main identifying assumption is the staggered parallel trends assumption. This assumption requires that, absent the adoption of strong merit programs, outcomes such as log college enrollment, employment, and earnings would have evolved in parallel across treated and control units (Roth et al., 2023). Treated units are the 10 strong merit states, while control units include the 25 non-merit states and unexposed cohorts within strong

[26] I also assess robustness using the Sun and Abraham (2021) heterogeneity-robust estimator for labor market outcomes. Results are shown in Table C7 and discussed in Section 6.2.

merit states.

The main threat to this identifying assumption is non-parallel trends in college enrollment and labor market outcomes between the treatment and control groups. I follow the literature and test for parallel pre-trends with a nonparametric event study specification. The event study approach is similar for college enrollment and labor market outcomes, so I specify the approach for labor market outcomes for simplicity. The specification is as follows:

$$Y_{isct} = \gamma_s + \pi_c + \psi_n \sum_{n=-7}^{11} \mathbb{1}(EventTime_{sc} = n) + \xi_{isct}, \tag{8}$$

where $Y_{isct}$ is the outcome (e.g., earnings) for individual $i$ in birth state $s$ and birth cohort $c$ at survey year time $t$; $\gamma_s$ represents the birth state fixed effects and $\pi_c$ represents the birth cohort fixed effects; $EventTime_{sc}$ is an event-time variable ranging from -7 to 11, with indicators constructed for each event year relative to program adoption;[27] and $\xi_{sc}$ is the error term, which is clustered at the birth state level. $\psi_n$ estimates the effect of strong merit aid exposure on earnings $n$ years before or after program adoption.[28]

Because treatment timing is staggered across states, TWFE estimates may incorporate comparisons between units treated at different times and be sensitive to treatment effect heterogeneity. To address this concern, I assess the robustness of my results to the Sun and Abraham (2021) heterogeneity-robust estimator. In this specification, the never-treated non-merit states are used as the control group, so treatment effects are identified relative to a stable untreated comparison group and avoid comparisons with already-treated cohorts. Event study estimates are discussed in Sections 6.1 and 6.2 and show TWFE and Sun and Abraham (2021) estimates side-by-side.

[27] The event-time variable is centered at -1 for the cohort that reached age 18 in the year before adoption, so negative values correspond to pre-adoption cohorts and zero or positive values to post-adoption cohorts.

[28] For the college enrollment event study, event time is grouped into two-year bins to reflect the biennial structure of the IPEDS survey. The specification for the college enrollment event study is otherwise similar to the specification in Equation 8 with the relevant state and cohort fixed effects incorporated into the model.

# 6 Results

The results section begins with merit program impacts on college enrollment and educational attainment (Section 6.1), followed by estimated effects on employment, earnings, and state residence (Section 6.2). Section 6.3 examines heterogeneity by race and evaluates the mechanisms underlying labor market impacts. Finally, Section 6.4 presents robustness checks.

## 6.1 Effects on College Enrollment and Educational Attainment

**Effects on College Enrollment.** Figure 2 presents event study estimates of the impact of strong merit program adoption on log college enrollment across college sectors.[29] For two-year colleges, there is some evidence of decreasing pre-trends in out-of-state enrollment and increasing pre-trends in in-state enrollment; post-adoption effects largely follow these trends, so causal interpretation is limited. For four-year colleges, pre-trends are flat, and post-adoption effects indicate a decrease in out-of-state enrollment of about 20 percent and in-state enrollment rises by about 15 percent (marginally significant). For all colleges, modest pre-trends are driven primarily by two-year colleges, while post-adoption effects suggest a gradual shift from out-of-state to in-state enrollment, reaching roughly a 20 percent enrollment shift over time. Overall, the event studies provide strong evidence of enrollment shifts into four-year colleges in strong merit states, with limited evidence of effects in two-year colleges.

Table 2 presents TWFE point estimates for log enrollment. Panels A-C report out-of-state, in-state, and total enrollment, respectively; columns (1)-(3) show two-year, four-year, and all colleges. For two-year colleges, estimates are large but imprecise: out-of-state enrollment declines are negative but insignificant, while in-state and total enrollment increases are marginally significant, though pre-trends suggest caution. For four-year colleges, out-of-state enrollment falls by roughly 19 percent, while in-state enrollment increases by 15

[29]For both the TWFE and Sun and Abraham (2021) estimators, point estimates and confidence intervals are nearly identical.

percent (marginally insignificant), and total enrollment changes are small. For all colleges, out-of-state enrollment falls by 19 percent, in-state enrollment rises by 16 percent (marginally significant), and total enrollment effects are modest and insignificant. Results are robust to the Sun and Abraham (2021) estimator. These estimates provide similar or even stronger evidence of enrollment shifts from out-of-state to in-state for four-year and all colleges.[30] Given the wide confidence intervals for college enrollment, the main takeaway is directional: strong merit programs appear to shift enrollment from out-of-state to in-state four-year colleges.[31]

**Effects on Educational Attainment.** Next, I examine how exposure to strong merit programs affects educational attainment using data from the Census and ACS. Subsequent analyses of labor market outcomes, heterogeneity, and mechanisms also rely on these data. Estimates are reported for the full sample as well as separately for females and males. Table 3 presents estimated impacts of strong merit program exposure on four outcomes: completing at least one year of college (College 1+ Years, column(1)), earning an associate's degree or higher (AA+, column (2)), a bachelor's degree or higher (BA+, column (3)), and a master's degree or higher (MA+, column (4)).

For most attainment outcomes, there is little evidence of meaningful effects in these samples: estimates are generally precise and centered near zero. The primary exception is completion of at least one year of college. Exposure to a strong merit program increases the likelihood of completing one year of college by nearly 1 percentage point (p.p.) overall, with the effect concentrated among males. For males, exposure raises completion of one year of college by 1.4 p.p. and is statistically significant, while the estimate for females is smaller and statistically insignificant. These results suggest that merit aid exposure modestly reduces first-year college dropout, particularly for males. In contrast, there is little evidence that

[30]Table C6 compares TWFE and Sun and Abraham (2021) estimates. For two-year colleges, point estimates shrink and lose marginal significance under Sun and Abraham (2021), suggesting this estimator better addresses differential pre-trend concerns. For four-year and all colleges, estimates are largely unchanged, with slightly smaller and considerably more precise point estimates.

[31]Table C8 shows that four-year colleges in strong merit states have lower post-college earnings, graduation rates, and slightly lower average SAT scores compared to out-of-state colleges. This suggests merit aid may reduce college selectivity, although individual-level shifts cannot be observed.

merit program exposure affects degree completion in these samples. Estimates for AA+, BA+, and MA+ are small and statistically indistinguishable from zero for both females and males.[32][33]

Appendix Figure C2 presents the corresponding event study estimates for College 1+ Years and BA+. The event studies provide modest evidence of increases in completing one year of college, concentrated among males, but show no consistent evidence of gains in BA+ completion. Some positive pre-trends among females for College 1+ Years warrant caution in interpreting those estimates as fully causal. For BA+, coefficients are generally near zero, with some negative post-event estimates of roughly 1 p.p.[34] Overall, there is no evidence that strong merit programs increase BA completion and, if anything, occasional negative post-event estimates may suggest small declines in BA receipt.

Taken together with the enrollment results, these findings indicate that strong merit programs primarily reallocate students across institutions rather than increase overall educational attainment. While exposure reduces first-year dropout, it does not raise bachelor's degree completion. Because enrollment shifts are concentrated toward in-state four-year colleges that are less selective on average, the results are consistent with changes in student sorting across institutions without corresponding gains in accumulated human capital. Such reallocation may alter the quality of student-college matches, with potentially important implications for long-term labor market outcomes.

## 6.2 Effects on Employment, Earnings, and State Residence

Given the observed shifts in college enrollment and the limited evidence of gains in degree completion, I next examine whether these enrollment shifts translate into differences in labor market outcomes. I begin by examining the effects of strong merit program exposure

[32] Table C9 reports estimates across alternative specifications. Results are qualitatively similar and are discussed further in Section 6.4.

[33] These findings are consistent with prior work, including Sjoquist and Winters (2015b) and Carruthers and Özek (2016), which find limited evidence of effects on college degree completion.

[34] Some of these negative estimates are statistically significant under the Sun and Abraham (2021) estimator, while TWFE estimates are less precise.

on employment and earnings. I then assess how program reach and generosity relate to labor market outcomes. Finally, I explore impacts on both extensive and intensive margin measures of employment, as well as state of residence.

**Main Employment and Earnings Results.** Figure 3 presents event study estimates of strong merit program exposure on employment and earnings for separately for females and males.[35] For females, pre-period employment coefficients are small and generally statistically insignificant, though there is modest evidence of a slight upward trend prior to treatment. In the post-period, this pattern reverses: most coefficients are negative and statistically significant, implying employment declines of roughly 0.5 to 1 p.p. Female earnings follow a similar pattern. Pre-period estimates are close to zero with some noise, while post-period coefficients are consistently negative, and often statistically significant, suggesting earnings reductions of approximately $500. Overall, the estimates indicate a post-treatment reversal in labor market outcomes for females.

For males, the evidence is weaker and less consistent. Pre-period coefficients for employment fluctuate around zero without a clear monotonic trend, though two estimates are significantly positive. Post-period employment effects are generally small and statistically insignificant. For earnings, the pre-period includes two negative coefficients that are on the margin of significance (around -$500), suggestive of pre-existing divergence. Post-period estimates are modestly negative in some years but not consistently significant. Taken together, there is limited evidence of meaningful labor market effects for males, and the pre-period instability, particularly for earnings, motivates the inclusion of demographic, economic, and linear trend controls in subsequent TWFE specifications.

Table 4 reports estimates of the effect of strong merit program exposure on employment across alternative specifications. Column (1) includes birth state and birth cohort fixed effects, column (2) adds demographic controls, column (3) adds state-by-year economic controls, and column (4) further incorporates linear trend controls. For females, strong merit

[35] Employment is defined as having positive earnings over the previous year and earnings are trimmed at the 90th percentile. See Section 4 for outcome definitions.

program exposure significantly reduces employment across all specifications. The baseline estimate in column (1) implies a 1 p.p. decline, and the magnitude is unchanged when adding demographic controls. Including economic controls attenuates the estimate modestly to about 0.8 p.p., with little additional change after adding linear trend controls. In the fully specified model, the implied TOT corresponds to a 3.4 p.p. decrease in employment. For males, employment effects are consistently close to zero and precisely estimated across specifications. Tests of equality reject that the female and male coefficients are the same, with p-values below 0.05 in all cases.

Table 5 presents analogous estimates for earnings. Columns (1)-(4) mirror the specifications above, while column (5) restricts the sample to employed individuals to isolate intensive margin effects. For females, strong merit program exposure significantly reduces earnings when measured across all females, including those not employed (i.e., accounting for the extensive margin). Point estimates are stable across specifications. In the fully controlled model, earnings decline by about \$500 (1.9 percent decrease), implying a TOT decrease of approximately \$2,000 (7.7 percent decrease). Restricting to employed females attenuates the estimate by about half and the result is no longer statistically significant, indicating that the earnings effect is driven largely by reduced employment. The remaining negative point estimate suggests that intensive-margin adjustments may also contribute.

For males, earnings effects are smaller and statistically insignificant. Estimates in columns (1)-(3) are modestly negative, and the inclusion of linear trend controls in column (4) increases the magnitude to about -\$250 (-0.63 percent), corresponding to a TOT of roughly -\$1,000 (-2.5 percent). Restricting to employed males yields a similar estimate, consistent with the absence of employment effects. Tests of equality between female and male earnings effects yield mixed results: estimates are at least marginally significantly different in columns (1)-(3), but not in the fully controlled specifications. Overall, the evidence indicates economically meaningful and robust labor market declines for females, while effects for males are smaller and statistically indistinguishable from zero.

Most estimates are similar in sign and magnitude across specifications, with modest attenuation when economic and linear trend controls are included. Column (4) in Tables 4 and 5 presents the preferred specification. This specification strengthens identification in three ways. First, it incorporates demographic, state-by-year economic, and linear trend controls, accounting for compositional differences, evolving state economic conditions, and birth state-by-cohort trajectories.[36] These additions are particularly important given some instability in pre-period event study estimates. Second, estimates are generally more precise when economic and linear trend controls are included. Third, this specification typically yields more conservative estimates relative to models that omit these controls.[37] Consistent with Equation 7, this is the preferred specification used throughout most of the remainder of the analysis.

I next assess the robustness of these findings to the Sun and Abraham (2021) estimator in Appendix Table C7. Employment and earnings estimates are similar in sign, magnitude, and statistical significance to the TWFE results. For comparison, the table also reports effects on a survey-based measure of labor force participation. Under TWFE, I estimate a significant decline in labor force participation for females similar in magnitude to the employment results. However, using the Sun and Abraham (2021) estimator, the estimate becomes slightly positive but near zero and is substantially less precise. This imprecision likely reflects the fact that labor force participation is measured based on current survey status, whereas my primary employment measure is based on positive earnings over the previous year and captures a more persistent measure of labor supply.

**Parameterized Estimates.** Next, I report parameterized estimates of merit program impacts on labor market outcomes, shown in Table 6. Exposure is measured by program intensity, with the key variables being the share of students receiving merit aid (reach) and merit aid per recipient (generosity). Columns (1) and (3) include the 10 strong merit aid

[36] See Section 5.1 for details. Economic controls include labor force participation and employment rates, as well as the 25th, 50th, and 75th percentiles of earnings at the state-by-survey-year level.

[37] An exception is male earnings, where estimates become less precise and somewhat larger in magnitude when linear trends are included.

states and 25 non-merit states, while columns (2) and (4) additionally include the 15 weak merit states among the treatment states. For females, program reach drives the labor market effects. A 10 p.p. increase in the share of students receiving merit aid reduces employment by about 0.3 p.p. and earnings by about $200. These effects are highly significant and largely unchanged when weak merit states are included. Program generosity has more limited effects: a $1,000 increase in aid per recipient slightly decreases employment (0.1 p.p.) and earnings ($80) in the strong merit states, but these effects become insignificant when weak merit states are added. For males, there is no evidence of employment effects. Earnings estimates are negative but insignificant across samples, with a 10 p.p. increase in reach corresponding to $77-$148 lower earnings and a $1,000 increase in generosity corresponding to a $40-$66 earnings decrease.

Figure 4 presents estimated impacts of the binary exposure measure, shown separately by quintiles of merit states based on program reach and generosity. Each point estimate compares individuals in a given quintile to all 25 non-merit states, illustrating how estimated effects vary with program intensity.[38] These results complement Table 6. For females, employment and earnings effects increase with higher reach, though not monotonically: effects are near zero in the first and third quintiles (0-2% and 8-16%) and largest in the fourth and fifth quintiles (16-44%). For males, no clear pattern emerges for employment, though a small negative effect appears in the fourth reach quintile; earnings show modest negative effects in all but the third reach quintile, which has an estimate near zero.

For generosity quintiles, patterns are more nuanced. For females, employment and earnings effects are largest in the first and second quintiles ($0K-1.75K) and near zero in the third and fourth ($1.75K-4.4K), reflecting that some states with lower generosity have higher reach. The fifth quintile ($4.4K-6.3K) shows a negative employment effect but earnings near zero. For males, there is no consistent relationship between generosity and labor market outcomes, with employment effects near zero and earnings effects only marginally significant

[38]Each quintile contains five merit states.

in some cases.

Overall, these parameterized results reinforce the main findings from the TWFE and event study analyses: labor market impacts are concentrated among females and are primarily driven by program reach rather than generosity, underscoring the importance of broad exposure to merit programs in shaping outcomes at scale.[39] The results also support the use of the binary exposure measure comparing the strong merit states to non-merit states, as this approach captures meaningful variation in exposure to merit aid programs.

One limitation of the research design is that exposure is proxied using birth state in nationally representative data. Because some states offer highly generous awards to a relatively small share of students, estimated effects in those states may be attenuated, mechanically emphasizing the role of reach. This suggests that generosity may also influence long-run labor market outcomes, particularly in high-reach contexts.[40]

**Effects on Other Employment Outcomes and State Residence.** Next, I examine impacts on alternative extensive- and intensive-margin employment outcomes as well as state residence (Table 7). Column (1) reports employment based on positive annual earnings, column (2) employment at the time of the survey, column (3) weeks worked, and column (4) hours worked per week.[41] Columns (5) and (6) report weeks worked and hours per week restricted to those who are employed based on positive earnings to isolate intensive-margin responses. Column (7) examines the likelihood of residing in one's birth state.

For females, strong merit aid exposure reduces employment as measured by positive annual earnings but has a precisely estimated zero effect on employment at the time of the survey. This pattern suggests that impacts operate through more persistent reductions in labor force attachment rather than through short-term unemployment spells.[42] Exposure

[39]These findings are consistent with Cohodes and Goodman (2014), which finds that being eligible for a relatively modest merit scholarship in Massachusetts decreased college selectivity and degree receipt.

[40]When restricting the treatment group to strong merit states, where program reach is generally higher, a $1,000 increase in generosity leads to significant declines in female employment and earnings. Including weak merit states in the treatment group attenuates these estimates, as some of these states combine relatively generous awards with limited reach, diluting the relationship between generosity and exposure at scale.

[41]Column (1) replicates the preferred specification in Table 4, column (4), and is included for comparison.

[42]Employment at the time of the survey is coded as missing for individuals out of the labor force.

decreases weeks worked by 0.4 weeks and hours per week by approximately 0.6 hours.[43] Restricting to employed females, the effect on weeks worked attenuates to near zero, while the effect on hours per week attenuates to a decrease of 0.25 hours and is marginally insignificant. Together, these results indicate that reductions in weeks worked are driven primarily by extensive-margin adjustments, while reductions in hours reflect both extensive- and intensive-margin responses.

For males, I find significant negative effects on employment at the time of the survey (1 p.p. decrease) and weeks worked (0.3 weeks decrease), but no effect on employment measured by positive annual earnings.[44] This pattern suggests that exposure increases the likelihood of short unemployment spells without affecting longer-term labor force attachment. Consistent with this interpretation, estimates for weeks worked conditional on positive annual earnings are similar in magnitude to the unrestricted sample estimate, indicating that the reduction occurs largely among employed males. Overall, the small and insignificant earnings effects are consistent with limited impacts on longer-term labor force attachment.

Finally, strong merit program exposure does not significantly affect the likelihood of residing in one's birth state. Estimates are modestly positive and similar for females and males. In the pooled sample, the point estimate shows a 0.9 p.p. increase in the probability of residing in the birth state.[45] These findings indicate that strong merit programs have limited long-run effects on the retention of in-state residents.

## 6.3 Heterogeneity and Mechanisms

Next, I investigate effect heterogeneity by race-gender subgroups and potential mechanisms that may explain labor market effects, including college selectivity, family formation,

[43]Event study estimates in Figure C3 are consistent with these results, showing little effect on employment at the survey date and negative effects on weeks and hours worked.

[44]Event study estimates in Figure C3 show negative impacts on employment at the survey date and weeks worked while showing little effect on hours worked.

[45]Event study estimates in Figure C4 provide suggestive evidence of positive effects on the likelihood of residing in one's birth state for females. Pre-treatment coefficients exhibit some variability, while most post-treatment estimates are positive and several are statistically significant. For males, the event study shows no clear or consistent effects on state residence.

and occupational choice.

**Heterogeneity.** Table 8 presents heterogeneity results for educational attainment, having a child, and labor market outcomes. Panels A-D report estimates for White females, Nonwhite females, White males, and Nonwhite males, respectively.[46] For White females, there is a clear and consistent pattern. Program exposure reduces educational attainment at multiple levels: associate's degree attainment decreases by 0.8 p.p. (marginally significant), bachelor's attainment falls by 0.6 p.p. (not significant), and master's attainment declines by 0.6 p.p. (significant). Results also show increases in childbearing by nearly 1 p.p. as well as decreases in employment by 0.8 p.p. and earnings by approximately $350. For Nonwhite females, results are more nuanced. Exposure significantly increases completion of at least one year of college, while effects on higher levels of educational attainment are positive but statistically insignificant. There is no significant effect on childbearing. However, employment declines by 0.8 p.p. and earnings fall by roughly $620, with both of these effects marginally significant.

For White males, results are mixed: exposure increases completion of at least one year of college and attainment of an associate's degree or higher but decreases completion of a master's degree or higher. Earnings effects are negative but statistically insignificant. For Nonwhite males, exposure significantly increases the likelihood of having a child, while estimates for educational attainment and earnings are statistically indistinguishable from zero.

Overall, the heterogeneity results reveal a particularly strong and internally consistent pattern for White females: strong merit program exposure reduces postsecondary degree completion, increases childbearing, and lowers employment and earnings. In contrast, effects for other gender-race subgroups are less systematic.

These findings are broadly consistent with prior research. Cohodes and Goodman (2014) shows that negative effects of merit program eligibility on college selectivity and persis-

[46] Employment is measured using positive earnings over the previous year.

tence are concentrated among White students, those from relatively higher socioeconomic backgrounds, and middle-achieving students.[47] While Cohodes and Goodman (2014) do not find significant gender differences, other research suggests that college selectivity has limited long-term effects for men but substantially influences women's advanced degree attainment, family formation, and labor market outcomes (Ge et al., 2022; Carlana et al., 2024; Kirkeboen et al., 2025). Consistent with this literature, my results suggest that by shifting students toward less selective institutions, strong merit programs have particularly consequential long-run effects for White women.

**Mechanisms.** I examine several mechanisms linking strong merit program exposure to long-run labor market outcomes. The evidence suggests that reduced college selectivity could feasibly be a primary channel, particularly for women, while family formation and occupation sorting play more limited roles.

A central implication of my main results is that strong merit programs shift enrollment toward in-state colleges that are less selective on average. Prior research indicates that college selectivity has especially large effects on women's long-run outcomes (Ge et al., 2022). Applying the estimates from this prior research to my results suggests that a 147 point decline in average institutional SAT scores would fully explain my TOT employment effect of a 3.4 p.p. decrease, while a 55 point decline fully explains my TOT earnings effect of a 7.7 percent decrease. While a decline in selectivity of this size would be large, it is not implausible, and observed shifts in enrollment suggest there may be a considerable decrease in college selectivity. These calculations suggest that changes in college selectivity could plausibly account for a substantial share of the female labor market effects.

I next assess whether changes in family formation contribute to the observed female labor market impacts. Table C10 shows that strong merit program exposure modestly increases the likelihood of having any children, with a marginally significant increase of 0.6 p.p. for women (TOT increase of 2.5 p.p.). There is no evidence of effects on marriage or on the intensive

[47] Ideally, I would also examine heterogeneity by socioeconomic status and academic achievement; however, these characteristics are not observed in Census and ACS survey data.

margin of childbearing.[48] To assess the importance of this channel, I estimate labor market effects controlling for childbearing. Including this control attenuates the female employment and earnings estimates by approximately 11 and 13 percent, respectively (Table C11). These results indicate that family formation explains a modest but non-negligible portion of the female labor market effects, but it is unlikely to be the primary driver.

Occupation choice plays a more important role for men. Table C12 shows no meaningful effects on women's major or occupation choices. For men, however, strong merit program exposure significantly reduces major and occupation predicted earnings by about 0.7 percent (TOT decrease of 3 percent).[49] To examine this channel, I estimate labor market effects controlling for occupational choice. I find that the inclusion of the STEM occupation control explains 23 percent of the earnings decline for men, and including occupation fixed effects fully absorbs the negative earnings estimate (Table C13). This evidence indicates that occupational sorting is an important mechanism for male earnings effects, consistent with muted impacts on male employment.

Finally, changes in college environments may influence outcomes through social norms that shape aspirations and life choices, particularly for women. Strong merit states are disproportionately located in the South, where traditional gender norms are more prevalent. Balance tests using the female subsample of the Census and ACS data (Table C14) show that women born in strong merit states are more likely to marry earlier and have children. While these differences reflect broader regional culture rather than causal effects of merit aid, they suggest that colleges in strong merit states may foster environments that emphasize family formation over career advancement.[50] These institutional and cultural differences may

[48]Event study estimates in Figure C5 show limited evidence of significant effects on childbearing and marriage.

[49]Event study estimates in Figure C6 show suggestive evidence of a decrease in STEM occupations and strong evidence of a significant decrease in occupation predicted earnings for males. This furthers prior research that shows strong merit program exposure reduces STEM degree receipt and major predicted earnings for men (Sjoquist and Winters, 2015a). Other research shows that stricter GPA requirements for scholarship renewal contribute to students' shift away from more difficult STEM majors (Jia, 2019).

[50]Prior research shows that gender norms strongly mediate the divergence in labor market outcomes between women and men following childbirth, with larger gaps in more culturally conservative areas (Kleven, 2025).

amplify the labor market consequences of reduced college selectivity for women.

## 6.4 Robustness

I assess robustness along four dimensions: (i) compositional changes across states, (ii) sample selection, (iii) earnings trimming, and (iv) alternative specifications for educational attainment.

**Compositional Changes.** A central identification concern is that strong merit programs may alter the composition of residents in treated states, such as by increasing the in-state retention of college-educated individuals. However, earlier results show no effect of program exposure on the likelihood of residing in one's birth state (Table 7; Figure C4), suggesting no systematic migration response. In addition, Table C15 shows no significant changes in gender, racial, or ethnic composition. Together, these results indicate that compositional changes are unlikely to bias the estimated labor market effects.

**Sample Selection.** Because the main analysis focuses on college attendees in the Census and ACS, another concern is selection bias if strong merit programs increase college attendance among marginal students with weaker labor market prospects. Table C3, which expands the sample to include high school graduates, shows no effect of strong merit exposure on college enrollment or degree attainment. This suggests that the college-attendee restriction does not mechanically generate the negative labor market effects.

Table C16 further examines labor market outcomes across three samples: college attendees only, high school graduates only, and the combined sample. Estimates for high school graduates only, who are mostly unaffected by merit aid, serve as a placebo test. These estimates are near zero, which supports that main results are unlikely to be driven by non-parallel trends in outcomes. When both groups are combined, female employment and earnings effects attenuate, as expected given considerable measurement error in exposure for the high school graduate only sample, but remain statistically significant. These patterns reinforce that the main results are driven by program exposure among college attendees.

**Earnings Trimming.** Figure C1 evaluates sensitivity to alternative earnings trimming thresholds, ranging from excluding earnings above the 80th percentile to including the full earnings distribution. Female earnings effects are negative and stable across percentiles, and in specifications with controls the magnitude increases slightly when higher percentiles are included. Estimates without controls are less precise but similar in magnitude and highly stable. Male earnings effects remain small and stable across specifications. Overall, the earnings results are not sensitive to trimming choices.

**Educational Attainment Specifications.** Table C9 examines alternative specifications for completing at least one year of college and a BA degree or higher. Across models with varying controls, there is no consistent evidence that strong merit exposure increases BA attainment, and some specifications even show significant reductions in BA receipt for females. While some specifications suggest small positive effects on completing one year of college, these are not robust. Taken together, the results do not indicate meaningful positive effects on degree attainment under alternative specifications.

# 7 Welfare Analysis

This section evaluates the welfare effects of state merit aid by estimating the marginal value of public funds (7.1) and analyzing revenue neutrality to assess the distribution of fiscal impacts across governments (7.2).

## 7.1 Marginal Value of Public Funds

This welfare analysis estimates the net benefits of strong merit programs to recipients relative to net government costs by computing the marginal value of public funds (MVPF), which compares recipients' willingness to pay (WTP) to net government costs (G). I follow the approach in Hendren and Sprung-Keyser (2020), who apply the MVPF to evaluate a range of policies, including individual state merit aid programs. I extend this work by using directly estimated earnings impacts to estimate an aggregate MVPF for all strong state merit aid programs.[51]

**Welfare Analysis Approach.** Recipients' WTP is modeled as the sum of changes in students' private educational costs and earnings impacts net of taxes. Changes in educational costs are computed separately for "affected" students, those who shift from 4-year out-of-state to 4-year in-state colleges, and "unaffected" students, who remain in-state. For affected students, educational costs change due to both their enrollment shift and merit award receipt, whereas for unaffected students costs change only through receipt of the merit award. Earnings impacts net of taxes are computed for three age ranges: 19-22, 23-34, and 35-65, using TOT estimates for the younger ages and projections for ages 35-65.

Net government costs are modeled as the sum of changes in government educational spending and changes in tax revenue driven by earnings impacts. Changes in government educational expenses are computed separately for affected and unaffected students. For affected students, government cost changes reflect both the enrollment shift and merit award expenses, while for unaffected students they reflect only merit award expenses. Tax effects are computed for the same age ranges as earnings impacts. Details on data sources, assumptions, and computations are in Appendix B.1.

**Welfare Analysis Results and Discussion.** Welfare analysis results are shown in Figure 5. Strong state merit aid reduces students' private tuition payments by approximately \$6,500, generating a component of WTP that is positive. However, these benefits are more than offset by negative earnings effects. Earnings decline modestly at ages 19-22, by nearly \$10,000 at ages 23-34, and by almost \$20,000 over ages 35-65. Aggregating across components, I estimate a total WTP of approximately -\$23,400.

Net government costs (G) increase due to both higher program and enrollment expenditures and lower tax revenue from reduced earnings. Government spending on program and enrollment costs rises by roughly \$8,000 per student. Tax revenue losses are small at ages

[51] Main welfare results use estimates from the full sample (Figure 5), while additional results by gender are shown in Figure C7.

19-22 but amount to about $2,200 at ages 23-34 and $4,500 at ages 35-65. Overall, I estimate an increase in net government costs of nearly $15,000.

Dividing WTP by G yields an MVPF of approximately -$1.57, implying a loss of $1.57 in recipient welfare for every dollar of government spending. The 95 percent confidence interval ranges from -$2.69 to $1.98, so the full-sample estimates are not precise enough to rule out a modest positive return. Gender-specific estimates (Figure C7) are similar in magnitude. For females, the MVPF is -$1.82 and precisely estimated, allowing rejection of a positive return. For males, the point estimate is -$1.39 but substantially less precise.[52]

The main implication is that strong merit aid programs do not generate positive economic returns despite substantial public spending per student. Reductions in private tuition are outweighed by losses in lifetime earnings, and exposure to these programs does not meaningfully increase long-run residence in recipients' birth states. The results of this welfare analysis suggest that these programs are ineffective tools for human capital development or state economic retention.

These findings complement Hendren and Sprung-Keyser (2020), who report MVPFs between $0.72 and $4.00 for certain state merit programs, with positive estimates driven by enrollment gains or tuition reductions. Using directly estimated lifetime earnings impacts, I find that when strong merit programs are evaluated in aggregate, returns are negative. Consistent with Hendren and Sprung-Keyser (2020), other policies with negative MVPFs tend to involve higher education subsidies that do not meaningfully increase attainment, suggesting that poorly targeted aid and program-induced distortions are common features of policies with negative returns.

[52]The 95 percent confidence intervals for the MVPF are constructed using a bootstrap procedure based on the estimated coefficients and standard errors in my main analysis. For males, the upper bound is unbounded (infinite), reflecting bootstrap draws in which sufficiently large positive earnings effects generate enough additional tax revenue to imply net government savings.

## 7.2 Revenue Neutrality Ratio

Beyond comparing recipient benefits to net government costs, it is important to understand the fiscal impact of strong merit programs across levels of government. I examine how the policy affects lifetime tax revenue for: (i) the in-state government of a state that implements a strong merit program, (ii) out-of-state governments in all other states that do not implement the program, and (iii) the federal government.[53] For each level of government, I compare lifetime tax revenue under the policy to the revenue that would have been collected in the absence of program adoption. I summarize these effects using a Revenue Neutrality Ratio (RNR), defined as:

$$RNR = \frac{Policy\ Revenue\ (PR)}{Counterfactual\ Revenue\ (CR)}. \tag{9}$$

An $RNR$ equal to one implies that policy-induced revenue fully offsets net government costs.

**Revenue Neutrality Approach.** $Counterfactual\ Revenue\ (CR)$. Counterfactual revenue is constructed by applying relevant tax rates to the distribution of earnings for individuals from ages 19 to 65 in strong merit states prior to program adoption. This yields the present discounted value of lifetime tax revenue absent the policy.

$Policy\ Revenue\ (PR)$. Policy revenue equals counterfactual revenue plus the net fiscal effects of the program. These include: (i) direct merit program expenditures, (ii) changes in public higher education spending induced by enrollment responses, and (iii) changes in tax revenue due to earnings and migration effects. For the in-state government in a state implementing a strong merit program, $PR$ reflects direct program costs, the state share of enrollment-induced spending changes, and changes in state tax revenue arising from earnings and migration effects. For out-of-state governments, $PR$ includes only earnings- and migration-induced tax changes. For the federal government, $PR$ includes the federal share of enrollment-induced spending changes and federal tax revenue effects from earnings.

[53]Out-of-state governments also collect tax revenue from strong merit program recipients who migrate out-of-state.

Overall, this framework allows me to assess whether the in-state government recovers its costs through increased lifetime tax revenue and whether strong state merit aid generates fiscal spillovers across jurisdictions. Appendix B.2 provides details on the construction of $CR$ and $PR$, including data sources, assumptions, allocation rules, and computations.

**Revenue Neutrality Results and Discussion.** Revenue neutrality results are presented in Figure 6. For the in-state government, counterfactual lifetime tax revenue is approximately \$18,700. Direct merit program expenditures reduce revenue by \$4,700, and enrollment-induced spending increases lower it by an additional \$1,900. Earnings-related tax effects further reduce revenue by about \$600. Increased long-run in-state retention adds roughly \$1,100, offsetting only a small fraction of these costs. As a result, total revenue under the policy is \$12,500, well below the counterfactual benchmark.

The program also generates spillovers to other jurisdictions. Out-of-state governments would have collected \$12,200 in counterfactual lifetime revenue from out-migrating recipients. Under the policy, earnings-related tax effects reduce revenue by about \$400, and reduced outmigration lowers it by a further \$1,100. Revenue under the policy for out-of-state governments thus falls to \$10,700.

For the federal government, counterfactual lifetime tax revenue is \$191,100. The federal share of enrollment-induced spending reduces revenue by \$1,500. Because federal marginal tax rates exceed state rates, earnings-related tax effects have a larger fiscal effect at the federal level, lowering revenue by \$5,900. Total revenue under the policy is \$183,800. Although the absolute revenue loss is larger than for the in-state government, it represents a much smaller proportional decline given the higher federal tax base.

*Revenue Neutrality Ratio* ($RNR$). The $RNR$ summarizes these effects by comparing policy revenue to counterfactual revenue. For all levels of government, the $RNR$ is below one, the value that indicates that policy-induced revenue fully offsets net government costs. For the in-state government, the $RNR$ is 0.67, meaning that policy revenue replaces only

67 cents of each counterfactual dollar; this estimate is statistically different from one.[54] The $RNR$ is 0.88 for out-of-state governments and 0.96 for the federal government, with the latter statistically different from one.

In sum, increased in-state retention generates only modest fiscal benefits for a strong merit state government and does not offset net program costs. Moreover, state merit aid produces negative fiscal spillovers for both the out-of-state and federal governments through its effects on enrollment, taxes, and migration. This analysis extends Hendren and Sprung-Keyser (2020) by demonstrating how the $RNR$ can be used to assess whether policy-induced revenue gains offset costs across levels of government, and by illustrating how spending at one level can shift tax bases across jurisdictions.

# 8 Conclusion

State merit aid programs were created to attract high-performing students to in-state colleges and retain them in state workforces. While states may implement these programs to expand the supply of skilled labor and raise future tax revenue, the awards are costly and may alter students' college choices in ways that affect long-term outcomes. This paper uses variation from the introduction of all major state merit aid programs to estimate effects on college enrollment and longer-term labor market outcomes. Using the resulting earnings estimates, I conduct a welfare analysis to evaluate the long-term returns to state merit aid spending and its fiscal implications for government budgets. This study contributes to the small literature examining the labor market impacts of financial aid[55] and provides new evidence on the welfare consequences of higher education spending for students and governments (Hendren and Sprung-Keyser, 2020).

The introduction of state merit aid shifts enrollment from out-of-state colleges to in-state

[54] The 95 percent confidence intervals for the $RNR$ are constructed using a bootstrap procedure based on the estimated coefficients and standard errors in my main analysis.

[55] See Denning et al. (2019); Black et al. (2023); Bettinger et al. (2019); Carlson et al. (2022); Welch (2014); Scott-Clayton and Zafar (2019).

colleges that are, on average, less selective, without significantly affecting degree attainment. For labor market outcomes, program exposure reduces women's employment by 0.8 p.p. and earnings by about $500 (a 1.9 percent decline), while effects for men are smaller and statistically insignificant. Effects on women are primarily concentrated among White women, consistent with prior research linking college selectivity to labor market outcomes.[56] Mediation analysis suggests that declines in college selectivity plausibly explain a substantial portion of the negative impacts on women's employment and earnings. Welfare and revenue neutrality analysis indicates that earnings losses outweigh reductions in students' educational costs, yielding an estimated $1.57 loss for every dollar of government spending on merit aid. State governments recover only 67 cents in future tax revenue per dollar of counterfactual revenue, with additional negative spillovers for out-of-state and federal governments. Overall, merit aid changes college sorting but generates limited economic returns for students and governments.

Taken together, the results suggest that state merit aid programs as currently designed are ineffective investments for both students and state governments. Merit aid shifts students toward less selective colleges without increasing educational attainment, leading to reductions in lifetime earnings that outweigh the financial benefits of the award. For states, the programs require substantial upfront spending yet fail to increase attainment, reduce brain drain, or raise future tax revenue.

States can improve the efficiency and equity of higher education investments by targeting aid toward financially constrained or academically marginal students. Targeted aid is more likely to increase educational attainment and expand the supply of skilled workers. Prior research shows that well-targeted financial aid programs that raise attainment can increase future earnings and generate fiscal returns that offset program costs (Hendren and Sprung-Keyser, 2020). Recent evidence also highlights the large returns to expanding college access for academically marginal students, particularly by shifting individuals from not attending

[56] See Cohodes and Goodman (2014); Ge et al. (2022); Carlana et al. (2024); Kirkeboen et al. (2025).

college to attending two-year institutions or from two-year to four-year colleges (Mountjoy, 2022, 2026). By directing resources toward these margins, states can improve residents' labor market prospects while generating stronger returns on higher education investments.

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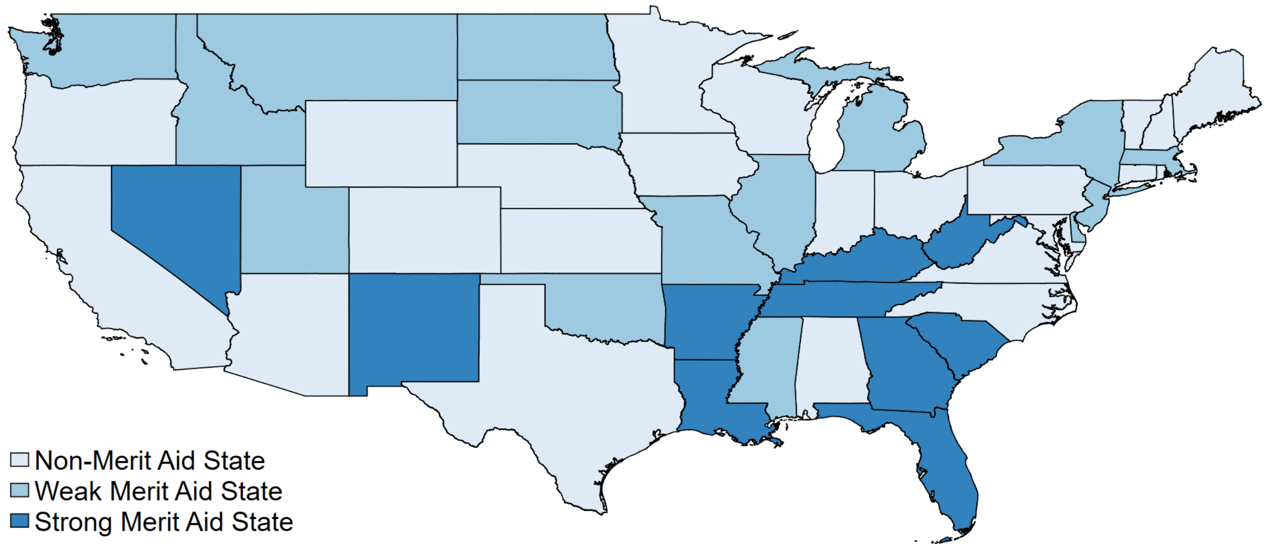
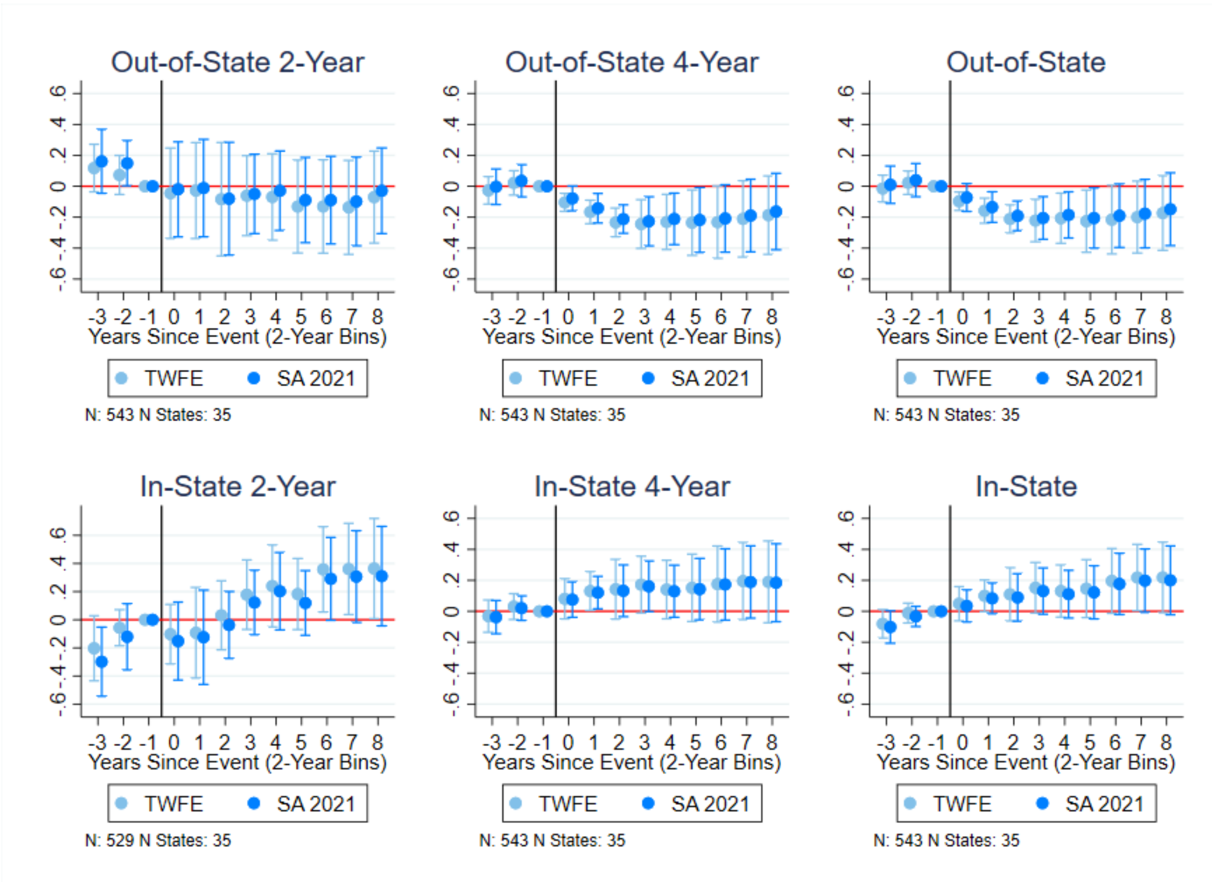
# Figures and Tables

Figure 1: Map of Merit Aid State Classification




*Sources*: NASSGAP (2020), Sjoquist and Winters (2015b), Sjoquist and Winters (2015a), Jia (2019), Baum et al. (2012), and Hawley and Rork (2022).
*Notes*: Hawaii is classified as a non-merit aid state, while Alaska is excluded from the analysis because it adopted a large merit program in 2011, leaving few treated cohorts with observable outcomes. The 10 strong merit aid states established the largest programs, with merit aid spending between \$493 and \$1,755 per full-time equivalent (FTE) student in 2019-2020. The 15 weak merit aid states established smaller programs, with spending between \$11 and \$240 per FTE student in 2019-2020 (NASSGAP, 2020). See Table 1 and Tables A1 and A2 for additional details on state merit aid program spending and eligibility.

Figure 2: Event Study Estimates for Log College Enrollment

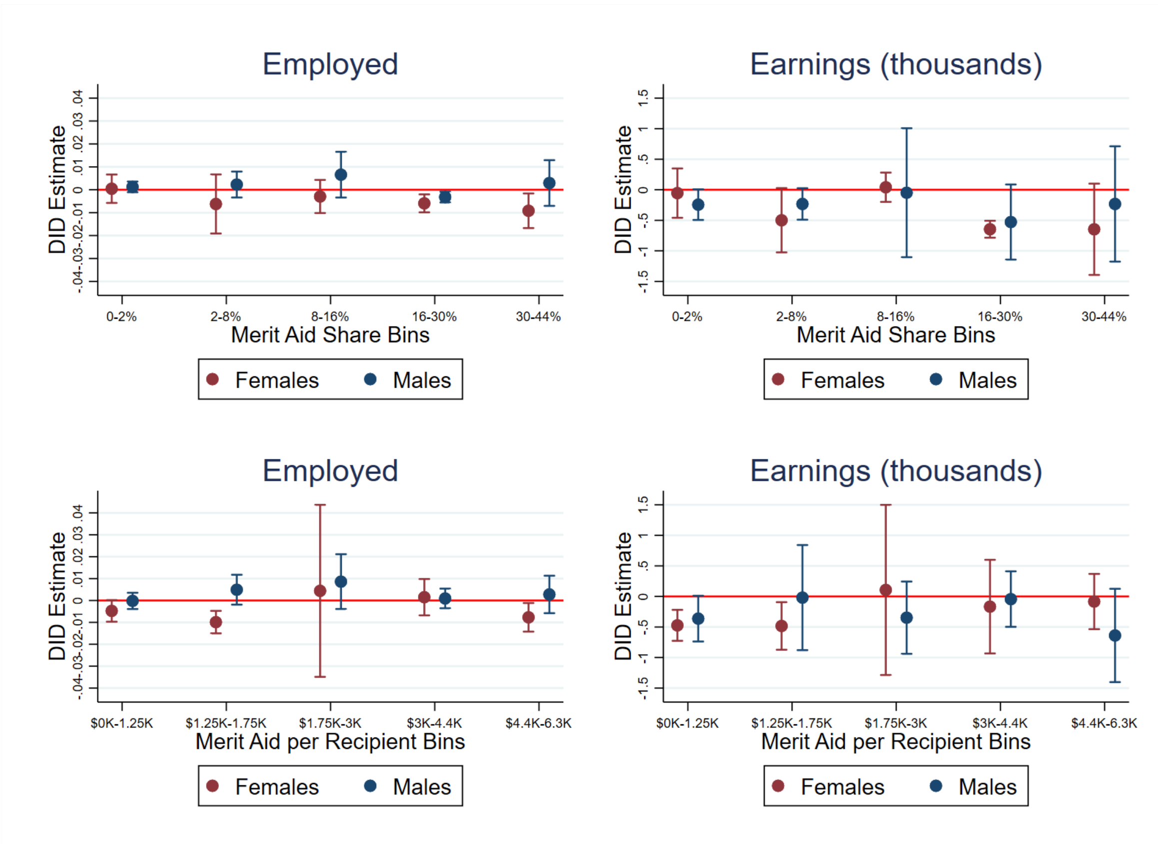



*Notes*: The figure reports event study estimates of the effects of strong merit aid program exposure on college enrollment. Regressions include lead and lag indicators for program rollout, along with state and year fixed effects. Outcomes are log enrollment of recent high school graduates in out-of-state and in-state colleges in the 2-year and 4-year sectors, measured at the state-by-year level using data from the IPEDS Residence and Migration Survey (NCES, 2023b). Treatment is defined by the rollout of strong merit aid programs in 10 adopting states. The comparison group consists of the 25 non-merit aid states and not-yet-treated cohorts in adopting states. Estimates are shown using both the two-way fixed effects (TWFE) estimator and the Sun and Abraham (2021) estimator. Event time is grouped into two-year bins to reflect the biennial structure of the IPEDS survey. Event time is centered at -1 for the two cohorts of students who enrolled in college in the two years before merit aid was offered, so negative values represent pre-adoption cohorts and zero and positive values represent post-adoption cohorts. Robust standard errors are clustered at the state level, and 95 percent confidence intervals are displayed.

Figure 3: Event Study Estimates of Merit Program Effects on Employment and Earnings

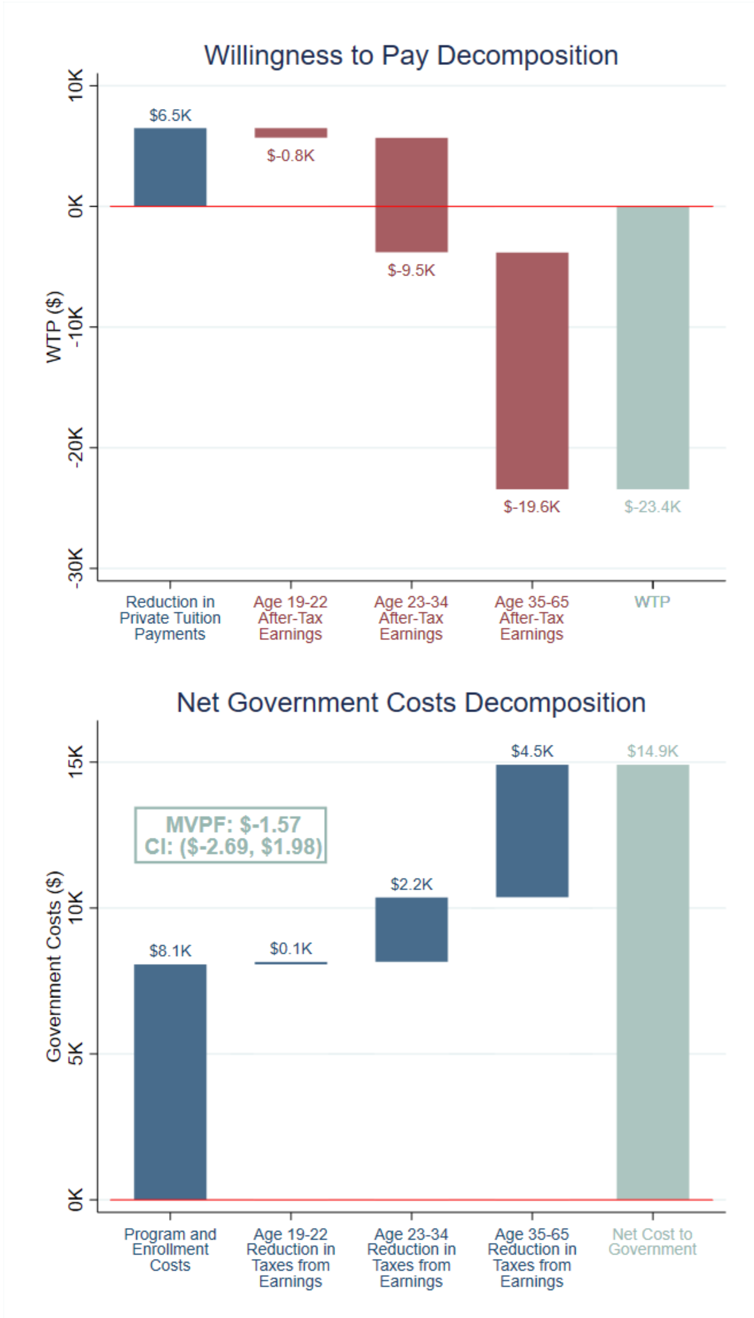

*Notes*: The figure reports event study estimates of the effects of strong merit program exposure on labor market outcomes. Regressions include lead and lag indicators for exposure to strong merit aid programs, along with birth state and birth cohort fixed effects. The analytic sample and labor market outcomes are described in Section 4. Earnings are measured in thousands of 2019 dollars. Treatment is defined by exposure to a strong merit aid program in an individual's birth state when the cohort reached age 18; the comparison group includes cohorts in the 25 non-merit aid states and cohorts in adopting states prior to program implementation. Estimates are shown using both the two-way fixed effects (TWFE) estimator and the Sun and Abraham (2021) estimator. Event time is centered at -1 for the cohort that reached age 18 in the year prior to program adoption, so negative values correspond to pre-adoption cohorts and zero and positive values to post-adoption cohorts. Regressions are weighted using the person weight variable. Robust standard errors are clustered at the birth state level, and 95 percent confidence intervals are displayed.

Figure 4: Effects on Labor Market Outcomes by Reach and Generosity



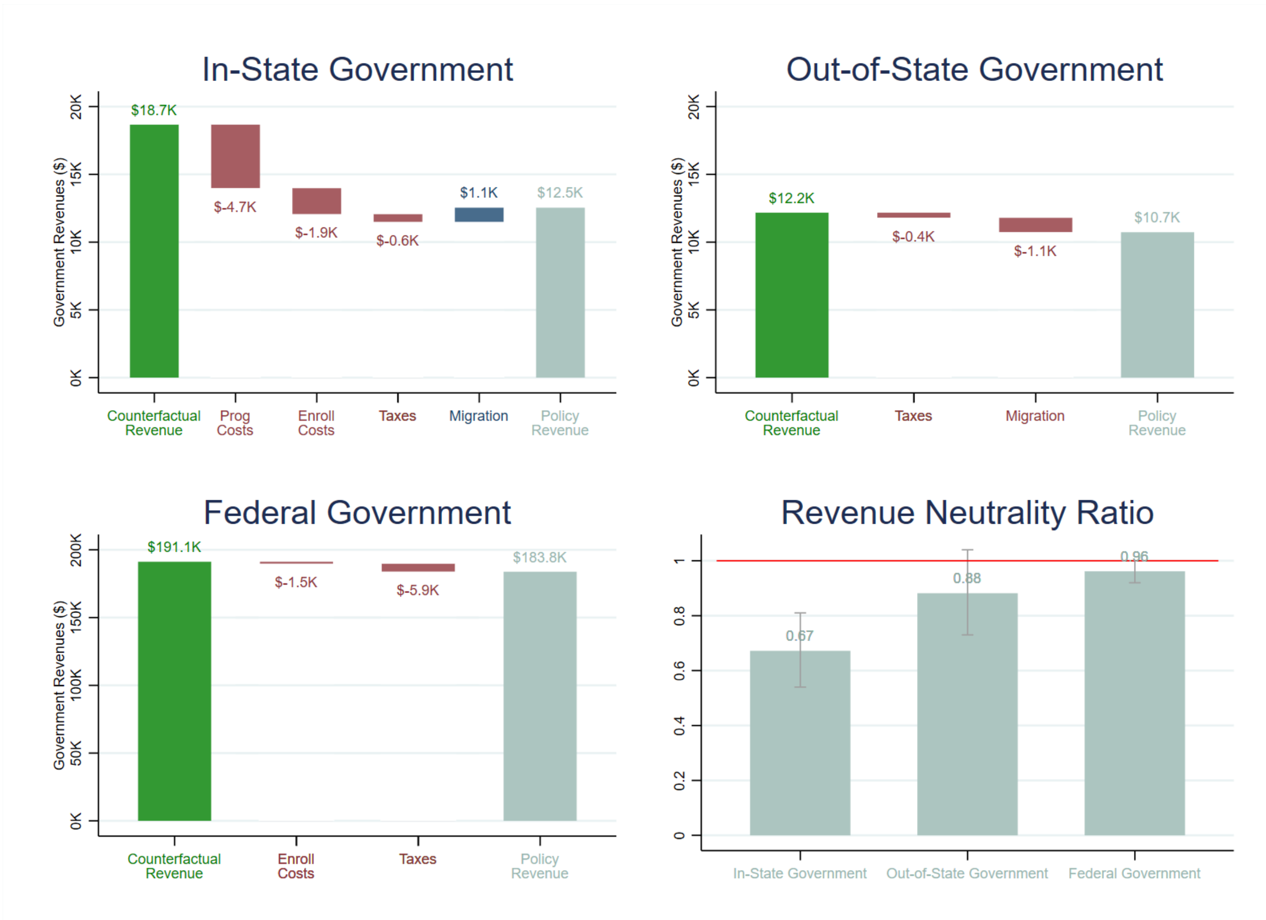

*Notes*: The figure reports ITT estimates of the impact of merit aid program exposure on employment and earnings, parameterized by program intensity. I use two measures of intensity: merit aid reach (the share of students receiving an award) and merit aid generosity (average aid per recipient). For each measure, adopting states are grouped into five bins containing equal numbers of treated states (five per bin). Treatment is defined as exposure to a merit aid program in an individual's birth state when the cohort reached age 18. The comparison group consists of the 25 non-merit states and pre-adoption cohorts in adopting states. Estimates are shown separately by gender. The analytic sample and outcome definitions are described in Section 4; earnings are measured in thousands of 2019 dollars. Estimates are obtained using my main TWFE specification with birth state and birth cohort fixed effects, demographic controls, state-by-year labor market controls, and birth state by birth cohort linear trends (as described in Section 5.1). Regressions are weighted using the person weight variable. Robust standard errors are clustered at the birth state level, and 95 percent confidence intervals are displayed.

Figure 5: Willingness to Pay and Government Cost Components for Strong Merit Programs




*Notes*: This figure decomposes the marginal value of public funds (MVPF) for strong state merit aid programs in the 10 adopting states into willingness to pay (WTP) and net government cost components. WTP is measured as the change in net tuition payments (inclusive of the merit aid award) plus the change in after-tax earnings. The net cost to the government includes direct program expenditures, changes in public higher education spending due to enrollment responses, and changes in tax revenue generated by earnings effects. The MVPF is defined as the ratio of WTP to the net cost to the government. Estimates are expressed in 2019 dollars and discounted at a 3 percent real annual rate. Bootstrapped 95 percent confidence intervals are reported for the MVPF.

Figure 6: Government Revenue Impacts and Revenue Neutrality of Strong Merit Programs




*Notes*: This figure decomposes the government revenue impacts of strong state merit aid programs across levels of government. Net revenue impacts are reported for the (i) in-state government of a state that implements a strong merit program, (ii) out-of-state governments in all other states that did not implement the program, and (iii) federal government. For each level of government, lifetime tax revenue under the policy is compared with the counterfactual revenue that would have been collected in the absence of strong merit aid adoption. Net revenue impacts incorporate direct program expenditures, changes in public higher education spending driven by enrollment responses, and changes in tax revenue resulting from earnings and migration effects. The bottom right subfigure reports the Revenue Neutrality Ratio (RNR), defined as the ratio of policy revenue to counterfactual revenue. The RNR measures the extent to which the revenue generated by the policy offsets its total net costs. An RNR of one indicates that policy-generated revenue fully covers these net costs. Estimates are expressed in 2019 dollars and discounted at a 3 percent real annual rate. Bootstrapped 95 percent confidence intervals are reported for the RNRs.

Table 1: Institutional Details of Strong Merit Aid Programs

| State | Program Name | Program Years | Aid/Recipient | Aid/FTE | Recipients/FTE | Main Program |
|---|---|---|---|---|---|---|
| **Arkansas** | **Arkansas Academic Challenge Scholarship** | **1991-** | **$2,771** | **$803** | **0.29** | **Yes** |
| Arkansas | Arkansas Governor's Distinguished Scholarship | 1983- | $9,755 | $181 | 0.02 | No |
| **Florida** | **Bright Futures Scholarship Program** | **1997-** | **$5,521** | **$835** | **0.15** | **Yes** |
| **Georgia** | **Georgia HOPE Scholarship** | **1993-** | **$4,278** | **$1,247** | **0.29** | **Yes** |
| Georgia | Zell Miller Scholarship | 2011- | $8,071 | $723 | 0.09 | No |
| **Kentucky** | **Kentucky Educational Excellence Scholarship** | **1999-** | **$1,662** | **$733** | **0.44** | **Yes** |
| **Louisiana** | **Louisiana TOPS Scholarship** | **1998-** | **$5,664** | **$1,755** | **0.31** | **Yes** |
| **Nevada** | **Nevada Millennium Scholarship** | **2000-** | **$1,589** | **$493** | **0.31** | **Yes** |
| **New Mexico** | **NM Lottery Success Scholarship** | **1997-** | **$1,849** | **$620** | **0.34** | **Yes** |
| New Mexico | Competitive Scholarship | 1997- | $1,292 | $30 | 0.02 | No |
| **South Carolina** | **LIFE Scholarship** | **1998-** | **$5,001** | **$1,250** | **0.25** | **Yes** |
| South Carolina | Palmetto Fellows Scholarship | 1988- | $8,048 | $426 | 0.05 | No |
| South Carolina | Lottery Tuition Assistance Program | 2002- | $1,388 | $315 | 0.23 | No |
| South Carolina | SC HOPE Scholarship Program | 2002- | $2,480 | $55 | 0.02 | No |
| **Tennessee** | **TN HOPE Scholarship** | **2003-** | **$3,602** | **$1,133** | **0.31** | **Yes** |
| Tennessee | General Assembly Merit Scholarship | 2003- | $898 | $33 | 0.04 | No |
| Tennessee | Ned McWherter Scholars Program | 1986- | $2,912 | $6 | 0.00 | No |
| **West Virginia** | **WV PROMISE Scholarship** | **2002-** | **$4,562** | **$551** | **0.12** | **Yes** |

*Sources*: NASSGAP (2020), Sjoquist and Winters (2015b), Sjoquist and Winters (2015a), Jia (2019), Baum et al. (2012), and Hawley and Rork (2022).

*Notes*: States and programs highlighted in blue represent the 10 strong merit aid programs. Statistics are based on the National Association of State Student Grant & Aid Programs (NASSGAP) Program Quick Finder and Annual Survey Report for 2019-2020, or the most recent prior year available (NASSGAP, 2020). The main program in each state is identified as the one with the highest merit aid per full-time equivalent (FTE) student, which accounts for both program generosity (aid per recipient) and program reach (share receiving an award). See Tables A1 and A2 for additional details on state merit aid program spending and eligibility.

Table 2: Merit Program Effects on Log Enrollment

| | (1) 2-Year | (2) 4-Year | (3) All |
|---|---|---|---|
| **A. Log Out-of-State Enrollment** | | | |
| Merit State | -0.1450 | -0.1966** | -0.1864** |
| | (0.1029) | (0.0773) | (0.0714) |
| N | 543 | 543 | 543 |
| **B. Log In-State Enrollment** | | | |
| Merit State | 0.2318* | 0.1507 | 0.1728* |
| | (0.1148) | (0.0998) | (0.0912) |
| N | 529 | 543 | 543 |
| **C. Log Total Enrollment** | | | |
| Merit State | 0.1939* | 0.0777 | 0.1097 |
| | (0.0990) | (0.0849) | (0.0758) |
| N | 543 | 543 | 543 |

*Notes*: This table reports estimates of the impact of strong merit aid programs on log college enrollment, using data from the Integrated Postsecondary Education Data System (IPEDS) Residence and Migration Survey NCES (2023b). IPEDS provides counts of recent high school graduates enrolling in each institution by their state of residence. Log enrollment is computed at the state-by-year level. Columns (1)-(3) report results for 2-year institutions, 4-year institutions, and all institutions, respectively. Panel A shows log out-of-state enrollment, Panel B shows log in-state enrollment, and Panel C shows log total enrollment. Estimates compare changes in log enrollment before and after program adoption between the 10 strong merit aid states and 25 non-merit aid states; the 15 weak merit aid states and Alaska are excluded. The specification is a two-way fixed effects (TWFE) model with state and year fixed effects. Robust standard errors are clustered at the state level.
*** p<0.01, ** p<0.05, * p<0.10

Table 3: Merit Program Effects on Educational Attainment

| | (1) College 1+ Years | (2) AA+ | (3) BA+ | (4) MA+ |
|---|---|---|---|---|
| **A. Females & Males** | | | | |
| Merit State | 0.0093*** | 0.0013 | -0.0014 | -0.0031 |
| | (0.0031) | (0.0037) | (0.0053) | (0.0024) |
| Unexposed Mean | .8733 | .5657 | .4298 | .1073 |
| Percent Change | {1.07%} | {.24%} | {-.33%} | {-2.88%} |
| TOT | [.0371] | [.0053] | [-.0057] | [-.0122] |
| N | 2,885,108 | 2,885,108 | 2,885,108 | 2,885,108 |
| **B. Females** | | | | |
| Merit State | 0.0058 | -0.0032 | -0.0006 | -0.0028 |
| | (0.0038) | (0.0039) | (0.0058) | (0.0028) |
| Unexposed Mean | .8784 | .5846 | .4395 | .1158 |
| Percent Change | {.66%} | {-.55%} | {-.14%} | {-2.38%} |
| TOT | [.0231] | [-.0128] | [-.0025] | [-.0109] |
| N | 1,597,090 | 1,597,090 | 1,597,090 | 1,597,090 |
| **C. Males** | | | | |
| Merit State | 0.0136*** | 0.0069 | -0.0028 | -0.0040 |
| | (0.0040) | (0.0051) | (0.0064) | (0.0024) |
| Unexposed Mean | .8668 | .5418 | .4174 | .0965 |
| Percent Change | {1.57%} | {1.28%} | {-.67%} | {-4.15%} |
| TOT | [.054] | [.0276] | [-.0111] | [-.0159] |
| N | 1,288,018 | 1,288,018 | 1,288,018 | 1,288,018 |

*Notes*: This table reports intention-to-treat (ITT) estimates of the impact of exposure to strong merit aid programs on educational attainment. The analytic sample is described in Section 4. Outcomes are: College 1+ Years (completion of one or more years of college), AA+ (completion of an associate degree or higher), BA+ (completion of a bachelor's degree or higher), and MA+ (completion of a master's degree or higher). Estimates compare changes in outcomes between exposed and unexposed cohorts in the 10 strong merit aid states and 25 non-merit aid states; the 15 weak merit aid states and Alaska are excluded. The main specification is a two-way fixed effects (TWFE) model with birth state and birth cohort fixed effects, controlling for: indicators for gender, race, gender-race interactions, and age; labor force participation and employment rates as well as the 25th, 50th, and 75th percentile of earnings measured at the state by survey year level; and birth state by birth cohort linear time trends. Statistics in braces report the coefficient divided by the unexposed mean, representing the percent change in the outcome for the 10 strong merit aid states. The implied treatment-on-the-treated (TOT) effect is computed by dividing the ITT coefficient by 0.252, the share of students in strong merit states receiving merit aid in 2019-2020 (NASSGAP, 2020). Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 4: Merit Program Effects on Employment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **A. Females & Males** | | | | |
| Merit State | -0.0069* | -0.0072* | -0.0048* | -0.0042** |
| | (0.0041) | (0.0040) | (0.0026) | (0.0019) |
| Unexposed Mean | 0.9080 | 0.9080 | 0.9080 | 0.9080 |
| Percent Change | {-0.76%} | {-0.80%} | {-0.53%} | {-0.47%} |
| TOT | [-0.0276] | [-0.0287] | [-0.0190] | [-0.0168] |
| N | 2,885,108 | 2,885,108 | 2,885,108 | 2,885,108 |
| **B. Females** | | | | |
| Merit State | -0.0110*** | -0.0110** | -0.0081*** | -0.0085*** |
| | (0.0040) | (0.0041) | (0.0029) | (0.0022) |
| Unexposed Mean | 0.8710 | 0.8710 | 0.8710 | 0.8710 |
| Percent Change | {-1.27%} | {-1.27%} | {-0.93%} | {-0.97%} |
| TOT | [-0.0437] | [-0.0438] | [-0.0322] | [-0.0337] |
| N | 1,597,090 | 1,597,090 | 1,597,090 | 1,597,090 |
| **C. Males** | | | | |
| Merit State | -0.0028 | -0.0032 | -0.0014 | 0.0013 |
| | (0.0047) | (0.0044) | (0.0031) | (0.0023) |
| Unexposed Mean | 0.9548 | 0.9548 | 0.9548 | 0.9548 |
| Percent Change | {-0.29%} | {-0.34%} | {-0.15%} | { 0.14%} |
| TOT | [-0.0111] | [-0.0127] | [-0.0056] | [0.0052] |
| N | 1,288,018 | 1,288,018 | 1,288,018 | 1,288,018 |
| p(Female = Male) | 0.0035 | 0.0046 | 0.0289 | 0.0000 |
| Controls Demo | | X | X | X |
| Controls Econ | | | X | X |
| Controls Trends | | | | X |

*Notes*: This table reports ITT estimates of the impact of strong merit aid program exposure on employment, defined as having positive earnings in the previous year. The analytic sample is described in Section 4. Estimates compare changes in outcomes between exposed and unexposed cohorts in the 10 strong merit aid states and 25 non-merit aid states; the 15 weak merit aid states and Alaska are excluded. Columns (1)-(4) report results from TWFE specifications with increasingly rich controls: column (1) includes birth state and birth cohort fixed effects; column (2) adds indicators for gender, race, gender-race interactions, and age; column (3) further adds labor force participation, employment rates, and the 25th, 50th, and 75th percentiles of earnings at the state-by-survey-year level; and column (4) adds linear birth state by birth cohort trends. Statistics in braces report the coefficient divided by the unexposed mean, representing the percent change in employment for the 10 strong merit aid states. The implied TOT effect is computed by dividing the ITT coefficient by 0.252, the share of students in strong merit states receiving merit aid in 2019-2020 (NASSGAP, 2020). P-values test the equality of the merit state coefficient for females and males. Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.

*** p<0.01, ** p<0.05, * p<0.10

Table 5: Merit Program Effects on Earnings

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **A. Females & Males** | | | | | |
| Merit State | -230 | -273 | -178 | -333 | -222 |
| | (318) | (316) | (137) | (205) | (194) |
| Unexposed Mean | 31,445 | 31,445 | 31,445 | 31,445 | 35,492 |
| Percent Change | {-0.73%} | {-0.87%} | {-0.56%} | {-1.06%} | {-0.63%} |
| TOT | [-914] | [-1,082] | [-705] | [-1,322] | [-881] |
| N | 2,583,974 | 2,583,974 | 2,583,974 | 2,583,974 | 2,347,960 |
| **B. Females** | | | | | |
| Merit State | -556* | -569* | -491*** | -501*** | -254 |
| | (286) | (290) | (174) | (181) | (251) |
| Unexposed Mean | 25,880 | 25,880 | 25,880 | 25,880 | 30,637 |
| Percent Change | {-2.15%} | {-2.20%} | {-1.90%} | {-1.93%} | {-0.83%} |
| TOT | [-2,208] | [-2,257] | [-1,946] | [-1,986] | [-1,006] |
| N | 1,432,714 | 1,432,714 | 1,432,714 | 1,432,714 | 1,255,442 |
| **C. Males** | | | | | |
| Merit State | -42 | -128 | -13 | -250 | -298 |
| | (423) | (389) | (163) | (362) | (336) |
| Unexposed Mean | 39,607 | 39,607 | 39,607 | 39,607 | 42,062 |
| Percent Change | {-0.11%} | {-0.32%} | {-0.03%} | {-0.63%} | {-0.71%} |
| TOT | [-167] | [-508] | [ -53] | [-992] | [-1,184] |
| N | 1,151,993 | 1,151,993 | 1,151,993 | 1,151,993 | 1,094,605 |
| p(Female = Male) | 0.0440 | 0.0720 | 0.0211 | 0.5256 | 0.9207 |
| Controls Demo | | X | X | X | X |
| Controls Econ | | | X | X | X |
| Controls Trends | | | | X | X |
| Earnings <90p | X | X | X | X | X |
| Employed | | | | | X |

*Notes*: This table reports ITT estimates of the impact of strong merit aid program exposure on earnings (below the 90th percentile) in 2019 dollars. The analytic sample is described in Section 4. Estimates compare changes in outcomes between exposed and unexposed cohorts in the 10 strong merit aid states and 25 non-merit aid states; the 15 weak merit aid states and Alaska are excluded. Columns (1)-(4) estimate impacts for both employed and non-employed individuals, and report TWFE specifications with increasingly rich controls, as described in Table 4 (birth state and cohort fixed effects, demographic indicators, labor market measures, and linear state-by-cohort trends). Column (5) estimates impacts for employed individuals only with all controls included. Statistics in braces report the coefficient divided by the unexposed mean, representing the percent change in earnings for the 10 strong merit aid states. The implied TOT effect is computed by dividing the ITT coefficient by 0.252, the share of students in strong merit states receiving merit aid in 2019-2020 (NASSGAP, 2020). Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 6: Parameterized Estimates of Merit Program Effects on Employment and Earnings

| | (1)<br>Employed | (2)<br>Employed | (3)<br>Earnings | (4)<br>Earnings |
|---|---|---|---|---|
| **A. Females & Males** | | | | |
| Aid Share, + 10 p.p. | -0.0015** | -0.0017** | -127 | -190*** |
| | (0.0008) | (0.0007) | (75) | (67) |
| Aid per Recipient, + $1,000 | -0.0005 | -0.0001 | -49 | -65 |
| | (0.0004) | (0.0007) | (44) | (42) |
| Unexposed Mean | 0.9080 | 0.9081 | 31,445 | 33,215 |
| N | 2,885,108 | 4,348,091 | 2,583,974 | 3,897,718 |
| **B. Females** | | | | |
| Aid Share, + 10 p.p. | -0.0030*** | -0.0029*** | -194*** | -225*** |
| | (0.0008) | (0.0007) | (56) | (62) |
| Aid per Recipient, + $1,000 | -0.0014*** | -0.0006 | -80* | -51 |
| | (0.0004) | (0.0010) | (45) | (63) |
| Unexposed Mean | 0.8710 | 0.8734 | 25,880 | 27,273 |
| N | 1,597,090 | 2,397,346 | 1,432,714 | 2,152,199 |
| **C. Males** | | | | |
| Aid Share, + 10 p.p. | 0.0004 | -0.0001 | -77 | -148 |
| | (0.0009) | (0.0009) | (131) | (112) |
| Aid per Recipient, + $1,000 | 0.0006 | 0.0006 | -40 | -66 |
| | (0.0006) | (0.0004) | (79) | (49) |
| Unexposed Mean | 0.9548 | 0.9487 | 39,607 | 41,104 |
| N | 1,288,018 | 1,950,745 | 1,151,993 | 1,744,335 |
| Treatment States | 10 | 25 | 10 | 25 |
| Control States | 25 | 25 | 25 | 25 |
| Excluded States | 16 | 1 | 16 | 1 |
| Total States | 35 | 50 | 35 | 50 |
| Earnings <90p | | | X | X |

*Notes*: This table reports parameterized estimates of the impact of merit aid program exposure on employment and earnings, where exposure is measured by program intensity. The key explanatory variables are the share of students receiving a merit award and merit aid per recipient. A one-unit increase in the aid share variable corresponds to a 10 p.p. increase in the share receiving aid, while a one-unit increase in merit aid per recipient variable corresponds to a $1,000 increase in aid. Earnings are measured as earnings below the 90th percentile in 2019 dollars. The analytic sample is described in Section 4. Columns (1) and (3) use the baseline sample including the 10 strong merit aid states and 25 non-merit aid states while excluding the 15 weak merit aid states and Alaska. Columns (2) and (4) additionally include the weak merit aid states among the treated states. Estimates are based on the main TWFE specification described in Section 5.1 and Table 3. Each cell reports results from a separate regression. Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** p<0.01, ** p<0.05, * p<0.10

Table 7: Effects on Employment Outcomes and State Residence

| | (1) Employed (Earnings > 0) | (2) Employed (Survey) | (3) Weeks Worked | (4) Hours per Week | (5) Weeks Worked | (6) Hours per Week | (7) Live in Birth State |
|---|---|---|---|---|---|---|---|
| **A. Females & Males** | | | | | | | |
| Merit State | -0.0042** | -0.0046* | -0.3555*** | -0.3507** | -0.1689 | -0.2064* | 0.0090 |
| | (0.0019) | (0.0023) | (0.1261) | (0.1425) | (0.1136) | (0.1026) | (0.0077) |
| Unexposed Mean | 0.9080 | 0.9511 | 41.6092 | 37.0529 | 45.7924 | 40.7774 | 0.6053 |
| Percent Change | {-0.47%} | {-0.48%} | {-0.85%} | {-0.95%} | {-0.37%} | {-0.51%} | { 1.48%} |
| TOT | [-0.0168] | [-0.0182] | [-1.4106] | [-1.3918] | [-0.6703] | [-0.8192] | [0.0355] |
| N | 2,885,108 | 2,541,281 | 2,885,108 | 2,885,108 | 2,621,957 | 2,621,957 | 2,885,108 |
| **B. Females** | | | | | | | |
| Merit State | -0.0085*** | -0.0000 | -0.4026*** | -0.5657*** | 0.0016 | -0.2556 | 0.0080 |
| | (0.0022) | (0.0024) | (0.1455) | (0.1550) | (0.1458) | (0.1588) | (0.0083) |
| Unexposed Mean | 0.8710 | 0.9456 | 38.8953 | 33.3599 | 44.6131 | 38.2633 | 0.6201 |
| Percent Change | {-0.97%} | {-0.00%} | {-1.04%} | {-1.70%} | { 0.00%} | {-0.67%} | { 1.29%} |
| TOT | [-0.0337] | [-0.0000] | [-1.5977] | [-2.2447] | [0.0062] | [-1.0141] | [0.0318] |
| N | 1,597,090 | 1,338,494 | 1,597,090 | 1,597,090 | 1,398,839 | 1,398,839 | 1,597,090 |
| **C. Males** | | | | | | | |
| Merit State | 0.0013 | -0.0098*** | -0.3027* | -0.0832 | -0.3865** | -0.1634 | 0.0099 |
| | (0.0023) | (0.0029) | (0.1720) | (0.2354) | (0.1764) | (0.1641) | (0.0075) |
| Unexposed Mean | 0.9548 | 0.9573 | 45.0507 | 41.7361 | 47.1566 | 43.6859 | 0.5866 |
| Percent Change | { 0.14%} | {-1.02%} | {-0.67%} | {-0.20%} | {-0.82%} | {-0.37%} | { 1.70%} |
| TOT | [0.0052] | [-0.0388] | [-1.2012] | [-0.3300] | [-1.5337] | [-0.6484] | [0.0395] |
| N | 1,288,018 | 1,202,787 | 1,288,018 | 1,288,018 | 1,223,118 | 1,223,118 | 1,288,018 |
| Employed (Earnings > 0) | | | | | X | X | |

*Notes*: This table reports ITT estimates of the impact of strong merit aid program exposure on a range of extensive- and intensive-margin employment outcomes, as well as the likelihood of residing in one's birth state. The outcomes are: employed based on positive earnings (1), employed at the time of the survey (2), weeks worked (3), hours worked per week (4), weeks worked and hours worked per week among individuals with positive earnings (5)-(6), and residing in birth state (7). Employment at the time of the survey is set to missing for those who are out of the labor force. Columns (1)-(4) and (7) include both employed and non-employed individuals, while columns (5)-(6) are restricted to individuals with positive earnings. The analytic sample is described in Section 4. Estimates compare changes in outcomes between exposed and unexposed cohorts in the 10 strong merit aid states and 25 non-merit aid states; the 15 weak merit aid states and Alaska are excluded. Estimates are based on the main TWFE specification described in Section 5.1 and Table 3. Statistics in braces report the coefficient divided by the unexposed mean, representing the percent change in the outcome for the 10 strong merit aid states. The implied TOT effect is computed by dividing the ITT coefficient by 0.252, the share of students in strong merit states receiving merit aid in 2019-2020 (NASSGAP, 2020). Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** p<0.01, ** p<0.05, * p<0.10

Table 8: Heterogeneous Effects by Race

| | (1) College 1+ Years | (2) AA+ | (3) BA+ | (4) MA+ | (5) Any Children | (6) Employed | (7) Earnings |
|---|---|---|---|---|---|---|---|
| **A. White Females** | | | | | | | |
| Merit State | 0.0031 | -0.0086* | -0.0061 | -0.0061** | 0.0094** | -0.0076*** | -350* |
| | (0.0039) | (0.0044) | (0.0053) | (0.0029) | (0.0037) | (0.0021) | (173) |
| Unexposed Mean | 0.8867 | 0.6202 | 0.4740 | 0.1251 | 0.5448 | 0.8612 | 26,086 |
| Percent Change | {0.35%} | { -1.39%} | { -1.28%} | { -4.90%} | {1.72%} | { -0.89%} | { -1.34%} |
| TOT | [0.0123] | [-0.0342] | [-0.0241] | [-0.0243] | [0.0372] | [-0.0302] | [-1,391] |
| N | 1,214,791 | 1,214,791 | 1,214,791 | 1,214,791 | 1,214,791 | 1,214,791 | 1,092,033 |
| **B. Nonwhite Females** | | | | | | | |
| Merit State | 0.0118** | 0.0082 | 0.0121 | 0.0056 | -0.0009 | -0.0083* | -617* |
| | (0.0057) | (0.0094) | (0.0119) | (0.0072) | (0.0083) | (0.0045) | (308) |
| Unexposed Mean | 0.8560 | 0.4882 | 0.3462 | 0.0904 | 0.6155 | 0.8977 | 25,291 |
| Percent Change | {1.38%} | {1.68%} | {3.49%} | {6.16%} | { -0.15%} | { -0.92%} | { -2.44%} |
| TOT | [0.0467] | [0.0326] | [0.0479] | [0.0221] | [-0.0037] | [-0.0329] | [-2,449] |
| N | 382,299 | 382,299 | 382,299 | 382,299 | 382,299 | 382,299 | 342,014 |
| **C. White Males** | | | | | | | |
| Merit State | 0.0134*** | 0.0092* | -0.0030 | -0.0056* | 0.0044 | 0.0006 | -377 |
| | (0.0037) | (0.0048) | (0.0078) | (0.0029) | (0.0069) | (0.0032) | (362) |
| Unexposed Mean | 0.8709 | 0.5672 | 0.4450 | 0.1040 | 0.3914 | 0.9605 | 41,596 |
| Percent Change | {1.54%} | {1.63%} | { -0.67%} | { -5.36%} | {1.11%} | {0.06%} | { -0.91%} |
| TOT | [0.0533] | [0.0366] | [-0.0118] | [-0.0221] | [0.0173] | [0.0023] | [-1,495] |
| N | 1,013,389 | 1,013,389 | 1,013,389 | 1,013,389 | 1,013,389 | 1,013,389 | 907,870 |
| **D. Nonwhite Males** | | | | | | | |
| Merit State | 0.0134 | 0.0008 | -0.0016 | 0.0000 | 0.0236*** | 0.0038 | -101 |
| | (0.0103) | (0.0147) | (0.0107) | (0.0065) | (0.0084) | (0.0048) | (581) |
| Unexposed Mean | 0.8532 | 0.4570 | 0.3250 | 0.0715 | 0.3852 | 0.9359 | 33,808 |
| Percent Change | {1.57%} | {0.19%} | { -0.51%} | {0.06%} | {6.12%} | {0.41%} | { -0.30%} |
| TOT | [0.0530] | [0.0034] | [-0.0065] | [0.0002] | [0.0936] | [0.0151] | [ -401] |
| N | 274,629 | 274,629 | 274,629 | 274,629 | 274,629 | 274,629 | 245,341 |
| Earnings <90p | | | | | | | X |

*Notes*: This table reports ITT estimates of the impact of strong merit aid program exposure on outcomes by race-gender subgroups. The analytic sample is described in Section 4. Estimates are based on the main TWFE specification described in Section 5.1 and Table 3. Statistics in braces report the coefficient divided by the unexposed mean, representing the percent change in the outcome for the 10 strong merit aid states. The implied TOT effect is computed by dividing the ITT coefficient by 0.252. Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** p<0.01, ** p<0.05, * p<0.10

# Appendix A State Merit Aid Programs Background Additional Details

This appendix provides detailed institutional information on state merit aid programs that underlie the classification of "strong", "weak", and "non-merit" states used throughout the paper. Because the empirical analysis exploits cross-state variation in program intensity and design, it is important to document how programs differ in generosity, reach, eligibility requirements, and award structure. The sections below summarize: (i) program features, (ii) define and illustrate the primary measure of program intensity, (iii) describe academic and residency eligibility criteria, and (iv) detail differences in award use, application processes, and tiered award structures.

**Program Features.** Table 1 summarizes key features of the programs in strong merit states. Although some strong merit states have a single primary merit aid program, others operate multiple programs that may have been introduced at different times. The majority of programs were introduced in the 1990s; the earliest award was established in 1983 and the most recent in 2011. The table reports: (i) aid per recipient, a measure of average award size; (ii) aid per full-time equivalent (FTE) student, a measure of average aid per FTE student enrolled in institutions within the state; (iii) the share of recipients relative to total state FTE enrollment; and (iv) an indicator for the state's main program, shown in blue text.[57]

**Program Intensity Measures.** The primary measure of merit aid program intensity is aid per FTE student, which captures both award generosity (aid per recipient) and program reach (the share of total FTE enrollment receiving an award). By construction, all strong merit states have at least one program with aid per FTE student of $400 or more, while all programs in weak merit states have aid per FTE student below $300. The main program in each state is defined as the program with the highest aid per FTE student. Louisiana's TOPS

[57]Statistics are computed using data from the National Association of State Student Grant Aid Programs (NASSGAP) Program Quick Finder and Annual Survey Report for 2019-2020 or the most recent prior academic year available (NASSGAP, 2020).

Scholarship has the largest main program, with $1,755 per FTE student in 2019-2020, while Nevada's Millennium Scholarship is the smallest at $493 per FTE student. These programs also represent the most and least generous main awards per recipient ($5,664 and $1,589, respectively).[58] The average award among all main programs in strong merit states is $3,985. The scholarships are also widely available: for eight of the ten main programs, more than 20 percent of the state's FTE enrollment received an award in 2019-2020. The Kentucky Educational Excellence Scholarship (KEES) is the most widely available, with 44 percent of students in Kentucky receiving the scholarship.

**Eligibility.** Programs in strong merit states share several common features. Students typically must reside in and attend high school in the state for at least one year. Initial eligibility is determined by academic performance, typically using some combination of high school GPA, SAT or ACT scores, or state standardized test scores.[59] A defining feature common to all strong merit programs is that awards must be used at in-state postsecondary institutions. This design is intentional and reflects state efforts to retain high-achieving students in-state (OSFA, 2023).

**Award Use, Application Process, and Award Structure.** There are similarities and differences across states in institutional eligibility, application requirements, and award structure. Students may use awards at four-year universities and state colleges, two-year colleges, and technical or vocational institutions. Most states allow awards to be used at both public and private institutions, although Nevada and New Mexico restrict use to public institutions.[60] All but four strong merit states require students to submit an online application, sometimes including the Free Application for Federal Student Aid (FAFSA). In Kentucky, Nevada, New Mexico, and South Carolina, eligible students are identified through coordination between high schools and state education agencies and are notified directly.

[58] Arkansas, Georgia, and South Carolina have non-main programs that are more generous than Louisiana TOPS, but these are targeted to particularly high-achieving students and are not widely available.

[59] Initial academic eligibility requirements for all state merit aid programs are shown in Table A2.

[60] Nevada allows a limited exception for the private Roseman University of Health Sciences, which has relatively small enrollment. Although most states allow use at private institutions, only Florida, Georgia, Kentucky, and West Virginia allow awards at private for-profit institutions.

Most states use tiered eligibility and award structures, under which lower-achieving eligible students receive smaller awards, often usable at two-year colleges, while higher-achieving students receive larger awards, typically usable at four-year institutions (Jia, 2019; Sjoquist and Winters, 2016, 2015a,b).

Table A1: Institutional Details of Merit Aid Spending for All Programs

| State | Program Name | Program Years | Aid/Recipient | Aid/FTE | Recipients/FTE | Main Program |
|---|---|---|---|---|---|---|
| **Arkansas** | **Arkansas Academic Challenge Scholarship** | **1991-** | **$2,771** | **$803** | **0.29** | **Yes** |
| Arkansas | Arkansas Governor's Distinguished Scholarship | 1983- | $9,755 | $181 | 0.02 | No |
| Delaware | Delaware SEED Program | 2005- | $1,886 | $148 | 0.08 | Yes |
| Delaware | DSU Inspire Scholarship | 2010- | $3,829 | $44 | 0.01 | No |
| Delaware | Diamond State Scholarship | 2001- | $1,244 | $7 | 0.01 | No |
| **Florida** | **Bright Futures Scholarship Program** | **1997-** | **$5,521** | **$835** | **0.15** | **Yes** |
| **Georgia** | **Georgia HOPE Scholarship** | **1993-** | **$4,278** | **$1,247** | **0.29** | **Yes** |
| Georgia | Zell Miller Scholarship | 2011- | $8,071 | $723 | 0.09 | No |
| Idaho | Promise Scholarship Category B | 2001-2013 | $500 | $42 | 0.08 | Yes |
| Idaho | Promise Scholarship Category A | 2001-2017 | $3,016 | $4 | 0.00 | No |
| Illinois | Illinois Merit Recognition Scholarship | 1999-2004 | $984 | $11 | 0.01 | Yes |
| **Kentucky** | **Kentucky Educational Excellence Scholarship** | **1999-** | **$1,662** | **$733** | **0.44** | **Yes** |
| **Louisiana** | **Louisiana TOPS Scholarship** | **1998-** | **$5,664** | **$1,755** | **0.31** | **Yes** |
| Massachusetts | MA Adams Scholarship | 2005- | $1,310 | $52 | 0.04 | Yes |
| Michigan | Michigan Merit & Promise Scholarship | 2000-2008 | $1,205 | $240 | 0.20 | Yes |
| Mississippi | Mississippi TAG and ESG | 1996- | $807 | $143 | 0.18 | Yes |
| Missouri | A Plus Scholarship | 1994- | $3,177 | $191 | 0.06 | Yes |
| Missouri | Missouri Bright Flight Scholarship | 1987- | $2,848 | $98 | 0.03 | No |
| Montana | Governor's Best and Brightest | 2006-2016 | $1,987 | $33 | 0.02 | Yes |
| **Nevada** | **Nevada Millennium Scholarship** | **2000-** | **$1,589** | **$493** | **0.31** | **Yes** |

## Table A1 (Continued)

| State | Program Name | Program Years | Aid/Recipient | Aid/FTE | Recipients/FTE | Main Program |
|---|---|---|---|---|---|---|
| New Jersey | New Jersey OSRP | 1998-2006 | $3,824 | $79 | 0.02 | Yes |
| New Jersey | NJ STARS | 2006- | $2,805 | $24 | 0.01 | No |
| New Jersey | Bloustein Distinguished Scholars | 1986-2010 | $908 | $16 | 0.02 | No |
| **New Mexico** | **NM Lottery Success Scholarship** | **1997-** | **$1,849** | **$620** | **0.34** | **Yes** |
| New Mexico | Competitive Scholarship | 1997- | $1,292 | $30 | 0.02 | No |
| New York | NY STEM Incentive Program | 2014- | $6,279 | $16 | 0.00 | Yes |
| New York | NY Scholarships for Academic Excellence | 1997- | $686 | $10 | 0.02 | No |
| New York | NY AIMS | 2015- | $434 | $0 | 0.00 | No |
| North Dakota | ND Academic/CTE Scholarships | 2010- | $1,436 | $200 | 0.14 | Yes |
| North Dakota | ND Scholars Program | 1994- | $8,039 | $23 | 0.00 | No |
| Oklahoma | Academic Scholars Program | 1988- | $4,028 | $41 | 0.01 | Yes |
| Oklahoma | Regional University Baccalaureate Scholarship | 1994- | $2,977 | $5 | 0.00 | No |
| **South Carolina** | **LIFE Scholarship** | **1998-** | **$5,001** | **$1,250** | **0.25** | **Yes** |
| South Carolina | Palmetto Fellows Scholarship | 1988- | $8,048 | $426 | 0.05 | No |
| South Carolina | Lottery Tuition Assistance Program | 2002- | $1,388 | $315 | 0.23 | No |
| South Carolina | SC HOPE Scholarship Program | 2002- | $2,480 | $55 | 0.02 | No |
| South Dakota | SD Regents Opportunity Scholarship Program | 2003- | $1,417 | $173 | 0.12 | Yes |
| **Tennessee** | **TN HOPE Scholarship** | **2003-** | **$3,602** | **$1,133** | **0.31** | **Yes** |
| Tennessee | General Assembly Merit Scholarship | 2003- | $898 | $33 | 0.04 | No |
| Tennessee | Ned McWherter Scholars Program | 1986- | $2,912 | $6 | 0.00 | No |
| Utah | Regents' Scholarship | 2008- | $2,404 | $54 | 0.02 | Yes |
| Utah | New Century Scholarship | 1999- | $2,171 | $5 | 0.00 | No |
| Washington | Washington Promise Scholarship | 1999-2006 | $1,096 | $35 | 0.03 | Yes |
| **West Virginia** | **WV PROMISE Scholarship** | **2002-** | **$4,562** | **$551** | **0.12** | **Yes** |

*Sources*: NASSGAP (2020), Sjoquist and Winters (2015b), Sjoquist and Winters (2015a), Jia (2019), Baum et al. (2012), and Hawley and Rork (2022).
*Notes*: States and programs in blue comprise the 10 strong merit aid programs. Statistics are computed using data from the National Association of State Student Grant & Aid Programs (NASSGAP) Program Quick Finder and Annual Survey Report for 2019-2020, or the most recent prior academic year for which information is available (NASSGAP, 2020). The main program in each state is identified as the one with the highest merit aid per FTE student, which accounts for both program generosity (aid per recipient) and program reach (share receiving an award).

Table A2: Institutional Details of Merit Aid Eligibility for All Programs

| State | Program Name | Initial Requirement | Renewal Requirement |
|---|---|---|---|
| **Arkansas** | **Arkansas Academic Challenge Scholarship** | **GPA: 2.5, ACT: 19** | **GPA: 2.5** |
| Arkansas | Arkansas Governor's Distinguished Scholarship | GPA: 3.5, ACT: 32, SAT: 1410 | GPA: 3.0-3.25 |
| Delaware | Delaware SEED Program | GPA: 2.5 | GPA : 2.5 |
| Delaware | DSU Inspire Scholarship | GPA: 2.75 | GPA : 2.75 |
| Delaware | Diamond State Scholarship | GPA: 3.0, SAT: 1290 | GPA: 3.0 |
| **Florida** | **Bright Futures Scholarship Program** | **GPA: 3.0-3.5, ACT: 17-29, SAT: 880-1330** | **GPA: 2.75-3.0** |
| **Georgia** | **Georgia HOPE Scholarship** | **GPA: 3.0** | **GPA: 3.0** |
| Idaho | Promise Scholarship Category B | N/A | N/A |
| Idaho | Promise Scholarship Category A | N/A | N/A |
| Illinois | Illinois Merit Recognition Scholarship | N/A | N/A |
| **Kentucky** | **Kentucky Educational Excellence Scholarship** | **GPA: 2.5, ACT: 15** | **GPA: 2.5-3.0** |
| **Louisiana** | **Louisiana TOPS Scholarship** | **GPA: 2.5-3.0, ACT: 17-27** | **GPA: 2.0-3.0** |
| Massachusetts | MA Adams Scholarship | State Test: District 75th Percentile | GPA: 3.0 |
| Michigan | Michigan Merit & Promise Scholarship | State Test: Level 2, ACT: 24 | GPA: 2.5 |
| Mississippi | Mississippi TAG and ESG | GPA: 2.5-3.5, ACT: 14-29 | GPA: 2.5-3.5 |
| Missouri | A Plus Scholarship | GPA: 2.5 | GPA: 2.0-2.5 |
| Missouri | Missouri Bright Flight Scholarship | GPA: 2.5, ACT/SAT: 97th Percentile | GPA: 2.5 |
| Montana | Governor's Best and Brightest | GPA: 3.0, ACT: 20 | N/A |
| **Nevada** | **Nevada Millennium Scholarship** | **GPA: 3.25, ACT: 21, SAT: 1070** | **GPA: 2.5-2.75** |

Table A2 (Continued)

| State | Program Name | Initial Requirement | Renewal Requirement |
|---|---|---|---|
| New Jersey | New Jersey OSRP | Class Rank: Top 15%, SAT: 1250 | GPA: 3.0 |
| New Jersey | NJ STARS | Class Rank: Top 15% | GPA: 3.25 |
| New Jersey | Bloustein Distinguished Scholars | Class Rank: Top 10%, SAT: 1260 | N/A |
| **New Mexico** | **NM Lottery Success Scholarship** | **N/A** | **GPA: 2.5** |
| New Mexico | Competitive Scholarship | GPA: 3.0-3.5, ACT: 20-23 | GPA: 3.0 |
| New York | NY STEM Incentive Program | Class Rank: Top 10% | GPA: 2.5 |
| New York | NY Scholarships for Academic Excellence | Regents Exam | N/A |
| New York | NY AIMS | GPA: 3.3, Class Rank: Top 15% | N/A |
| North Dakota | ND Academic/CTE Scholarships | GPA: 3.0, ACT: 24 | GPA: 2.75 |
| North Dakota | ND Scholars Program | ACT/SAT: 95th percentile | N/A |
| Oklahoma | Academic Scholars Program | ACT/SAT: 99.5th percentile | GPA: 3.25 |
| Oklahoma | Regional University Baccalaureate Scholarship | ACT: 30 | N/A |
| **South Carolina** | **LIFE Scholarship** | **3.0/30%/24/1100** | **GPA: 3.0** |
| South Carolina | Palmetto Fellows Scholarship | 3.5-4.0/6%/27-32/1200-1400 | GPA: 3.0 |
| South Carolina | Lottery Tuition Assistance Program | N/A | GPA: 2.0 |
| South Carolina | SC HOPE Scholarship Program | GPA: 3.0 | N/A |
| South Dakota | SD Regents Opportunity Scholarship Program | GPA: 3.0, ACT: 24-28, SAT: 1090-1310 | GPA: 3.0 |
| **Tennessee** | **TN HOPE Scholarship** | **GPA: 3.0, ACT: 21, SAT: 1060** | **GPA: 2.75-3.0** |
| Tennessee | General Assembly Merit Scholarship | GPA: 3.75, ACT: 29, SAT: 1330 | N/A |
| Tennessee | Ned McWherter Scholars Program | GPA: 3.5, ACT: 29 | GPA: 3.0 |
| Utah | Regents' Scholarship | HS GPA: 3.3, ACT: 22 | GPA: 3.0 |
| Utah | New Century Scholarship | HS GPA: 3.0, AA GPA: 3.0 | GPA: 3.3 |
| Washington | Washington Promise Scholarship | Class Rank: Top 15%, ACT: 27, SAT: 1200 | N/A |
| **West Virginia** | **WV PROMISE Scholarship** | **GPA: 3.0, ACT: 21, SAT: 1080** | **GPA: 2.75-3.0** |

*Sources*: NASSGAP (2020), Sjoquist and Winters (2015b), Sjoquist and Winters (2015a), Jia (2019), Baum et al. (2012), and Hawley and Rork (2022).
*Notes*: States and programs in blue comprise the 10 strong merit aid programs. The main program in each state is identified as the one with the highest merit aid per FTE student, which accounts for both program generosity (aid per recipient) and program reach (share receiving an award).

# Appendix B Welfare Analysis Methodology

## Appendix B.1 Marginal Value of Public Funds Methodology

This appendix section describes the MVPF methodology. Data for the estimation of the MVPF are drawn from a wide variety of sources. All monetary values are converted to 2019 dollars. For students' private and government educational costs, data on net tuition and total educational costs are from the IPEDS Delta Cost Project (NCES, 2015) and merit aid awards are from NASSGAP (2020). I also use data from my estimated effects on college enrollment shown in Table 2, enrollment and graduation rates from IPEDS (NCES, 2023b), and average tuition rates over time from the Digest of Education Statistics (NCES, 2023a).

Using data on annual net tuition, total cost, and merit award value, I compute cumulative costs over the course of college by summing annual costs and discounting back to age 18 at a 3 percent discount rate.[61] I make two key assumptions for these computations. First, I assume that the difference between total direct educational spending and net student tuition is borne by the government. Second, I assume that the only enrollment shift induced by merit aid is from 4-year out-of-state to 4-year in-state institutions. I use my estimated effect on 4-year out-of-state enrollment in Table 2, combined with the baseline 4-year out-of-state enrollment rate, to compute the share of affected students. The share of unaffected students is the difference between the share of students receiving merit aid and the share of affected students. Changes in students' private and government educational costs are then computed for both affected and unaffected students.

For earnings and tax impacts, I use data from my estimated TOT effects on earnings shown in Tables C2 and 5, earnings from the 1990 Census (Ruggles et al., 2022), tax rates from the Congressional Budget Office (CBO, 2016), and poverty thresholds from the Census (US Census Bureau, 2024). To compute ages 19-22 and ages 23-34 earnings impacts, I use

[61] I assume 4-year enrollees who graduate attend college for four years and 4-year enrollees who do not graduate attend college for two years while 2-year enrollees who graduate attend college for two years and 2-year enrollees who do not graduate attend college for one year.

TOT estimates for the full sample from Tables C2 and 5. I apply the same TOT impact for every age in each respective range, discount back to age 18 using a 3 percent discount rate, and sum the discounted impact for each age to get cumulative discounted earnings impacts for these age ranges. For ages 35-65 earnings impacts, I assume that the same percent earnings impact in the ages 23-34 range will occur over the course of the entire lifecycle from ages 35 to 65 and apply this to counterfactual earnings.

To compute counterfactual earnings at different ages, I use the distribution of average earnings from ages 19 to 65 in strong merit states from 1990 Census data.[62] To compute the ages 23-34 percent earnings impact, I use the ages 23-34 earnings TOT effect and divide this by counterfactual age 29 earnings. This same percent earnings impact is applied to counterfactual earnings from ages 35 to 65. I sum the discounted earnings impact for each age to get the cumulative discounted earnings impact during this age range. To compute tax rates, I link counterfactual earnings to data on effective marginal tax rates for each 50 percent earnings bin of the federal poverty level (FPL) from CBO (2016).[63] I use counterfactual age 21 earnings to determine the ages 19-22 marginal tax rate, counterfactual age 29 earnings to determine the ages 23-34 marginal tax rate, and counterfactual age 40 earnings to determine the ages 35-65 marginal tax rate. Finally, cumulative discounted earnings impacts and tax rates for the three respective age ranges are combined to compute cumulative discounted earnings impacts net of taxes and changes in tax revenue.

[62] I adjust earnings from the 1990 Census by converting to 2019 dollars and I assume 0.5 percent wage growth per year.

[63] The CBO uses a tax-and-transfer rate that includes the combined effects of federal and average state individual income taxes, average federal payroll taxes, Supplemental Nutrition Assistance Program (SNAP) benefits, and cost-sharing subsidies for health insurance. I use the median marginal tax rate within each bin. For the FPL, I assume that each household has one parent and one child because Table C4 shows that about half of individuals in my analytic sample are unmarried and there is less than one own child in the household on average. CBO estimates the contribution of average state income taxes to the total marginal tax rate to be 2.6 p.p. State income taxes were calculated using state tax laws in place in 2013. In my analysis, I exclude the payroll tax component, which CBO estimates to be 13.9 p.p. on average, from my marginal tax rates because at least some portion of these benefits are returned to individuals. I assume that the contribution of payroll taxes to the marginal tax rate is constant across the earnings distribution.

## Appendix B.2 Revenue Neutrality Ratio Methodology

This appendix section describes the *Revenue Neutrality Ratio* ($RNR$) methodology, including the construction of $Counterfactual\ Revenue$ ($CR$) and *Policy Revenue* ($PR$) for each level of government. The analysis builds on the MVPF framework and incorporates two additional inputs. First, I allocate enrollment-related public higher education costs between state and federal governments using revenue shares reported in Pew (2015). Second, I use the long-run state residence results in two ways: to allocate counterfactual tax revenue from earnings between in-state and out-of-state governments, and to capture policy-induced shifts in revenue resulting from migration effects.

$Counterfactual\ Revenue$ ($CR$). $CR$ is constructed by applying relevant tax rates to the distribution of earnings for individuals over ages 19-65 in strong merit states prior to program adoption. This yields the present discounted value of lifetime tax revenue absent the policy, discounting future revenues to age 18 at a 3 percent annual rate, expressed in 2019 dollars, and assuming earnings grow at 0.5 percent annually, consistent with the MVPF analysis.

For state governments, I apply a 2.6 p.p. marginal tax rate to age-specific earnings and sum discounted revenues over the lifecycle. State $CR$ is then allocated between in-state and out-of-state governments using the estimated level of long-run in-state residence.

For the federal government, I link counterfactual earnings to effective marginal tax rates from CBO (2016), consistent with the MVPF calculation. These effective marginal tax rates are defined net of state income and payroll taxes. Age-specific rates are applied to the earnings distribution, and discounted revenues are summed over the lifecycle to obtain cumulative federal $CR$.

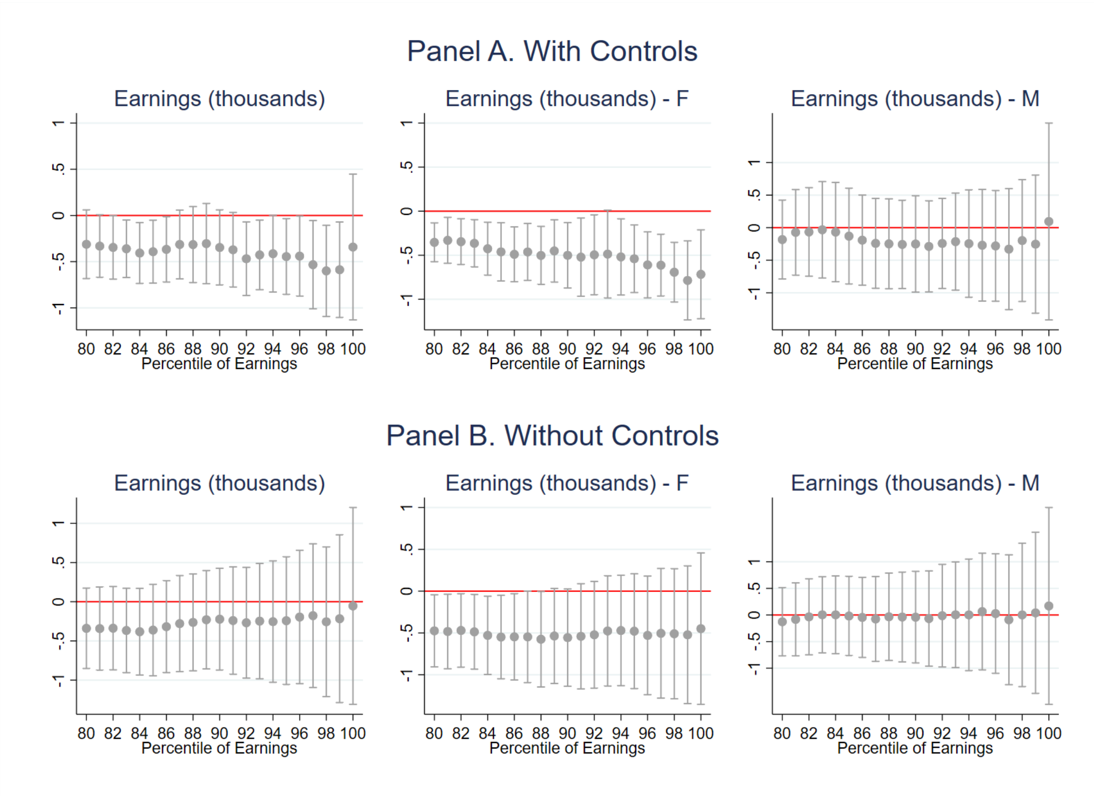
*Policy Revenue* ($PR$). $PR$ equals $CR$ plus the net fiscal effects of the program. Direct award costs, enrollment-induced spending changes, and earnings-related tax effects are taken from the MVPF analysis, and I incorporate estimates from my analysis of long-run state residence to allocate revenue across jurisdictions.

Incremental public higher education costs induced by enrollment responses are allocated between state and federal governments in proportion to observed revenue shares reported in Pew (2015) (21 percent state, 16 percent federal). These estimated revenue shares are based on IPEDS data in 2013.

Earnings-related tax effects are constructed using the combined effective marginal tax rates from the MVPF analysis for three age ranges (federal plus state, excluding payroll taxes). I recover the effective federal marginal tax rate by subtracting 2.6 p.p., the average state income tax component reported in CBO (2016), from the combined rate. I then apply the resulting federal and state marginal tax rates separately to the merit-induced changes in pre-tax earnings within each age range. The state component of the earnings-related tax effect is allocated between in-state and out-of-state governments using the estimated level of long-run in-state residence (Table 7). Finally, I incorporate my estimated TOT effect on long-run in-state retention to capture migration-induced shifts in tax revenue across jurisdictions.

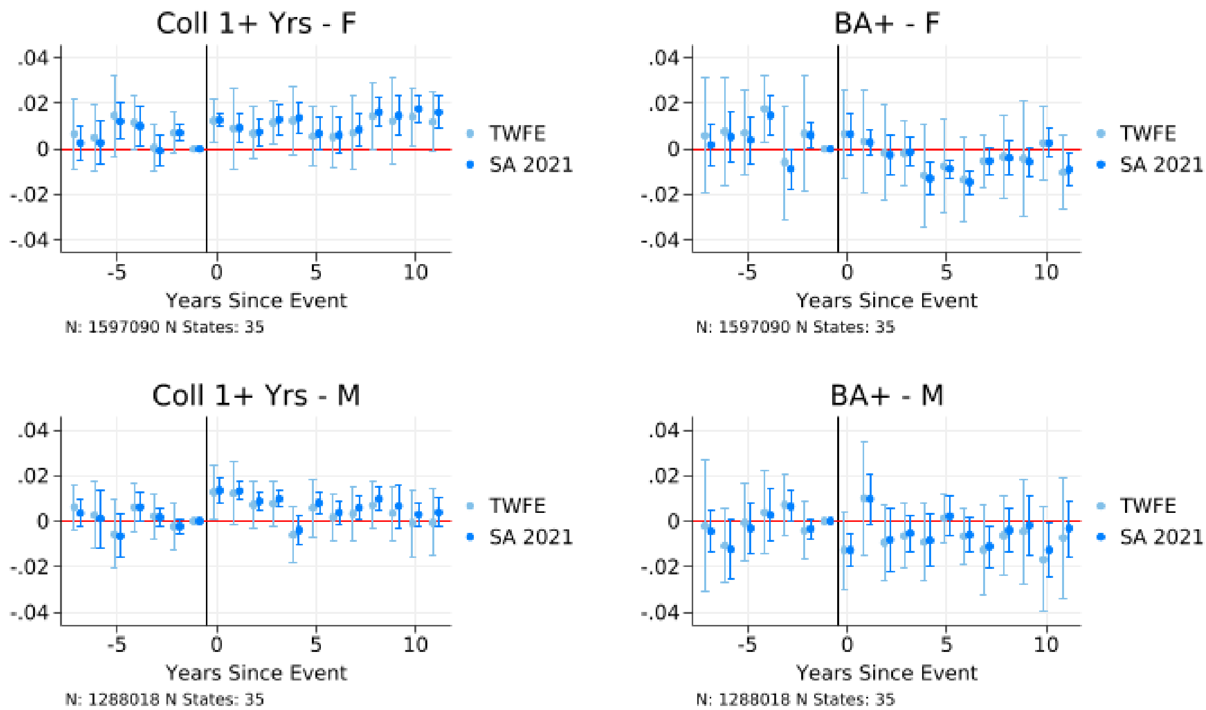
# Appendix C Additional Figures and Tables

Figure C1: Estimates of Merit Program Effects on Earnings Trimmed at Different Percentiles



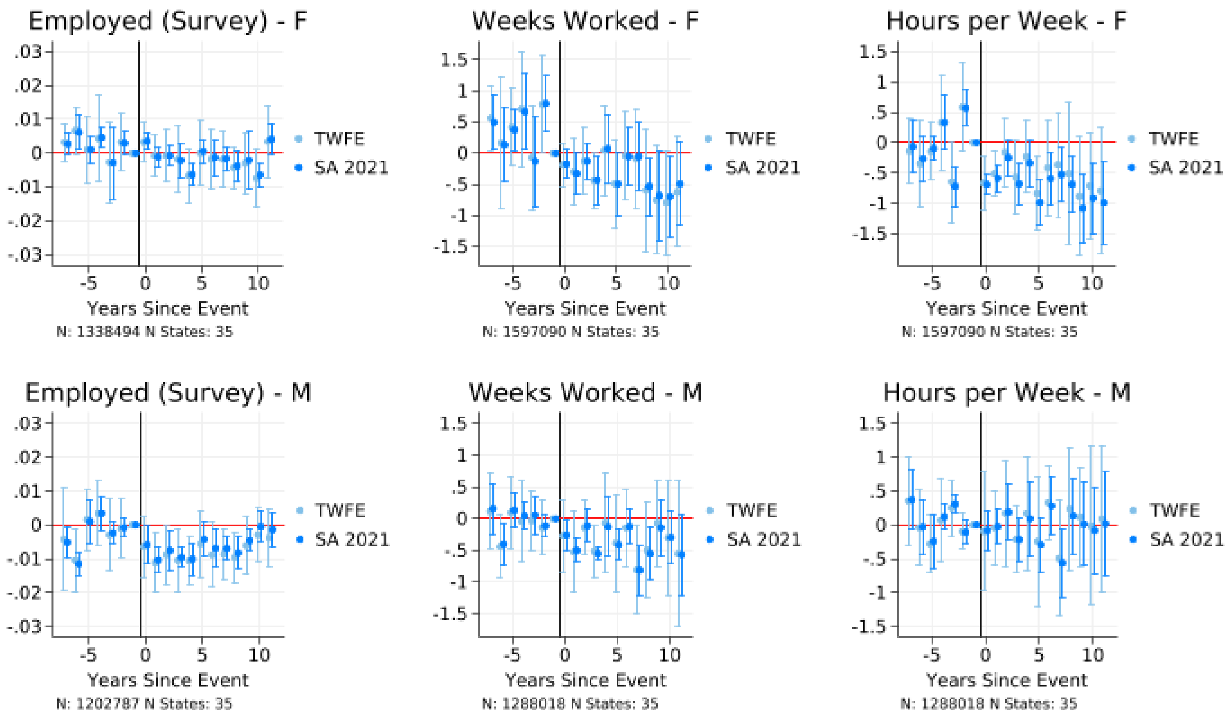

*Notes*: The figure reports coefficients and 95 percent confidence intervals for the effects of strong merit aid program exposure on earnings (in thousands of 2019 dollars), computed for samples with the top percentiles of earners removed, varying the trimming threshold from the 80th percentile to the 100th percentile. For example, the estimate at the 80th percentile excludes the top 20 percent of earners, while the estimate at the 100th percentile includes the full sample. The analytic sample and earnings outcomes are described in Section 4. Panel A shows estimates based on the main TWFE specification with rich controls, as described in Section 5.1 and Table 3, while Panel B shows estimates excluding the rich set of controls. Estimates compare changes in outcomes between exposed and unexposed cohorts in the 10 strong merit aid states and 25 non-merit aid states; the 15 weak merit aid states and Alaska are excluded. Regressions are weighted using the person weight variable. Robust standard errors are clustered at the birth state level, and 95 percent confidence intervals are displayed.

Figure C2: Event Study Estimates of Merit Program Effects on Educational Attainment



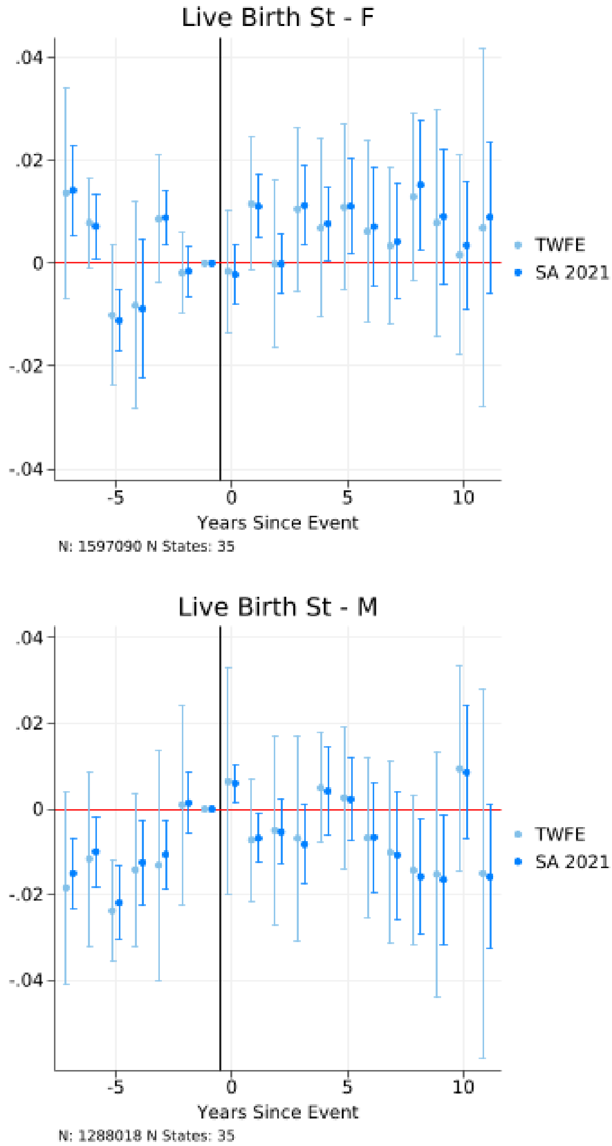

*Notes*: The figure reports event study estimates of the effects of strong merit program exposure on educational attainment. Regressions include lead and lag indicators for exposure to strong merit aid programs, along with birth state and birth cohort fixed effects. The analytic sample and educational attainment outcomes are as described in Section 4. Outcomes are: College 1+ Years (completion of one or more years of college) and BA+ (completion of a bachelor's degree or higher). Treatment is defined by exposure to a strong merit aid program in an individual's birth state when the cohort reached age 18; the comparison group includes cohorts in the 25 non-merit aid states and cohorts in adopting states prior to program implementation. Estimates are shown using both the two-way fixed effects (TWFE) estimator and the Sun and Abraham (2021) estimator. Event time is centered at -1 for the cohort that reached age 18 in the year prior to program adoption, so negative values correspond to pre-adoption cohorts and zero and positive values to post-adoption cohorts. Regressions are weighted using the person weight variable. Robust standard errors are clustered at the birth state level, and 95 percent confidence intervals are displayed.

Figure C3: Event Study Estimates of Merit Program Effects on Alternative Employment Outcomes



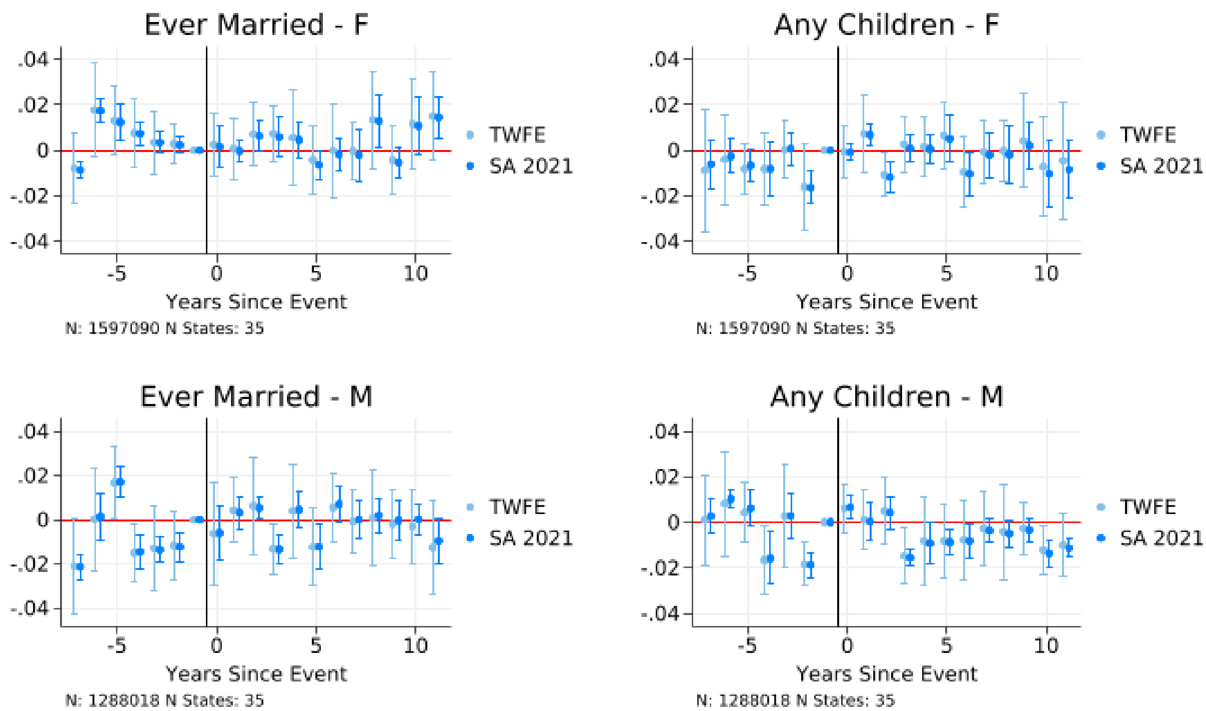

*Notes*: The figure reports event study estimates of the effects of strong merit aid program exposure on alternative employment outcomes including employment at the time of the survey, weeks worked, and hours worked. Regressions include lead and lag indicators for program exposure, along with birth state and birth cohort fixed effects. The analytic sample and employment outcomes are described in Section 4. Treatment is defined by exposure to a strong merit aid program in an individual's birth state when the cohort reached age 18; the comparison group includes cohorts in the 25 non-merit aid states and cohorts in adopting states prior to program implementation. Estimates are shown using both the two-way fixed effects (TWFE) estimator and the Sun and Abraham (2021) estimator. Event time is centered at -1 for the cohort that reached age 18 in the year prior to program adoption, so negative values correspond to pre-adoption cohorts and zero and positive values to post-adoption cohorts. Regressions are weighted using the person weight variable. Robust standard errors are clustered at the birth state level, and 95 percent confidence intervals are displayed.

Figure C4: Event Study Estimates of Merit Program Effects on State Residence



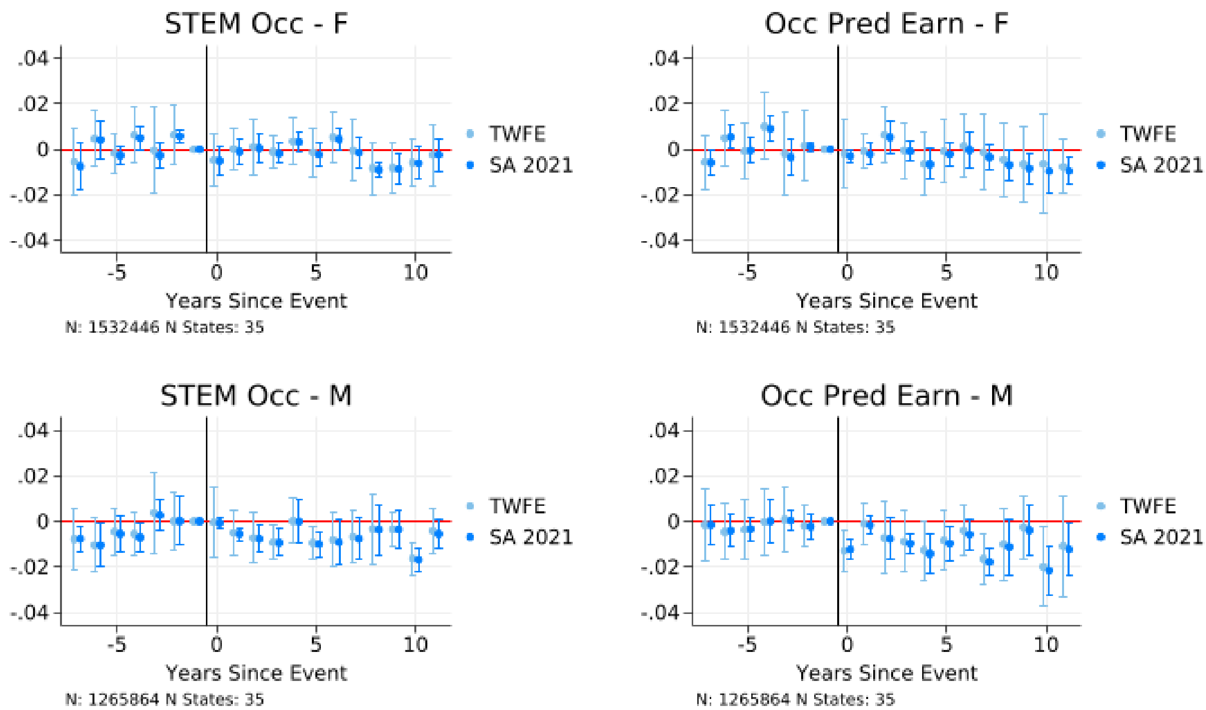

*Notes*: The figure reports event study estimates of the effects of strong merit aid program exposure on state residence. The state residence outcome is an indicator for living in one's birth state. Regressions include lead and lag indicators for program exposure, along with birth state and birth cohort fixed effects. The analytic sample is described in Section 4. Treatment is defined by exposure to a strong merit aid program in an individual's birth state when the cohort reached age 18; the comparison group includes cohorts in the 25 non-merit aid states and cohorts in adopting states prior to program implementation. Estimates are shown using both the two-way fixed effects (TWFE) estimator and the Sun and Abraham (2021) estimator. Event time is centered at -1 for the cohort that reached age 18 in the year prior to program adoption, so negative values correspond to pre-adoption cohorts and zero and positive values to post-adoption cohorts. Regressions are weighted using the person weight variable. Robust standard errors are clustered at the birth state level, and 95 percent confidence intervals are displayed.

Figure C5: Event Study Estimates of Merit Program Effects on Family Formation



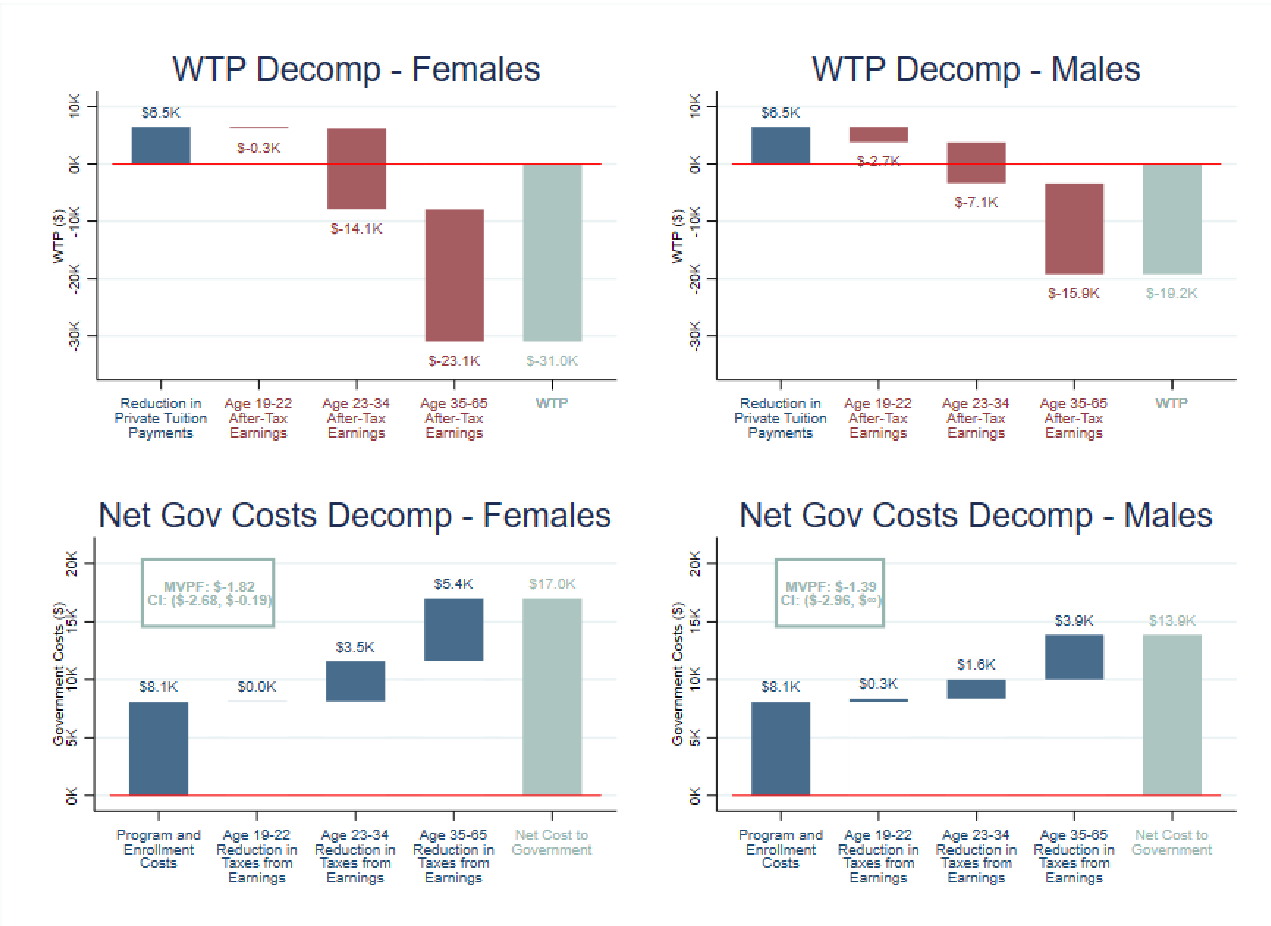

*Notes*: The figure reports event study estimates of the effects of strong merit aid program exposure on family formation outcomes. Regressions include lead and lag indicators for program exposure, along with birth state and birth cohort fixed effects. The analytic sample and family formation outcomes are described in Section 4. Treatment is defined by exposure to a strong merit aid program in an individual's birth state when the cohort reached age 18; the comparison group includes cohorts in the 25 non-merit aid states and cohorts in adopting states prior to program implementation. Estimates are shown using both the two-way fixed effects (TWFE) estimator and the Sun and Abraham (2021) estimator. Event time is centered at -1 for the cohort that reached age 18 in the year prior to program adoption, so negative values correspond to pre-adoption cohorts and zero and positive values to post-adoption cohorts. Regressions are weighted using the person weight variable. Robust standard errors are clustered at the birth state level, and 95 percent confidence intervals are displayed.

Figure C6: Event Study Estimates of Merit Program Effects on Occupations




*Notes*: The figure reports event study estimates of the effects of strong merit aid program exposure on occupation outcomes. Regressions include lead and lag indicators for program exposure, along with birth state and birth cohort fixed effects. The analytic sample and occupation outcomes are described in Section 4. STEM occupations are classified following Langdon et al. (2011) and Vilorio (2014). The occupation-predicted earnings variable is measured using coefficients from a regression of log earnings on occupation fixed effects. Treatment is defined by exposure to a strong merit aid program in an individual's birth state when the cohort reached age 18; the comparison group includes cohorts in the 25 non-merit aid states and cohorts in adopting states prior to program implementation. Estimates are shown using both the two-way fixed effects (TWFE) estimator and the Sun and Abraham (2021) estimator. Event time is centered at -1 for the cohort that reached age 18 in the year prior to program adoption, so negative values correspond to pre-adoption cohorts and zero and positive values to post-adoption cohorts. Regressions are weighted using the person weight variable. Robust standard errors are clustered at the birth state level, and 95 percent confidence intervals are displayed.

Figure C7: Willingness to Pay and Government Cost Components by Gender for Strong Merit Programs




*Notes*: This figure decomposes the marginal value of public funds (MVPF) for strong merit aid programs in the 10 adopting states into willingness to pay (WTP) and net government cost components, separately for females and males. WTP is measured as the change in net tuition payments (inclusive of the merit aid award) plus the change in after-tax earnings. The net government cost includes direct program expenditures, changes in public higher education spending due to enrollment responses, and changes in tax revenue generated by earnings effects. The MVPF is defined as the ratio of WTP to net government cost. Estimates are expressed in 2019 dollars and discounted at a 3 percent real annual rate. Bootstrapped 95 percent confidence intervals are reported for the MVPF. An unbounded (infinite) upper bound reflects bootstrap draws in which sufficiently large positive earnings effects generate enough additional tax revenue to imply net government savings.

Table C1: Summary Statistics for Postsecondary Institutions

| | (1) |
|---|---|
| 4-Year | 0.749 |
| 2-Year | 0.251 |
| Public | 0.775 |
| Private | 0.225 |
| For-Profit | 0.021 |
| Flagship | 0.112 |
| Net Tuition | 10,894 |
| Earnings | 49,504 |
| Graduation Rate | 0.534 |
| SAT Average | 1,201 |
| N Inst | 4,951 |
| N | 2,064,541 |

*Notes*: This table presents summary statistics for postsecondary institutions. Fall 2020 enrollment data come from the Integrated Postsecondary Education Data System (IPEDS) Residence and Migration Survey (NCES, 2023b). These data are linked to institution-level information on average net tuition revenue per student in 1989-90 from the Delta Cost Project (NCES, 2015), measured prior to the introduction of the first strong merit aid program. Additional institution characteristics, including median earnings, graduation rates, and average SAT scores, are drawn from the College Scorecard (US Department of Education, 2024). The table reports the distribution of students across postsecondary sectors, mean net tuition, median earnings in 2020 for the 2009-10 entry cohort, and graduation rates within 150 percent of expected time to degree. Average SAT scores are for first-time students enrolling in 2019-20. Statistics are student-weighted averages. Tuition is converted to 2019 dollars using a higher education tuition deflator based on the ratio of average tuition in 2018-19 relative to 1989-90 (NCES, 2023a), and earnings are reported in 2019 dollars.

Table C2: Merit Program Effects on Earnings by Age

| | (1)<br>Ages 19-22 | (2)<br>Ages 23-24 | (3)<br>Ages 25-34 |
|---|---|---|---|
| **A. Females & Males** | | | |
| Merit State | -61 | -705** | -314 |
| | (178) | (340) | (200) |
| Unexposed Mean | 9,340 | 19,961 | 33,405 |
| Percent Change | {-.65%} | {-3.53%} | {-.94%} |
| TOT | [-240] | [-2,799] | [-1,246] |
| N | 620,802 | 400,104 | 2,189,621 |
| **B. Females** | | | |
| Merit State | -22 | -673 | -530*** |
| | (261) | (555) | (185) |
| Unexposed Mean | 8,669 | 18,199 | 27,262 |
| Percent Change | {-.26%} | {-3.7%} | {-1.94%} |
| TOT | [-88] | [-2,673] | [-2,103] |
| N | 346,151 | 223,274 | 1,210,763 |
| **C. Males** | | | |
| Merit State | -204 | -1261** | -76 |
| | (247) | (547) | (309) |
| Unexposed Mean | 10,492 | 22,656 | 42,340 |
| Percent Change | {-1.94%} | {-5.56%} | {-.18%} |
| TOT | [-808] | [-5,003] | [-301] |
| N | 274,764 | 176,780 | 975,413 |
| Earnings <90p | X | X | X |

*Notes*: This table reports ITT estimates of the impact of strong merit aid program exposure on earnings by age group. Columns (1)-(3) report estimates for ages 19-22, 23-24, and 25-34, respectively. Earnings are measured as earnings below the 90th percentile in 2019 dollars. The analytic sample is described in Section 4, except that column (1) includes individuals ages 19-22. Estimates compare changes in outcomes between exposed and unexposed cohorts in the 10 strong merit aid states and the 25 non-merit aid states; the 15 weak merit aid states and Alaska are excluded. I use my main TWFE specification described in Section 5.1 and Table 3. Statistics in braces report the coefficient divided by the unexposed mean, representing the percent change in earnings for the 10 strong merit aid states. The implied treatment-on-the-treated (TOT) effect is computed by dividing the ITT coefficient by 0.252, the share of students in strong merit states receiving merit aid in 2019-2020 (NASSGAP, 2020). Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** p<0.01, ** p<0.05, * p<0.10

Table C3: Merit Program Effects on Educational Attainment: Sample Including All High School Graduates

| | (1) College Enroll+ | (2) College 1+ Years | (3) AA+ | (4) BA+ | (5) MA+ |
|---|---|---|---|---|---|
| **A. Females & Males** | | | | | |
| Merit State | 0.0004 | 0.0064 | 0.0009 | -0.0008 | -0.0023 |
| | (0.0061) | (0.0048) | (0.0044) | (0.0046) | (0.0019) |
| Unexposed Mean | .6539 | .571 | .3699 | .281 | .0701 |
| Percent Change | {.06%} | {1.13%} | {.23%} | {-.28%} | {-3.26%} |
| TOT | [.0015] | [.0255] | [.0034] | [-.0032] | [-.0091] |
| N | 3,940,967 | 3,940,967 | 3,940,967 | 3,940,967 | 3,940,967 |
| **B. Females** | | | | | |
| Merit State | -0.0015 | 0.0025 | -0.0036 | -0.0013 | -0.0025 |
| | (0.0067) | (0.0049) | (0.0045) | (0.0045) | (0.0024) |
| Unexposed Mean | .6943 | .6098 | .4059 | .3052 | .0804 |
| Percent Change | {-.22%} | {.41%} | {-.88%} | {-.44%} | {-3.1%} |
| TOT | [-.0061] | [.01] | [-.0141] | [-.0053] | [-.0099] |
| N | 2,075,544 | 2,075,544 | 2,075,544 | 2,075,544 | 2,075,544 |
| **C. Males** | | | | | |
| Merit State | 0.0020 | 0.0103* | 0.0055 | -0.0005 | -0.0024 |
| | (0.0058) | (0.0051) | (0.0051) | (0.0055) | (0.0019) |
| Unexposed Mean | .609 | .5279 | .33 | .2542 | .0588 |
| Percent Change | {.33%} | {1.94%} | {1.66%} | {-.19%} | {-4.01%} |
| TOT | [.0081] | [.0407] | [.0218] | [-.0019] | [-.0093] |
| N | 1,865,423 | 1,865,423 | 1,865,423 | 1,865,423 | 1,865,423 |

*Notes*: This table reports ITT estimates of the impact of strong merit aid program exposure on educational attainment. The analytic sample is described in Section 4, with one exception: the sample includes all high school graduates (including those who do not enroll in college). Outcomes are defined as follows: College Enroll+ (enroll in college or higher), College 1+ Years (completion of one or more years of college), AA+ (completion of an associate degree or higher), BA+ (completion of a bachelor's degree or higher), and MA+ (completion of a master's degree or higher). Estimates compare changes in outcomes between exposed and unexposed cohorts in the 10 strong merit aid states and the 25 non-merit aid states; the 15 weak merit aid states and Alaska are excluded. I use my main TWFE specification described in Section 5.1 and Table 3. Statistics in braces report the coefficient divided by the unexposed mean, representing the percent change in the outcome for the 10 strong merit aid states. The implied treatment-on-the-treated (TOT) effect is computed by dividing the ITT coefficient by 0.252, the share of students in strong merit states receiving merit aid in 2019-2020 (NASSGAP, 2020). Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table C4: Summary Statistics for Census and ACS Data

| | (1) Females & Males | | (2) Females | | (3) Males | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| Female | 0.543 | 0.498 | 1.000 | 0.000 | 0.000 | 0.000 |
| Black | 0.104 | 0.305 | 0.115 | 0.319 | 0.090 | 0.286 |
| Hispanic | 0.093 | 0.291 | 0.095 | 0.293 | 0.091 | 0.288 |
| White | 0.751 | 0.432 | 0.739 | 0.439 | 0.765 | 0.424 |
| Asian | 0.025 | 0.156 | 0.023 | 0.150 | 0.027 | 0.162 |
| Other Race | 0.027 | 0.162 | 0.028 | 0.164 | 0.026 | 0.159 |
| Age | 28.505 | 3.419 | 28.494 | 3.420 | 28.517 | 3.418 |
| Strong Merit State | 0.071 | 0.257 | 0.073 | 0.260 | 0.068 | 0.252 |
| Any Merit State | 0.166 | 0.372 | 0.169 | 0.374 | 0.163 | 0.369 |
| College 1+ Years | 0.895 | 0.306 | 0.901 | 0.299 | 0.888 | 0.315 |
| AA+ | 0.621 | 0.485 | 0.641 | 0.480 | 0.598 | 0.490 |
| BA+ | 0.486 | 0.500 | 0.502 | 0.500 | 0.466 | 0.499 |
| MA+ | 0.117 | 0.322 | 0.131 | 0.337 | 0.101 | 0.302 |
| STEM Major | 0.220 | 0.414 | 0.151 | 0.358 | 0.310 | 0.463 |
| STEM Occ | 0.159 | 0.365 | 0.156 | 0.362 | 0.162 | 0.369 |
| Ever Married | 0.513 | 0.500 | 0.549 | 0.498 | 0.471 | 0.499 |
| Any Children | 0.389 | 0.488 | 0.459 | 0.498 | 0.307 | 0.461 |
| Num of Children | 0.710 | 1.056 | 0.846 | 1.112 | 0.549 | 0.961 |
| Employed (Earnings > 0) | 0.905 | 0.293 | 0.873 | 0.333 | 0.943 | 0.231 |
| Earnings | 41,209 | 41,959 | 33,781 | 33,185 | 50,026 | 48,987 |
| Employed (Survey) | 0.949 | 0.220 | 0.949 | 0.219 | 0.949 | 0.220 |
| Labor Force | 0.880 | 0.325 | 0.839 | 0.368 | 0.929 | 0.257 |
| Weeks Worked | 41.695 | 17.122 | 39.531 | 18.655 | 44.263 | 14.694 |
| Hours per Week | 36.443 | 16.018 | 33.126 | 16.232 | 40.381 | 14.827 |
| Live in Birth State | 0.631 | 0.483 | 0.638 | 0.481 | 0.623 | 0.485 |
| N | 4,348,091 | | 2,397,346 | | 1,950,745 | |

*Notes*: The analytic sample described in Section 4 includes all U.S.-born college attendees ages 23-34 in the 5 percent and 1 percent samples of the 2000 Census and the 2000-2019 ACS with unimputed information on age, birth state, educational attainment, employment status, and earnings. "Other Race" includes multiracial individuals, American Indian/Alaska Native individuals, and individuals reporting another unclassified race. The strong merit state variable is an indicator for being born in one of the 10 strong merit aid states and reaching age 18 after program implementation. The any merit state variable is defined analogously for all 25 merit aid states. Educational attainment is defined as completing at least one year of college (College 1+ Years), earning an associate degree or higher (AA+), a bachelor's degree or higher (BA+), and a master's degree or higher (MA+). STEM majors are classified following Sjoquist and Winters (2015a), and STEM occupations are classified following Langdon et al. (2011) and Vilorio (2014). Earnings are measured in 2019 dollars. Statistics are computed using the person weight variable provided by the Census and ACS.

Table C5: Balance Tests Between Strong Merit and Non-Merit States

| | (1)<br>Non-Merit<br>State Mean | (2)<br>Strong Merit<br>State Mean | (3)<br>Difference | (4)<br>P-Value |
|---|---|---|---|---|
| Female | 0.542 | 0.559 | 0.018 | 0.000 |
| Black | 0.086 | 0.186 | 0.100 | 0.000 |
| Hispanic | 0.117 | 0.060 | -0.057 | 0.000 |
| White | 0.739 | 0.723 | -0.017 | 0.000 |
| Asian | 0.030 | 0.009 | -0.021 | 0.000 |
| Other Race | 0.028 | 0.023 | -0.005 | 0.000 |
| Age | 28.492 | 28.323 | -0.169 | 0.000 |
| Strong Merit State | 0.000 | 0.633 | 0.633 | 0.000 |
| College 1+ Years | 0.893 | 0.885 | -0.007 | 0.000 |
| AA+ | 0.612 | 0.577 | -0.035 | 0.000 |
| BA+ | 0.477 | 0.437 | -0.040 | 0.000 |
| MA+ | 0.109 | 0.109 | -0.000 | 0.878 |
| STEM Major | 0.224 | 0.206 | -0.018 | 0.000 |
| STEM Occ | 0.159 | 0.156 | -0.003 | 0.001 |
| Ever Married | 0.516 | 0.540 | 0.023 | 0.000 |
| Any Children | 0.390 | 0.426 | 0.036 | 0.000 |
| Num of Children | 0.710 | 0.769 | 0.059 | 0.000 |
| Employed (Earnings $>$ 0) | 0.907 | 0.893 | -0.014 | 0.000 |
| Earnings | 40,863 | 36,703 | -4,160 | 0.000 |
| Employed (Survey) | 0.951 | 0.943 | -0.008 | 0.000 |
| Labor Force | 0.881 | 0.871 | -0.010 | 0.000 |
| Weeks Worked | 41.817 | 40.971 | -0.846 | 0.000 |
| Hours per Week | 36.510 | 35.903 | -0.607 | 0.000 |
| Live in Birth State | 0.647 | 0.623 | -0.024 | 0.000 |
| N | 2,419,903 | 465,205 | 2,885,108 | |
| N States | 25 | 10 | 35 | |

*Notes*: Balance tests are conducted for the analytic sample described in Section 4. I test for differences in observable characteristics between non-merit and strong merit states using an indicator for ever being born in one of the 10 strong merit aid states. Column (1) reports the non-merit state mean, column (2) reports the strong merit state mean, column (3) reports the coefficient from a regression of the characteristic on the ever strong merit indicator, and column (4) reports the associated p-value. Earnings are measured in 2019 dollars. Statistics are computed using the person weight variable provided by the Census and ACS. Regressions use robust standard errors.

Table C6: Merit Program Effects on Log Enrollment with Different Estimators

| | (1) 2-Year | (2) 2-Year | (3) 4-Year | (4) 4-Year | (5) All | (6) All |
|---|---|---|---|---|---|---|
| **A. Log Out-of-State Enrollment** | | | | | | |
| Merit State | -0.1450 | -0.0550 | -0.1966** | -0.1830*** | -0.1864*** | -0.1675*** |
| | (0.1029) | (0.0787) | (0.0773) | (0.0462) | (0.0714) | (0.0432) |
| N | 543 | 543 | 543 | 543 | 543 | 543 |
| **B. Log In-State Enrollment** | | | | | | |
| Merit State | 0.2318** | 0.1152 | 0.1507 | 0.1455*** | 0.1728* | 0.1272*** |
| | (0.1148) | (0.0926) | (0.0998) | (0.0416) | (0.0912) | (0.0405) |
| N | 529 | 529 | 543 | 543 | 543 | 543 |
| **C. Log Total Enrollment** | | | | | | |
| Merit State | 0.1939* | 0.0733 | 0.0777 | 0.0705* | 0.1097 | 0.0744** |
| | (0.0990) | (0.0831) | (0.0849) | (0.0406) | (0.0758) | (0.0370) |
| N | 543 | 543 | 543 | 543 | 543 | 543 |
| TWFE | X | | X | | X | |
| SA 2021 | | X | | X | | X |

*Notes*: This table reports estimates of the impact of strong merit aid programs on log college enrollment using data from the Integrated Postsecondary Education Data System (IPEDS) Residence and Migration Survey NCES (2023b). IPEDS provides counts of recent high school graduates enrolling in each institution by their state of residence. Log enrollment is computed at the state-by-year level. Columns (1)-(2) report results for 2-year institutions, columns (3)-(4) for 4-year institutions, and columns (5)-(6) for all institutions. Columns (1), (3), and (5) show estimates from a two-way fixed effects (TWFE) model with state and year fixed effects, while columns (2), (4), and (6) show estimates using the Sun and Abraham (2021) estimator. Panel A reports log out-of-state enrollment, Panel B reports log in-state enrollment, and Panel C reports log total enrollment. Estimates compare changes in log enrollment before and after program adoption between the 10 strong merit aid states and 25 non-merit aid states; the 15 weak merit aid states and Alaska are excluded. Robust standard errors are clustered at the state level.
*** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table C7: Merit Program Effects on Labor Market Outcomes with Different Estimators

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Employed | Employed | Earnings | Earnings | Labor Force | Labor Force |
| **A. Females & Males** | | | | | | |
| Merit State | -0.0048* | -0.0042* | -178 | -205* | -0.0040* | 0.0008 |
| | (0.0026) | (0.0026) | (137) | (119) | (0.0023) | (0.0051) |
| Unexposed Mean | 0.9080 | 0.9080 | 31,445 | 31,445 | 0.8722 | 0.8722 |
| Percent Change | {-0.53%} | {-0.47%} | {-0.56%} | {-0.65%} | {-0.46%} | {0.09%} |
| TOT | [-0.0190] | [-0.0168] | [-705] | [-815] | [-0.0159] | [0.0031] |
| N | 2,885,108 | 2,885,108 | 2,583,974 | 2,583,974 | 2,885,108 | 2,885,108 |
| **B. Females** | | | | | | |
| Merit State | -0.0081*** | -0.0075** | -491*** | -395*** | -0.0080*** | 0.0011 |
| | (0.0029) | (0.0034) | (174) | (121) | (0.0026) | (0.0063) |
| Unexposed Mean | 0.8710 | 0.8710 | 25,880 | 25,880 | 0.8244 | 0.8244 |
| Percent Change | {-0.93%} | {-0.86%} | {-1.90%} | {-1.53%} | {-0.98%} | {0.13%} |
| TOT | [-0.0322] | [-0.0296] | [-1,946] | [-1,567] | [-0.0319] | [0.0043] |
| N | 1,597,090 | 1,597,090 | 1,432,714 | 1,432,714 | 1,597,090 | 1,597,090 |
| **C. Males** | | | | | | |
| Merit State | -0.0014 | -0.0014 | -13 | -227 | -0.0002 | -0.0013 |
| | (0.0031) | (0.0019) | (163) | (177) | (0.0034) | (0.0039) |
| Unexposed Mean | 0.9548 | 0.9548 | 39,607 | 39,607 | 0.9328 | 0.9328 |
| Percent Change | {-0.15%} | {-0.15%} | {-0.03%} | {-0.57%} | {-0.02%} | {-0.14%} |
| TOT | [-0.0056] | [-0.0056] | [-53] | [-902] | [-0.0007] | [-0.0052] |
| N | 1,288,018 | 1,288,018 | 1,151,993 | 1,151,993 | 1,288,018 | 1,288,018 |
| TWFE | X | | X | | X | |
| SA 2021 | | X | | X | | X |
| Earnings <90p | | | X | X | | |

*Notes*: This table reports ITT estimates of the impact of strong merit aid program exposure on labor market outcomes. Outcomes include employment (positive earnings), earnings below the 90th percentile in 2019 dollars, and labor force participation. The analytic sample is described in Section 4. Columns (1), (3), and (5) present estimates from the TWFE specification shown in Tables 4 and 5, column (3), while columns (2), (4), and (6) present estimates using the Sun and Abraham (2021) estimator with the same set of controls. Linear trend controls are omitted from the Sun and Abraham (2021) specifications because such trends can absorb variation used to identify dynamic treatment effects. For comparability, the corresponding TWFE specifications also exclude linear trend controls. Statistics in braces report the coefficient divided by the unexposed mean, representing the percent change in the outcome for the 10 strong merit aid states. The implied TOT effect is computed by dividing the ITT coefficient by 0.252, the share of students in strong merit states receiving merit aid in 2019-2020 (NASSGAP, 2020). Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.

*** p<0.01, ** p<0.05, * p<0.10

Table C8: Descriptive Statistics on Net Tuition and Student Outcomes by Postsecondary Sector

| (1)<br>Sector | (2)<br>Out-of-State | (3)<br>In-State | (4)<br>N Inst | (5)<br>N |
|---|---|---|---|---|
| **A. Net Tuition** | | | | |
| All | 16,440 | 6,907 | 4,951 | 2,064,541 |
| 4-Year | 19,799 | 7,557 | 2,113 | 1,545,506 |
| 2-Year | 5,182 | 2,255 | 2,838 | 519,035 |
| **B. Earnings** | | | | |
| All | 50,432 | 44,629 | 4,951 | 2,064,541 |
| 4-Year | 55,567 | 48,386 | 2,113 | 1,545,506 |
| 2-Year | 36,609 | 34,681 | 2,838 | 519,035 |
| **C. Graduation Rates** | | | | |
| All | 0.539 | 0.484 | 4,951 | 2,064,541 |
| 4-Year | 0.623 | 0.533 | 2,113 | 1,545,506 |
| 2-Year | 0.317 | 0.361 | 2,838 | 519,035 |
| **D. SAT Average** | | | | |
| 4-Year | 1,204 | 1,180 | 2,113 | 1,545,506 |

*Notes*: This table presents descriptive statistics on net tuition and student outcomes by postsecondary sector for all colleges in the linked IPEDS enrollment data (NCES, 2023b), Delta Cost Project net tuition data (NCES, 2015), and College Scorecard student outcomes data (US Department of Education, 2024). The data sources and variable definitions are detailed in the notes for Table C1. Average net tuition revenue per student is from the 1989-90 academic year, prior to the introduction of the first strong merit program. Column (1) shows the college sector (all institutions, 4-year, and 2-year). Column (2) reports means for out-of-state institutions located in the 15 weak merit states and 25 non-merit states. Column (3) reports means for in-state institutions located in the 10 strong merit states. Column (4) reports the number of institutions, and column (5) reports the number of first-time students. Statistics are computed as student-weighted averages. Tuition is converted to 2019 dollars using a higher education tuition deflator based on the ratio of average tuition in 2018-19 relative to 1989-90 (NCES, 2023a), and earnings are reported in 2019 dollars.

Table C9: Merit Program Effects on Educational Attainment with Different Specifications

| | (1) College 1+ Years | (2) College 1+ Years | (3) College 1+ Years | (4) College 1+ Years | (5) BA+ | (6) BA+ | (7) BA+ | (8) BA+ |
|---|---|---|---|---|---|---|---|---|
| **A. Females & Males** | | | | | | | | |
| Merit State | 0.0040 | 0.0043 | 0.0038 | 0.0093*** | -0.0071** | -0.0067* | -0.0079 | -0.0014 |
| | (0.0040) | (0.0042) | (0.0042) | (0.0031) | (0.0031) | (0.0037) | (0.0054) | (0.0053) |
| Unexposed Mean | 0.8733 | 0.8733 | 0.8733 | 0.8733 | 0.4298 | 0.4298 | 0.4298 | 0.4298 |
| Percent Change | {0.46%} | {0.49%} | {0.43%} | {1.07%} | {-1.64%} | {-1.55%} | {-1.85%} | {-0.33%} |
| TOT | [0.0159] | [0.0169] | [0.0149] | [0.0371] | [-0.0280] | [-0.0265] | [-0.0315] | [-0.0057] |
| N | 2,885,108 | 2,885,108 | 2,885,108 | 2,885,108 | 2,885,108 | 2,885,108 | 2,885,108 | 2,885,108 |
| **B. Females** | | | | | | | | |
| Merit State | 0.0037 | 0.0038 | 0.0032 | 0.0058 | -0.0087** | -0.0086* | -0.0098 | -0.0006 |
| | (0.0045) | (0.0047) | (0.0046) | (0.0038) | (0.0039) | (0.0044) | (0.0067) | (0.0058) |
| Unexposed Mean | 0.8784 | 0.8784 | 0.8784 | 0.8784 | 0.4395 | 0.4395 | 0.4395 | 0.4395 |
| Percent Change | {0.42%} | {0.44%} | {0.36%} | {0.66%} | {-1.98%} | {-1.97%} | {-2.23%} | {-0.14%} |
| TOT | [0.0146] | [0.0152] | [0.0127] | [0.0231] | [-0.0345] | [-0.0343] | [-0.0389] | [-0.0025] |
| N | 1,597,090 | 1,597,090 | 1,597,090 | 1,597,090 | 1,597,090 | 1,597,090 | 1,597,090 | 1,597,090 |
| **C. Males** | | | | | | | | |
| Merit State | 0.0041 | 0.0044 | 0.0041 | 0.0136*** | -0.0057 | -0.0051 | -0.0065 | -0.0028 |
| | (0.0038) | (0.0041) | (0.0041) | (0.0040) | (0.0037) | (0.0044) | (0.0051) | (0.0064) |
| Unexposed Mean | 0.8668 | 0.8668 | 0.8668 | 0.8668 | 0.4174 | 0.4174 | 0.4174 | 0.4174 |
| Percent Change | {0.48%} | {0.51%} | {0.47%} | {1.57%} | {-1.36%} | {-1.22%} | {-1.55%} | {-0.67%} |
| TOT | [0.0163] | [0.0175] | [0.0163] | [0.0540] | [-0.0224] | [-0.0202] | [-0.0258] | [-0.0111] |
| N | 1,288,018 | 1,288,018 | 1,288,018 | 1,288,018 | 1,288,018 | 1,288,018 | 1,288,018 | 1,288,018 |
| Controls Demo | | X | X | X | | X | X | X |
| Controls Econ | | | X | X | | | X | X |
| Controls Trends | | | | X | | | | X |

*Notes*: This table reports ITT estimates of the impact of exposure to strong merit aid programs on educational attainment. The analytic sample is described in Section 4. Outcomes are: College 1+ Years (completion of one or more years of college) and BA+ (completion of a bachelor's degree or higher). Estimates compare changes in outcomes between exposed and unexposed cohorts in the 10 strong merit aid states and 25 non-merit aid states; the 15 weak merit aid states and Alaska are excluded. Columns (1)-(4) and (5)-(8) report TWFE specifications with increasingly rich controls, as described in Table 4 (birth state and cohort fixed effects, demographic indicators, labor market measures, and linear state-by-cohort trends). Statistics in braces report the coefficient divided by the unexposed mean, representing the percent change in the outcome for the 10 strong merit aid states. The implied TOT effect is computed by dividing the ITT coefficient by 0.252, the share of students in strong merit states receiving merit aid in 2019-2020 (NASSGAP, 2020). Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.

*** p<0.01, ** p<0.05, * p<0.10

Table C10: Merit Program Effects on Family Formation

| | (1)<br>Ever<br>Married | (2)<br>Any<br>Children | (3)<br>Num of<br>Children |
|---|---|---|---|
| **A. Females & Males** | | | |
| Merit State | 0.0019 | 0.0079** | -0.0005 |
| | (0.0043) | (0.0031) | (0.0141) |
| Unexposed Mean | .6263 | .4872 | 1.8266 |
| Percent Change | {.3%} | {1.62%} | {-.03%} |
| TOT | [.0075] | [.0313] | [-.0019] |
| N | 2,885,108 | 2,885,108 | 1,193,578 |
| **B. Females** | | | |
| Merit State | -0.0014 | 0.0064* | 0.0008 |
| | (0.0039) | (0.0033) | (0.0135) |
| Unexposed Mean | .6593 | .5638 | 1.8482 |
| Percent Change | {-.22%} | {1.13%} | {.04%} |
| TOT | [-.0057] | [.0254] | [.0031] |
| N | 1,597,090 | 1,597,090 | 763,524 |
| **C. Males** | | | |
| Merit State | 0.0062 | 0.0090 | -0.0028 |
| | (0.0079) | (0.0058) | (0.0199) |
| Unexposed Mean | .5844 | .39 | 1.787 |
| Percent Change | {1.06%} | {2.31%} | {-.16%} |
| TOT | [.0245] | [.0357] | [-.0113] |
| N | 1,288,018 | 1,288,018 | 430,054 |
| Any Children | | | X |

*Notes*: This table reports ITT estimates of the impact of strong merit aid program exposure on family formation outcomes. The outcome in column (1) is an indicator for ever being married. The outcome in column (2) is an indicator for having any own children in the household. The outcome in column (3) is a discrete variable for the number of own children in the household, conditional on having at least one child, to estimate intensive margin effects. The analytic sample is described in Section 4, and estimates compare changes in outcomes between exposed and unexposed cohorts in the 10 strong merit aid states and 25 non-merit aid states, excluding the 15 weak merit aid states and Alaska. I use the main TWFE specification described in Section 5.1 and Table 3. Statistics in braces report the coefficient divided by the unexposed mean, representing the percent change in the outcome for the 10 strong merit aid states. The implied TOT effect is computed by dividing the ITT coefficient by 0.252, the share of students in strong merit states receiving merit aid in 2019-2020 (NASSGAP, 2020). Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table C11: Merit Program Effects with Family Formation Controls

| | (1) Employed | (2) Employed | (3) Earnings | (4) Earnings |
|---|---|---|---|---|
| **A. Females & Males** | | | | |
| Merit State | -0.0042** | -0.0037* | -333 | -297 |
| | (0.0019) | (0.0019) | (205) | (204) |
| Any Children | | -0.0653*** | | -3,991*** |
| | | (0.0021) | | (171) |
| Unexposed Mean | 0.9080 | 0.9080 | 31,445 | 31,445 |
| Percent Change | {-0.47%} | {-0.41%} | {-1.06%} | {-0.94%} |
| TOT | [-0.0168] | [-0.0147] | [-1,322] | [-1,178] |
| N | 2,885,108 | 2,885,108 | 2,583,974 | 2,583,974 |
| **B. Females** | | | | |
| Merit State | -0.0085*** | -0.0076*** | -501*** | -434** |
| | (0.0022) | (0.0023) | (181) | (185) |
| Any Children | | -0.1323*** | | -9,104*** |
| | | (0.0040) | | (170) |
| Unexposed Mean | 0.8710 | 0.8710 | 25,880 | 25,880 |
| Percent Change | {-0.97%} | {-0.88%} | {-1.93%} | {-1.68%} |
| TOT | [-0.0337] | [-0.0303] | [-1,986] | [-1,724] |
| N | 1,597,090 | 1,597,090 | 1,432,714 | 1,432,714 |
| **C. Males** | | | | |
| Merit State | 0.0013 | 0.0011 | -250 | -315 |
| | (0.0023) | (0.0023) | (362) | (352) |
| Any Children | | 0.0285*** | | 6,264*** |
| | | (0.0020) | | (161) |
| Unexposed Mean | 0.9548 | 0.9548 | 39,607 | 39,607 |
| Percent Change | {0.14%} | {0.11%} | {-0.63%} | {-0.80%} |
| TOT | [0.0052] | [0.0042] | [ -992] | [-1,251] |
| N | 1,288,018 | 1,288,018 | 1,151,993 | 1,151,993 |
| Any Children | | X | | X |
| Earnings <90p | | | X | X |

*Notes*: This table reports ITT estimates of the impact of strong merit aid program exposure on employment and earnings, including a control variable for having any own children in the household. Earnings are measured below the 90th percentile in 2019 dollars. Columns (1) and (3) use the main TWFE specification described in Section 5.1 and Table 3, while columns (2) and (4) additionally include the control for any children. The analytic sample is described in Section 4, and estimates compare changes in outcomes between exposed and unexposed cohorts in the 10 strong merit aid states and 25 non-merit aid states, excluding the 15 weak merit aid states and Alaska. Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** p<0.01, ** p<0.05, * p<0.10

Table C12: Merit Program Effects on College Majors and Occupations

| | (1) STEM Major | (2) Major Pred Earn | (3) STEM Occ | (4) Occ Pred Earn |
|---|---|---|---|---|
| **A. Females & Males** | | | | |
| Merit State | -0.0015 | -0.0025 | -0.0016 | -0.0032 |
| | (0.0032) | (0.0020) | (0.0038) | (0.0028) |
| Unexposed Mean | .2094 | | .1588 | |
| Percent Change | {-.71%} | | {-1%} | |
| TOT | [-.0059] | [-.0101] | [-.0063] | [-.0125] |
| N | 764,819 | 764,819 | 2,798,310 | 2,798,310 |
| **B. Females** | | | | |
| Merit State | 0.0019 | 0.0011 | -0.0005 | 0.0002 |
| | (0.0046) | (0.0040) | (0.0043) | (0.0042) |
| Unexposed Mean | .1387 | | .1606 | |
| Percent Change | {1.35%} | | {-.34%} | |
| TOT | [.0074] | [.0046] | [-.0021] | [.0006] |
| N | 441,398 | 441,398 | 1,532,446 | 1,532,446 |
| **C. Males** | | | | |
| Merit State | -0.0065 | -0.0076*** | -0.0032 | -0.0074** |
| | (0.0085) | (0.0018) | (0.0051) | (0.0033) |
| Unexposed Mean | .3018 | | .1566 | |
| Percent Change | {-2.14%} | | {-2.06%} | |
| TOT | [-.0257] | [-.03] | [-.0128] | [-.0294] |
| N | 323,421 | 323,421 | 1,265,864 | 1,265,864 |

*Notes*: This table reports ITT estimates of the impact of strong merit aid program exposure on college majors and occupations. STEM majors are classified following Sjoquist and Winters (2015a), and STEM occupations are classified following Langdon et al. (2011) and Vilorio (2014). The major and occupation predicted earnings variables are measured using the coefficients from regressions of log earnings on major and occupation fixed effects. The analytic sample is described in Section 4, and estimates compare changes in outcomes between exposed and unexposed cohorts in the 10 strong merit aid states and 25 non-merit aid states, excluding the 15 weak merit aid states and Alaska. I use the main TWFE specification described in Section 5.1 and Table 3. Statistics in braces report the coefficient divided by the unexposed mean, representing the percent change in the outcome for the 10 strong merit aid states. The implied TOT effect is computed by dividing the ITT coefficient by 0.252, the share of students in strong merit states receiving merit aid in 2019-2020 (NASSGAP, 2020). Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** p<0.01, ** p<0.05, * p<0.10

## Table C13: Merit Program Effects with Occupation Controls

| | (1) Employed | (2) Employed | (3) Employed | (4) Earnings | (5) Earnings | (6) Earnings |
|---|---|---|---|---|---|---|
| **A. Females & Males** | | | | | | |
| Merit State | -0.0027* | -0.0027* | -0.0022 | -282 | -266 | -229 |
| | (0.0014) | (0.0014) | (0.0015) | (175) | (191) | (146) |
| STEM Occ | | 0.0329*** | | | 10,703*** | |
| | | (0.0011) | | | (204) | |
| Unexposed Mean | 0.9378 | 0.9378 | 0.9378 | 32,809 | 32,809 | 32,809 |
| Percent Change | {-0.29%} | {-0.29%} | {-0.24%} | {-0.86%} | {-0.81%} | {-0.70%} |
| TOT | [-0.0109] | [-0.0107] | [-0.0088] | [-1,118] | [-1,057] | [-907] |
| N | 2,798,310 | 2,798,310 | 2,798,310 | 2,508,865 | 2,508,865 | 2,508,865 |
| **B. Females** | | | | | | |
| Merit State | -0.0073*** | -0.0073*** | -0.0072*** | -421** | -413** | -451*** |
| | (0.0022) | (0.0022) | (0.0023) | (193) | (197) | (134) |
| STEM Occ | | 0.0487*** | | | 9,446*** | |
| | | (0.0016) | | | (292) | |
| Unexposed Mean | 0.9118 | 0.9118 | 0.9118 | 27,387 | 27,387 | 27,387 |
| Percent Change | {-0.80%} | {-0.80%} | {-0.79%} | {-1.54%} | {-1.51%} | {-1.65%} |
| TOT | [-0.0289] | [-0.0288] | [-0.0285] | [-1,669] | [-1,641] | [-1,791] |
| N | 1,532,446 | 1,532,446 | 1,532,446 | 1,376,383 | 1,376,383 | 1,376,383 |
| **C. Males** | | | | | | |
| Merit State | 0.0028* | 0.0029* | 0.0034* | -193 | -148 | 9 |
| | (0.0016) | (0.0016) | (0.0017) | (290) | (311) | (262) |
| STEM Occ | | 0.0151*** | | | 11,497*** | |
| | | (0.0012) | | | (189) | |
| Unexposed Mean | 0.9698 | 0.9698 | 0.9698 | 40,438 | 40,438 | 40,438 |
| Percent Change | {0.29%} | {0.30%} | {0.35%} | {-0.48%} | {-0.37%} | {0.02%} |
| TOT | [0.0112] | [0.0114] | [0.0134] | [-764] | [-589] | [37] |
| N | 1,265,864 | 1,265,864 | 1,265,864 | 1,132,616 | 1,132,616 | 1,132,616 |
| Occ FE | | | X | | | X |
| Earnings <90p | | | | X | X | X |

*Notes*: This table reports ITT estimates of the impact of strong merit aid program exposure on employment and earnings, including occupation controls. Earnings are measured below the 90th percentile in 2019 dollars. Columns (1) and (4) show the main estimates, columns (2) and (5) add a STEM occupation indicator control, and columns (3) and (6) add an occupation fixed effect control. Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** p<0.01, ** p<0.05, * p<0.10

Table C14: Balance Tests Between Strong Merit and Non-Merit States for Females

| | (1) Non-Merit State Mean | (2) Strong Merit State Mean | (3) Difference | (4) P-Value |
|---|---|---|---|---|
| Female | 1.000 | 1.000 | 0.000 | . |
| Black | 0.103 | 0.204 | 0.101 | 0.000 |
| Hispanic | 0.100 | 0.058 | -0.042 | 0.000 |
| White | 0.743 | 0.706 | -0.037 | 0.000 |
| Asian | 0.025 | 0.008 | -0.017 | 0.000 |
| Other Race | 0.028 | 0.023 | -0.005 | 0.000 |
| Age | 28.517 | 28.322 | -0.194 | 0.000 |
| Strong Merit State | 0.000 | 0.633 | 0.633 | 0.000 |
| College 1+ Years | 0.902 | 0.893 | -0.009 | 0.000 |
| AA+ | 0.647 | 0.598 | -0.049 | 0.000 |
| BA+ | 0.509 | 0.452 | -0.057 | 0.000 |
| MA+ | 0.132 | 0.121 | -0.012 | 0.000 |
| STEM Major | 0.152 | 0.139 | -0.013 | 0.000 |
| STEM Occ | 0.155 | 0.160 | 0.005 | 0.000 |
| Ever Married | 0.545 | 0.573 | 0.028 | 0.000 |
| Age at 1st Marriage | 24.061 | 23.597 | -0.465 | 0.000 |
| Any Children | 0.453 | 0.505 | 0.052 | 0.000 |
| Num of Children | 0.835 | 0.924 | 0.089 | 0.000 |
| Employed (Earnings $>$ 0) | 0.875 | 0.860 | -0.015 | 0.000 |
| Earnings | 34,248 | 30,201 | -4,047 | 0.000 |
| Employed (Survey) | 0.951 | 0.940 | -0.011 | 0.000 |
| Labor Force | 0.840 | 0.829 | -0.011 | 0.000 |
| Weeks Worked | 39.639 | 38.705 | -0.935 | 0.000 |
| Hours per Week | 33.187 | 32.652 | -0.536 | 0.000 |
| Live in Birth State | 0.638 | 0.635 | -0.003 | 0.073 |
| N | 2,132,780 | 264,566 | 2,397,346 | |
| N States | 25 | 10 | 35 | |

*Notes*: Balance tests are conducted for females in the analytic sample described in Section 4. I test for differences in observable characteristics between non-merit and strong merit states using an indicator for ever being born in one of the 10 strong merit aid states. Column (1) reports the non-merit state mean, column (2) reports the strong merit state mean, column (3) reports the coefficient from a regression of the characteristic on the ever strong merit indicator, and column (4) reports the associated p-value. Earnings are measured in 2019 dollars. Statistics are computed using the person weight variable provided by the Census and ACS. Regressions use robust standard errors.

Table C15: Balance Tests for Changes in Observable Characteristics

| | (1) |
|---|---|
| Female | -0.0017 |
| | (0.0018) |
| Black | -0.0013 |
| | (0.0036) |
| Hispanic | -0.0044 |
| | (0.0102) |
| White | 0.0139 |
| | (0.0135) |
| Asian | -0.0068 |
| | (0.0049) |
| Other Race | -0.0015 |
| | (0.0010) |
| Age | -0.0099 |
| | (0.0151) |
| N | 2,885,108 |
| N States | 35 |

*Notes*: Balance tests for changes in observable characteristics are performed on the analytic sample described in Section 4. Estimates compare changes in observable characteristics before and after merit aid program adoption between the 10 strong merit aid states and 25 non-merit aid states, excluding the 15 weak merit aid states and Alaska. The specification is TWFE with birth state and birth cohort fixed effects. Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** p<0.01, ** p<0.05, * p<0.10

Table C16: Merit Program Effects on Labor Market Outcomes: Sample Including All High School Graduates

| | (1) Employed | (2) Employed | (3) Employed | (4) Earnings | (5) Earnings | (6) Earnings |
|---|---|---|---|---|---|---|
| **A. Females & Males** | | | | | | |
| Merit State | -0.0042** | 0.0061 | -0.0012 | -333 | -77 | -214 |
| | (0.0019) | (0.0041) | (0.0024) | (205) | (174) | (173) |
| Unexposed Mean | 0.9080 | 0.8344 | 0.8825 | 31,445 | 20,377 | 27,719 |
| Percent Change | {-0.47%} | {0.74%} | {-0.14%} | {-1.06%} | {-0.38%} | {-0.77%} |
| TOT | [-0.0168] | [0.0244] | [-0.0049] | [-1,322] | [-304] | [-850] |
| N | 2,885,108 | 1,055,859 | 3,940,967 | 2,583,974 | 946,854 | 3,524,392 |
| **B. Females** | | | | | | |
| Merit State | -0.0085*** | 0.0046 | -0.0051** | -501*** | -177 | -321** |
| | (0.0022) | (0.0044) | (0.0024) | (181) | (246) | (149) |
| Unexposed Mean | 0.8710 | 0.7553 | 0.8356 | 25,880 | 13,626 | 22,084 |
| Percent Change | {-0.97%} | {0.62%} | {-0.61%} | {-1.93%} | {-1.30%} | {-1.46%} |
| TOT | [-0.0337] | [0.0184] | [-0.0201] | [-1,986] | [-702] | [-1,275] |
| N | 1,597,090 | 478,454 | 2,075,544 | 1,432,714 | 431,649 | 1,860,786 |
| **C. Males** | | | | | | |
| Merit State | 0.0013 | 0.0072 | 0.0032 | -250 | 62 | -143 |
| | (0.0023) | (0.0075) | (0.0038) | (362) | (364) | (288) |
| Unexposed Mean | 0.9548 | 0.9032 | 0.9346 | 39,607 | 27,278 | 34,789 |
| Percent Change | {0.14%} | {0.80%} | {0.34%} | {-0.63%} | {0.23%} | {-0.41%} |
| TOT | [0.0052] | [0.0285] | [0.0127] | [-992] | [246] | [-567] |
| N | 1,288,018 | 577,405 | 1,865,423 | 1,151,993 | 516,489 | 1,666,280 |
| HS Grad | | X | X | | X | X |
| College Enroll | X | | X | X | | X |
| Earnings <90p | | | | X | X | X |

*Notes*: This table reports ITT estimates of the impact of strong merit aid program exposure on employment and earnings, using different sample restrictions for educational attainment. Earnings are measured below the 90th percentile in 2019 dollars. Columns (1) and (4) use the main analytic sample described in Section 4, columns (2) and (5) include only high school graduates, and columns (3) and (6) include both high school graduates and college attendees. I use the main TWFE specification described in Section 5.1 and Table 3. Estimates compare changes in outcomes between exposed and unexposed cohorts in the 10 strong merit aid states and 25 non-merit aid states, excluding the 15 weak merit aid states and Alaska. Statistics in braces report the coefficient divided by the unexposed mean, representing the percent change in the outcome for the 10 strong merit aid states. The implied TOT effect is computed by dividing the ITT coefficient by 0.252, the share of students in strong merit states receiving merit aid in 2019-2020 (NASSGAP, 2020). Regressions are weighted using the person weight variable, and robust standard errors are clustered at the birth state level.
*** p<0.01, ** p<0.05, * p<0.10