State Merit Aid and the Supply Curve of Higher Education

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Abstract

Financial aid is ubiquitous in higher education. A fundamental goal of giving financial aid to students is to lessen financial barriers to attending higher education. A consequence of increasing financial aid generosity is that higher education institutions may raise prices in response to higher demand for their services. Previous research largely focuses on estimating price responses to increases in financial aid generosity. In contrast, we exploit cross-state differences in the generosity of state merit aid programs to estimate the elasticity of supply of higher education to quantify institutions' short-run response to changes in these programs. Our approach has two main advantages: the elasticity of supply determines the price response, and it captures institutions' ability to respond to changes in financial aid policy by changing enrollments. We find that public four-year institutions have an estimated elasticity of supply of 2.1 and private non-profit four-year institutions have an estimated elasticity of 1.31. Based on these results, we conclude that the institutional response to these programs did not inhibit their intended effect of reducing the financial burden of higher education and increasing post-secondary enrollments.

Keywords: Higher education, Financial aid, Bennett Hypothesis

JEL Codes: I2, H2, H3

1 Introduction

Increases in the cost of attendance at American colleges and universities have outpaced the rate of inflation for many years. Average published tuition and fees at public four-year institutions doubled between academic years 1999-2000 and 2018-19 (Ma et al. 2019). Over the same period, financial aid to students has risen substantially as policymakers have become more concerned with the accessibility of higher education; federal and state governments spent $164 billion on financial aid (comprised of both grants and loans) in 2018-19. With such a substantial investment of public resources, it is important to evaluate the impact of these programs on the primary target of the programs — students.

One prominent type of financial aid has been state merit grant programs that provide financial aid to high achieving students who graduate from a high school in the state and chose to attend a higher educational institution in the state.[1] Financial need or (family) income is not considered for program eligibility. Recipients can use their award to pay for tuition and fees, with some grants allowing recipients to use their award to pay for additional expenses like room and board and textbooks. Given the structure of these programs, they can be viewed as essentially a subsidy to students attending an in-state school which leads to an increase in the demand for in-state higher education, through both an increase in the number of students choosing to continue schooling past high school and a decrease in the number of students choosing to attend an out-of-state higher education institution. As with any subsidy provided to the consumers of a good or service one question is how much of the subsidy accrues to the consumers and how much of the subsidy is captured by producer through higher prices?[2] This concern has resulted in a number of studies of financial aid programs focusing on the increase in tuition resulting from the introduction or expansion of these program (Black, Turner, and Denning 2023; Kramer, Ortagus, and Lacy 2018; Lucca, Nadauld, and Shen 2019; Turner 2017; Welch 2014).

1. These programs generally condition eligibility on having a high school grade point average (GPA) above a threshold, frequently 3.0, or scoring above a threshold on standardized tests. Many of these programs also require recipients to maintain a college GPA above a threshold to renew eligibility. All programs provide grants to students attending public in-state schools; some states also provide grants to students attending private non-profit schools in a state.

2. One expression of this concern in the case of state merit aid programs came from William Bennett, the Secretary of the Department of Education who claimed that "increases in financial aid in recent years have enabled colleges and universities blithely to raise their tuitions, confident that Federal loan subsidies would help cushion the increase" Bennett (1987).

Of course, any producer whose costs are an increasing function of the amount of output produced will respond to a shift out in demand by both increasing the amount of output produced as well as by increasing the price charged for the product, so evidence of an increase in the price charged by producers in response to a subsidy is consistent with the subsidy being a benefit to consumers in the market . The impact of the subsidy on consumers will be a function of both the change in the net price paid by consumer (the price received by producers minus the subsidy) and the quantity of the product consumed and ultimately whether consumers of producers receive the bulk of the benefits from the subsidy depends of the relative elasticity of demand and supply.

In order to capture the overall impact of state merit-based aid programs on students, in this paper we estimate simultaneous equation models of both enrollment and tuition at the institution level, where enrollment is a function of tuition, using data on state merit aid programs combined with institutional data on enrollment, tuition and other costs. We identify the short-run elasticity of supply by exploiting plausibly exogeneous cross-state variation in the existence and generosity of state merit grant programs, while other controlling for factors that affect enrollment at the institution, such as other sources of revenue (e.g., direct government appropriations and revenue from endowments). This model produces an estimate of the net-tuition elasticity of enrollment around the equilibrium level of enrollment, which allows us to assess the relative benefit of state merit aid programs accruing to students. As far as we are aware, this is the first paper to estimate the short-run elasticity of the supply response of higher education to changes in tuition leveraging the implementation of state merit grant programs in the 1990s and 2000s.

We believe there are several advantages to this approach. First, the implications of our results for students are easily interpretable. Estimated elasticities greater than one implies that institutions increased enrollments by proportionally more than tuition, indicating that students obtain a majority of the benefit from the subsidy. Elasticity estimates of less than one suggests that higher education institutions are able to capture a majority of the benefits from these programs. Second, our approach estimates the response of institutions to demand shocks rather than changes in market outcomes. We believe this approach is more informative with respect to institutions' strategic behavior. Third, our approach provides a direct estimate of the elasticity of supply for higher education institutions around the existing equilibrium, which could be used to assess the likely impact on students of other potential policies that provide direct aid to students.

Our paper contributes contribute to the literature on evaluating merit grant programs by using a more complete set of state merit grant programs in our analysis and by allowing the impact of merit grant programs to vary by the time since implementation of the program. In addition, we allow for the impact of a merit grant program to vary with its per-student generosity; accounting for differences in generosity is important as there is significant variation in spending levels across programs. Finally, we also study the supply response among public two-year institutions whereas previous studies focus on the public and private four-year sectors.

We find that public four-year and private not-for-profit four-year (hereafter referred to as private) institutions have elastic supply curves (around the pre-merit grant equilibria). Our baseline estimates are that public four-year institutions have an estimated elasticity of supply of 2.1 and private institutions have an estimated elasticity of supply of 1.31. Our baseline estimate is 1.36 for public two-year institutions, although point estimates for this sector vary significantly in robustness checks. We find suggestive evidence that some institutions within the private sector have inelastic supply curves although we lack power to precisely estimate heterogeneity in supply curves in most of the subsector analyses. Overall, our results show that four-year not-for-profit institutions do increase prices following increases in per-enrolled undergraduate merit grant expenditures, but first-time first-year resident enrollments increase by about 2.1% and 1.3% for each 1% increase in tuition at four-year public and not-for-profit private institutions, respectively. It does not appear that the supply response is inelastic, at least in the case of merit grant programs. We find that institutions did raise prices, on average, in response to the implementation of merit grant programs but they increased enrollments by proportionally more. We conclude that this elastic supply response demonstrates that most of the benefit from merit-grant aid programs accrues to the students.

The rest of the paper is as follows. In the next section we provide more background on state-based merit aid programs along with a review of the literature examining the impact of these programs. Section 3 presents a conceptual framework for the basis of our subsequent empirical analysis. In section 4 we discuss the data used in our estimation and our empirical specification. We present our results in section 5 and our conclusions in section 6.

2 Background & Literature Review

State-sponsored merit grant programs provide financial aid to high achieving students who graduate from a high school in the state and who attend an in-state higher education institution.[3] Some grant programs offer more generous awards for attending public in-state institutions than for attending private in-state institutions.[4] Policymakers often state that their primary goal of implementing a merit grant program is to improve their state's workforce (Groen 2011). Merit grants incentivize improved academic performance in secondary school by financially rewarding students for achieving a certain test score or GPA while also trying to retain the "best and brightest" students.

Arkansas was the first state to implement a merit grant in 1991. Initially it was not a very generous grant although program eligibility and generosity were both later expanded. Georgia is typically credited with pioneering merit grants with its Helping Outstanding Pupils Educationally (GAHOPE) scholarship which it enacted in 1993. The GAHOPE scholarship fully covers tuition and mandatory fees at public Georgia institutions for Georgia residents who graduate high school with at least a 3.0 GPA. The GAHOPE program began as a request by then-Governor Zell Miller to the state legislature to enact a state lottery whose proceeds would go towards education (Dynarski 2000). The GAHOPE scholarship was a pathbreaking financial aid program and lead a number of other states to implement similar programs.

Most merit grant programs were implemented in the mid-1990s or 2000s (Sjoquist and Winters 2015). A crucial detail for the validity of our empirical strategy is that merit grants are funded by state lotteries or tobacco settlement funds (Heller and Marin 2004) rather than by states' general revenues. If merit grant programs were funded by state appropriations, then these financial aid dollars may represent a shift of state funding from institutions to students. For public institutions, this would constitute a simultaneous inwards shift of supply and an outward shift in demand. However, merit grant programs and state appropriations to in-state institutions of higher education are funded from different sources. Some merit grant programs can be quite generous. The GAHOPE scholarship, for example, covers up to 100% of tuition for recipients. State-specific average per-student merit grant expenditures are shown in Appendix Table A1.

3. Some states do have reciprocity agreements with other states allowing its residents to use their merit aid at out of state schools. We do not account for reciprocity agreements in our analysis.

4. This could cause merit grant expenditures to be weakly correlated with demand for private institutions. We investigate this concern in the Results section.

Previous empirical work has studied the institutional responses to changes in financial aid policy in a wide range of settings. We primarily focus our review on studies of grant programs (as opposed to loans) since we focus on grant programs. Turner (2017) studies the incidence of the Pell Grant using regression discontinuity and kink designs with student-level data and finds that an increase in Pell Grants is associated with between an 11 and 20% increase in posted tuition rates at colleges and Universities. Long (2004) estimates the impact of the GAHOPE scholarship on tuition by comparing changes in prices at Georgia institutions to changes in prices at other southeastern institutions using a difference-in-differences design. She finds that, on average, posted tuition rates at public institutions did not increase, although room and board rates did rise, while private Georgia institutions raised tuition by 3.2% and decreased financial aid offers. Private institutions with many GAHOPE recipients raised net prices by 30 cents per GAHOPE dollar. Welch (2014) and Kramer, Ortagus, and Lacy (2018) both study the effects of merit grant programs on prices at the national level using difference-in-differences designs. Welch (2014) finds no evidence that in-state public or private institutions raised their tuition in response to the implementation of a merit grant program.[5] Kramer, Ortagus, and Lacy (2018) find differential responses for in-state tuition across tuition-setting authority. Public institutions that can set their tuition rates and are in states that implement increase tuition by 5 to 6% following implementation compared to public institutions in merit-adopting states that are not allowed to set their tuition. These results suggests that the implementation of state merit grant programs lead to small changes in tuition. This weak price response could be because merit grant programs constitute a weak shock to student demand for higher education or because the eligible institutions have elastic supply curves; they expand to meet greater demand without substantially raising prices.

While most studies focus on institutional price responses, some studies estimate changes in enrollment as well. Lucca, Nadauld, and Shen (2019) study the institutional response to increases in the maximum Pell Grant award and federal loan amounts. They estimate that a $100 increase in the average Pell Grant per full-time-equivalent undergraduate results in between $55 and $65 increase in listed tuition and a 1.5% increase in enrollment. These estimates produce an elasticity of

5. Kramer, Ortagus, and Lacy (2018) study the merit grant programs of Georgia, Florida, Kentucky, Louisiana, New Mexico, Nevada, South Carolina, Tennessee, and West Virginia. Welch (2014) studies the same set of programs as Kramer, Ortagus, and Lacy (2018) with the addition of Michigan's merit grant program.

supply of higher education of 1.68.[6] They estimate statistically insignificant increases in enrollment of 0.6% and -0.02% associated with $100 increases in average federal subsidized and unsubsidized loans per full-time-equivalent undergraduate. While the authors view their results as evidence that institutions are able to capture a significant part of the increase in financial aid by raising prices, the change in supply suggests student are able to capture most of the benefit from the increase in Pell Grants.[7]

In a series of papers, Dennis Epple and co-authors derive structural models for how higher educational institutions respond to shocks to demand and supply and use these models to evaluate the supply response to several policy changes. In Epple, Romano, and Sieg (2006), the authors find that increasing public financial aid to students with low incomes induces poorer students to attend higher quality institutions. Epple et al. (2017) studies the impact of an increase in the federal grant from $6,000 to $8,000 and finds that enrollment increases by 6%, with most the new enrollments occurring at public institutions. Students at public colleges that qualified for the the maximum aid award saved the full $2,000 and the average cost savings of students who attend public institutions regardless of the policy change saved an average of $203. At private institutions, average federal aid packages increased by $880 but the average price paid by students actually increased by $370. Private institutions increased instructional expenditures and raised tuition to pay for it. For students who do not change the institution they attend due to the change in aid, the increased federal aid is fully offset by higher prices.

We contribute to the literature by taking a new approach to evaluating the institutional response to financial aid programs. Studies in the literature focus on estimating price changes and, more recently, changes in enrollment. Motivating this approach is the belief that institutions face short-run frictions to expanding enrollment capacity which generates an inelastic supply response. We estimate the elasticity of supply of higher education without imposing any assumptions about the

6. The authors note that the data on institutional grants are noisy and estimated pass-through rates of Pell Grant dollars to net prices are not fully robust to the inclusion of controls. We take their point estimates at face value for the purpose of this back-of-the-envelope calculation. Then, their results suggest that $\frac{1}{Enrolls}\frac{dEnrolls}{d\Delta Pell} = 0.015/100$ and $\frac{dNetPrice}{d\Delta Pell} = 0.954$ (0.577 + 0.377, Table 10). Then, using $10,691 as the average net price from their sample (Table 3), $\epsilon = \frac{dEnrolls}{dNetPrice} \cdot \frac{NetPrice}{Enrolls} = \frac{0.015/100}{0.954} \cdot 10,691 = 1.68$. While estimating ϵ in this manner likely suffers from simultaneity bias that arises from tuition and enrollments co-moving, this calculation highlights how estimating ϵ versus estimating changes in tuition and enrollments separately matters for understanding the institutional response to changes in demand.

7. There have been several other studies that have focused on increases in aid or loans to graduate students or increases in the generosity of the GI Bill and have found null and even negative effects of financial aid on enrollments (Baird et al. 2022; Black, Turner, and Denning 2023).

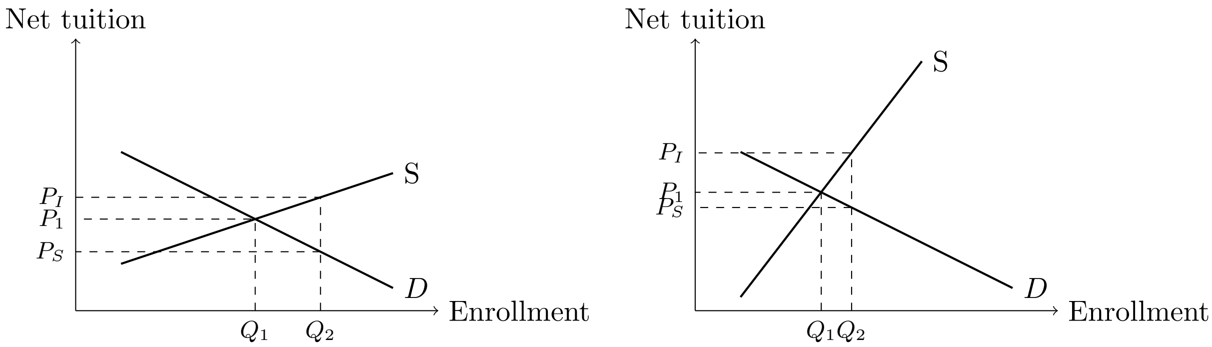
supply response to an increase in merit aid. We motivate our approach with a straightforward market-based framework of undergraduate higher education while acknowledging the differences between this market and more traditional product markets. We also contribute to the literature evaluating merit grant programs. We expand the set of merit grant programs that has been studied beyond the quite generous and widely available programs. We also allow for the effect of merit grant programs to vary by their per-student generosity whereas previous studies impose the assumption of homogeneous treatment effects of merit grant programs exposure.

3 Conceptual Framework

A large number of previous studies in the literature frame their work as empirically testing whether aid-eligible institutions raise their prices in response to the implementation of financial aid programs. To be sure, understanding how institutional tuition responds to changes in financial aid policy is important for quantifying how financial aid affects student welfare and access to higher education. However, quantifying tuition increases in response to merit aid program implementation by themselves are not sufficient to fully understand the impact that changes in financial aid have on students— a full accounting of the institutional supply response must consider how enrollments change as well. Consider Figure 1 below which shows the demand and supply curved for the market for undergraduate higher education. In this market students have downward sloping demand curves while higher education institutions have upward sloping supply curves. In Panel A, institutions have relatively elastic supply curves while in Panel B institutions have relatively inelastic supply curves.[8] The original equilibrium price and quantity are given by P_1 and Q_1, respectively. Now consider a subsidy of W provided to students. In the new equilibrium institutions receive P_I, while students pay the price P_S, with $W = P_I - P_S$, and the new equilibrium quantity is Q_2. Panel A shows the standard results that in markets where supply is elastic most of the benefits accrue to students—there is a relatively large decrease in the tuition students pay, a relatively large increase in the equilibrium quantity and a relatively small increase in the tuition institutions receive. In Panel B where supply is inelastic the opposite is true—the price students pay fall by a relatively

8. With the exception of the cases where institutions have perfectly elastic (i.e., horizontal) supply curves or face economies of scale in providing undergraduate education. Institutions that offer education virtually to a large proportion of their students may likely face economies of scale. The institutions we study, which primarily offer in-person education to residential students, more likely face increasing marginal costs.

small amount, there is a relatively small increase in output, there is a relatively large increase in the tuition institutions receive, and most of the benefits accrue to insitutions. In either case, this framework makes clear that to assess the benefits received by students from a tuition subsidy requires simultaneously estimating both the tuition and quantity response—that is estimating the own-price supply response.[9]



Panel A: Elastic supply

Panel B: Inelastic supply

Figure 1: Impact of a Financial Aid Expansion on Tuition and Enrollments
Note: P_I and P_S represent the prices that institutions receive and students pay, respectively.

This market-based approach is useful for studying higher educational responses to changes in demand, but it abstracts from two important aspects of the higher education market. First, the institutions we study here (public and private not-for-profit institutions) are likely not setting prices to maximize profits.[10] Second, this is a matching market which clears through admissions policies, not through prices that equate seats demanded to seats supplied. We are agnostic on the nature of intuitions' objective functions. The firms in our sample may not be maximizing profits but the goal of our empirical strategy is to estimate the supply response, so we do not need to take a stance on the exact nature of institutions' objective function. While these institutions may not be setting the profit-maximizing price, we believe that it is costly to maximize whatever their objectives are

9. The own price elasticity of demand also determines the change in equilibrium. In our setting, we are holding demand constant and using the policy change as plausibly exogenous changes in net price to estimate the supply response. As such, we cannot identify the own price elasticity of demand.

10. Structural models of the higher education market have not modeled institutions as profit maximizers. Epple et al. (2017) and Epple et al. (2019) model private institutions as quality maximizers where quality is a function of student body ability and educational expenditures and public institutions as maximizing the welfare of its in-state students. Fu (2014) models private institutions as maximizing a weighted average of student ability and net tuition revenues. Public institutions also maximize a weighted average of student ability and net tuition revenues, but with different weights placed on the net tuition and abilities of in-state and out-of-state students. Blair and Smetters (2021) define the objective function of elite institutions as a weighted average of net tuition revenues and their relative acceptance rate (relative to their peer institutions).

and they are sensitive to changes in their revenues, of which tuition is a major component, and changes in the costs of production output. With regards to admissions policies, one can think of the relevant demand as the demand from admitted students. Selective institutions may respond to an increase in demand by decreasing admissions rates (or increasing list price) without increasing enrollment, however the institutions in our sample are largely not selective.

4 Data

The primary data source for our analysis is the Integrated Postsecondary Education Data System (IPEDS) data set. Title IV institutions (institutions where students are eligible to receive federal financial aid) are required to complete the various IPEDS surveys annually, while non-Title IV institutions are not required to report to their data to IPEDS (although some do). Since states merit grant awards can only be used at Title IV institutions, IPEDS' sample coverage is not a limitation. IPEDS collects a wide range of data from participating postsecondary institutions. We assemble the IPEDS data into a panel of institutions' attributes, cost of attendance, enrollments, and revenue sources.[11] The panel spans the academic years beginning in the fall of 1986 through 2018. Table 1 presents summary statistics of IPEDS variables for the analytic sample. List price (tuition plus mandatory fees) is significantly higher at private institutions. Federal per-student revenues at public and private institutions are nearly the same. Public institutions rely more on revenues from state and local governments whereas private institutions rely more on revenues from private sources. We measure the enrollment of first- time first-year resident students who graduated from high school in the past 12 months. Institutions mandatorily report these data in even-numbered years (resident enrollments were not surveyed in 1990). About a third of institutions voluntarily report resident enrollment data in odd-numbered years. We restrict our sample to the even-numbered years in which IPEDS mandates institutions report enrollments by state of residence to avoid potential issues of endogenous reporting. We end our sample window at the 2018-19 academic year because it is the last year before the 2020 COVID pandemic that institutions mandatorily report enrollments by state of residence.[12]

11. We use the Urban Institute's EducationData application programming interface (API) to obtain the IPEDS data.

12. Institutions in IPEDS that have multiple campuses sometimes report financial data for the entire system under the flagship campus. Other multi-campus institutions report financial data separately for each campus. How institu-

We limit the sample to institutions in the 50 American states that are active and open to the public. We further limit the analytic sample to school-year observations that are not missing data on cost of attendance, enrollments, parent-child linkages, or institutional sector. Lastly, we limit the sample to four-year public, private four-year not-for-profit, and two-year public institutions. The result of these selection criteria is a panel containing 574 public four-year institutions, 1,013 private not-for-profit institutions, and 929 public two-year institutions in the analytic sample.

Table 1: Summary statistics for institutions by institutional sector

Institutional sector	Variable	Mean	SD
Public 4-year institutions	In-state Tuition + Mandatory Fees	3913.45	3357.68
	Average institutional grant	816.22	1252.61
	First-time first-year resident enrollees	1207.01	1154.46
	Per-student revenue from federal govt	2533.65	3708.91
	Per-student revenue from state and local govt	5622.92	7600.64
	Per-student revenue from private sources	527.57	1077.35
	Average Pell grant award	783.35	778.10
	Number unique instutions: 574		
Private institutions	Tuition + Mandatory Fees	14004.41	11149.03
	Average institutional grant	5453.77	8440.21
	First-time first-year resident enrollees	203.4	219.69
	Per-student revenue from federal govt	2624.73	15301.15
	Per-student revenue from state and local govt	511.91	1500.40
	Per-student revenue from private sources	4146.05	16722.87
	Average Pell grant award	811.08	1031.24
	Number unique instutions: 1013		
Public 2-year institutions	In-state Tuition + Mandatory Fees	1981.12	1710.59
	Average institutional grant	173.12	350.90
	First-time first-year resident enrollees	584.71	657.12
	Per-student revenue from federal govt	1465.27	1284.85
	Per-student revenue from state and local govt	3825.64	3497.92
	Per-student revenue from private sources	92.77	261.36
	Average Pell grant award	935.03	885.46
	Number unique instutions: 929		

Note: All dollar-valued variables are real 2016 dollars.

We obtain state-year counts of undergraduate enrollment and the number of 18 through 24 year

tions report finance data to IPEDS data depends on how the institution is registered with the federal Department of Education. This data reporting pattern is called "parent-child reporting" (PCR). We allocate finance variables by each campus' percentage of total system enrollment to account for PCR and make reported finance data comparable across institutions. A full explanation of how we handle PCR can be found in the Appendix and the topic is covered extensively by Jaquette and Parra (2014). While worth acknowledging, PCR is not overly prevalent in the sample. Only 2.8% of institution-year observations in the sample are flagged as parent institutions and 7% as children.

olds from the October Current Population Survey (CPS) and and state-year unemployment rates from the Bureau of Labor Statistics. We obtain data on annual state merit grant expenditures from the National Association of State Student Grant & Aid's (NASSGAP) Annual Surveys Reports beginning in 1998. We define merit grant expenditures as expenditures on a grant with eligibility only based on merit. We identify program implementation dates using Table 1 from Sjoquist and Winters (2015). Data on annual state merit grant expenditures in years prior to 1998 were sourced from state government reports.[13] Data on the Georgia HOPE grant for 1994 — 1997 reflect state appropriations, not expenditures, and were provided to the authors by the Georgia Governor's Office of Planning and Budget. We linearly interpolate expenditures for Utah in 2002 and 2003 because we could not find published expenditures for these years.[14] Annual state merit grant expenditures per enrolled undergraduate student, the independent variable of interest, are calculated by dividing expenditure data from NASSGAP by the estimated number of undergraduates in a state-year from the CPS. We use undergraduate counts from the CPS rather than IPEDS because this yields more reasonable per-student expenditure levels. Enrollment figures in IPEDS do not reflect the full student body (at the state level) since non-Title IV institutions are not required to report their data. Results are qualitatively unchanged when using data on undergraduate counts from IPEDS. Implementation dates and average per- student merit grant expenditures are presented in Appendix Table A1. All dollar-valued variables are deflated to real 2016 dollars using the Consumer Price Index. Some states are reported in the NASSGAP data as spending money on a merit grant for only a one- or two-year spell. Some of these states fund a more substantial, merit grant program in subsequent years. We define a state as having a merit grant in place in a given year if it is reported as spending any money on a merit grant in at least three out of five years on a rolling basis. This rule helps more accurately define which states are considered to have implemented a merit grant program and when a state implements a merit grant program. This eliminates 18 instances of a state having non-zero annual merit grant expenditures and shifts five states (California, Connecticut, Kansas, Maine, and Virginia) from being ever-treated to never-treated.[15] Our results are robust

13. These reports are linked in the publicly available data for this project.

14. In results not shown in the paper, we confirm that our results are unchanged when we zero out these pre-1998 merit grant expenditures. Our results are also qualitatively unchanged when we exclude years before 1990 from the sample.

15. The 13 never-adopting states are Arizona, California, Connecticut, Hawaii, Kansas, Maine, Minnesota, Nebraska, New Hampshire, Oregon, Rhode Island, Virginia, and Wyoming.

to estimating the model with or without using this rule for defining merit grant implementation. There are 37 states that ever spend money on a merit grant and 19 of those states continuously fund their merit grant after implementation.[16]

Previous studies categorize merit grant programs as "strong" or "weak" based on program participation rates and levels of per-student expenditures (Kramer, Ortagus, and Lacy 2018; Sjoquist and Winters 2015; Welch 2014). Strong merit grants have high program participation rates and substantial per-student expenditures. For example, the New Mexico Lottery Success Scholarship, which Sjoquist and Winters (2015) classify as a strong program, had per full-time-equivalent (FTE) student expenditure of \$494.67 and 20.71% of FTE students received an award in 2010 (see Table 1 from Sjoquist and Winters (2015)). Oklahoma's PROMISE scholarship is classified as weak for its 11.89% participation rate and \$58.33 average award per FTE student. Alaska and Arkansas have average expenditures more similar to the strong merit grant programs but they are classified as weak programs because they both have participation rates below 5% (Sjoquist and Winters 2015). A merit grant program could have low per-student expenditures because the aid packages are small on average or because they offer generous awards to only a small subset of students. South Carolina has the highest average per-student expenditures at \$828.78. The average per-student merit grant award conditional on one being in place is \$221.74.

5 Empirical Strategy

The parameter of interest is the short-run elasticity of supply of higher education, which we designate as ϵ. In our analysis short run to refers to the academic year immediately following the disbursement of funds. For example, we estimate the impact of merit grant funds disbursed in 2016 on tuition and enrollments in the 2016-17 academic year. We measure price as tuition plus mandatory fees minus the average institutional aid award, henceforth the net price P (What we call P_I in Figure 1). We measure quantity, Q, as the number of first-year, first-time in-state resident

16. Some states do implement a merit grant program that is later defunded. Presumably, the defunding of a merit grant program generates a negative shock to demand for higher education which can be used to identify the elasticity of supply. One may be concerned that the decision to defund a merit grant program is endogenous. Michigan is one such state that defunds their merit grant program. This occurred because policymakers funded the program with a tobacco settlement fund which was exhausted after four years of providing the grant. In this case, defunding is likely exogenous, but other states may decide to re-allocate lottery or tax revenues which may be endogenous to tuition or enrollments.

enrollments at an institution. We believe this is the appropriate measure since state merit grant awards are typically only available to resident students attending in-state institutions. If net price and (resident) enrollments are determined independently, conditional on observable factors, then ϵ can be estimated with ordinary least squares (OLS) in the model

$$ln(Q_{ist}) = \epsilon ln(P_{ist}) + \delta X_{ist} + \gamma S_{st} + \mu_s + \tau_t + \varepsilon_{ist}. \tag{1}$$

Estimates of ϵ from equation 1 are biased if price and quantity are simultaneously determined which we believe to be the case. Consider the following structural demand and supply equations:

$$ln(Q^d_{ist}) = \beta_1 ln(P_{ist}) + \delta_1 X_{ist} + \gamma_1 S_{st} + \mu_s + \tau_t + v_{ist} \tag{2}$$

$$ln(P_{ist}) = \beta_2 ln(Q^s_{ist}) + \delta_2 X_{ist} + \gamma_2 S_{st} + \mu_s + \tau_t + u_{ist} \tag{3}$$

where equation 2 specifies the natural logarithm of the quantity of seats demanded at institution i in state s and year t as a function of the natural logarithm of average net price, a vector of institution- level characteristics X_{ist}, state-year level variables S_{st}, and state and year fixed effects, μ_s and τ_t respectively. Equation 3 is the corresponding supply equation which specifies the natural logarithm of the price set by institutions as a function of the natural log of the quantity supplied and observable characteristics and X_{ist} and S_{st} and state and year fixed effects. Conditional on institutions' admissions policies, the market for undergraduate higher education in year t is in equilibrium when they charge prices P_{ist} such that $Q^s_{ist} = Q^d_{ist}$ for all instititutions i. The coefficient of interest is β_2 which is the inverse of ϵ.[17] A full set of results from estimating equations 2 and 3 are shown in Appendix Table A4. The simultaneity bias that exists in equation 1 arises in equations 2 and 3 when P_{ist} and v_{ist} are correlated. To overcome this bias, we estimate equations 2 and 3 using two-stage least squares (TSLS). The demand equation, equation 2, is the first stage equation and the supply equation is the second stage equation. We use per-student merit grant expenditures as an instrumental variable (IV) for the natural logarithm of price in the first-stage demand equation.

17. In results not reported here, we tried estimating the model with the P and Q variables flipped so that ϵ is the coefficient of interest in the second stage equation rather than its inverse. Per student merit grant expenditures proved to be a weak instrument for resident enrollments in the first stage. Conceptually, it also makes more sense to use the average subsidy amount as an instrument for average prices.

We report results from estimating the first stage in Appendix Table A4.

When estimating the system for public institutions, X_{ist} contains per-student federal, state, and local government revenues sent to an institution and the average Pell grant paid to students at the institution i in state s and year t. When estimating the system for private institutions, we replace per-student state and local revenues with per-student private revenues in X_{ist} since private institutions rely more heavily on private revenues. S_{st} contains the number of 18-24-year olds in state s and year t from the CPS and the unemployment rate in that state and year. We include these control variables to hold fixed other factors that may shift the supply or demand curves. We cluster standard errors at the state-year level because this is level at which treatment is assigned.[18] State policymakers set state budgets annually and thus state merit grant expenditures are determined annually. After estimating both equations in the system we present the estimates of the coefficient of interest, $\hat{\epsilon} = \frac{1}{\hat{\beta}_2}$. Standard errors of ϵ are calculated using the delta method. Appendix Table A5 presents point estimates and standard errors for the full set of independent variables from equation 3. Following the literature, we estimate equations 2 and 3 separately for public two-year, public four-year, and private institutions.

We do not impose a monotonicity assumption on the effect of merit grant programs on affected institutions and therefore our estimates are not interpreted as local. The imposition of a merit grant program may decrease demand for some merit-eligible institutions and increase demand for others. For example, a merit grant program whose awards do not vary by the institution that recipients attend may cause student demand for regional public institutions to decrease and increase for flagship public institutions. Since our measure of merit grant exposure only varies at the state-year level and not by institution-year, we cannot explore institution-specific demand shocks. Rather, we measure average demand shocks across merit-eligible institutions within the state-year.

Our conceptual framework is to use state merit grant programs as plausibly exogenous shocks to demand for in-state higher education across states with which we can estimate the slope of the supply curve of higher education. The internal validity of our approach relies on per-student merit grant program expenditures satisfying the assumptions for a valid instrument. Merit grant expenditures must be correlated with demand for higher education, Q_d. Program implementation

18. Abadie et al. (2023) demonstrate that clustering standard errors at the level of treatment assignment can be too conservative. Implementing their proposed estimator requires variation within cluster (here, the state-year) in treatment which does not exist in our setting.

as well as the intensity of merit grant expenditures needs to be uncorrelated with the structural errors v and u. This assumption is critical as it is imposing that, conditional on covariates, we are holding the supply curve fixed. And finally, merit grant programs must influence net prices only through their impact on student demand for higher education. Under this assumption, institutions respond to the change in student demand and not the merit grant programs themselves.

The first identifying assumption can be tested by looking at the results of the first stage regression. We report Olea and Pflueger (2013) F-tests for weak instruments in all specifications. Their F-test is equivalent to the Kleibergen-Paap F-test in our setting with one endogenous variable and one instrument. We also report adjusted standard errors using the tF procedure prescribed by Lee et al. (2022) which adjusts standard errors in the structural equation for the strength of the instrument in the first stage. We estimate alternate specifications with different parameterizations of merit grant program exposure to test the robustness of the assumption that the level of merit grant expenditures is conditionally independent. While we cannot directly prove that the exclusion restriction holds, we argue that it is not an overly strong assumption in this case. The main feature of these merit grant programs is that they reduce the out-of-pocket cost of attending grant-eligible institutions for recipients. It is common for merit grant programs to require recipients to maintain a GPA above a threshold or to take a minimum number of credit hours per semester (Scott-Clayton 2011). However, we are not measuring the impact of these requirements since we are measuring enrollments as the number of first-time first-year resident enrollments. Beyond specifying which institutions that recipients can use their merit grant award at and which charges the grant can be used towards, these programs do not impose any restrictions on the institutions that might change their behavior. One possible violation of the exclusion restriction is that merit grant awards are typically disbursed through the institution that recipients attend. Institutions can therefore identify which students are merit grant recipients and potentially use this information to price discriminate. A primary threat to identification is non-random adoption of state merit grant programs. We estimate event study models to test whether institutions in adopting and non-adopting states have different trends in net tuition and resident enrollments. In particular, we estimate

$$Y_{ist} = \sum_{\substack{x \neq -1 \\ x=-4}}^{x=19} \tau_x D_{st} 1\left[e_{sx} = x\right] + \theta X_{ist} + \pi S_{st} + \alpha_s + \eta_t + u_{ist} \tag{4}$$

where τ is a vector of event time coefficients, D_{st} is an indicator variable for whether state s had an active merit grant in year t, and e_{sx} is the event time variable. Since IPEDS only requires institutions to report resident enrollment data in even years, we bin event time into two year bins. If we did not use two year bins, we would not observe resident enrollments for many institutions in states that implemented a merit grant program in an odd year. The year before merit grant implementation is omitted and is the reference year. X and S are the institution and state covariates as before and α and η are vectors of state and year fixed effects respectively. Since states implemented their merit grant programs at different times, bias resulting from estimating a generalized difference-in-differences model with two-way fixed effects is a concern (Goodman-Bacon 2021). In addition to estimating the event study model with two-way fixed effects, we also implement the imputation estimator and corresponding tests of the identifying assumptions developed in Borusyak, Jaravel, and Spiess (2024).

Another threat to identification is the possibility that state merit grant programs change the composition of student bodies at eligible institutions. Different students pay different prices, so this could cause average net tuition to change. If this were the case, our estimates will capture the joint effects of increases in demand due to the grant and the changes in prices due to the grant's impact on student body composition. To empirically assess the degree to which state merit grant programs affect the racial and gender composition of student bodies at merit-eligible institutions We estimate

$$Y_{ist} = \beta D_{st} + \theta X_{ist} + \pi S_{st} + \alpha_s + \eta_t + u_{ist} \tag{5}$$

where D_{st}, X_{ist}, S_{st}, α_s, and η_t are defined as before in equations 3 and 4. Here, Y is one of the percent of the undergraduate student body at institution i in state s and year t that is Asian, Black, White, or female. For this robustness check, we use IPEDS data on total annual undergraduate enrollments which are reported annually, unlike resident enrollments.

There are two other threats to identification that are difficult to test using IPEDS data. The first is that the implementation of merit grant programs impose administrative costs on eligible institutions, thus simultaneously shifting the supply curve. IPEDS collects data on operating expenses associated with admissions and reports these expenditures combined with other student services expenditures like intramural sporting activities. However, institutional details support the idea that the implementation of merit grants did not impose significant costs on institutions. First, institutions already had systems in place to process financial aid awards before merit grants existed. Federal programs like the Pell Grant and student loans which predate merit grant programs are substantially larger in terms of dollars disbursed than merit grant programs. Second, these costs do not comprise a large portion of institutions' total costs. Student service salaries make up 3.4% and 5.7% of public four-year and private four-year institutions total costs, on average (authors' calculations from IPEDS data).

The other concern is that institutions may respond to a change in demand by changing their admissions policies rather than the number of students they admit. Institutions raising their admissions standards in response to higher demand, keeping their student body size constant, would cause us to mismeasure their response. Institutions may do this because they have high marginal costs, admissions standards are a signal of institutional quality, popular college rankings use admissions rates as an input, or some combination of these. IPEDS begins collecting data on admissions rates in the 2001-02 academic year. As Table A1 shows, most states implemented their merit grant program prior to 2001. Estimates of the impact of merit grant implementation on admissions rates at merit-eligible institutions from a difference-in-differences model would be identified only be a handful of post-2001 late adopters. We believe that it would be a mistake to interpret results from regressions estimated using IPEDS admissions data as representative of eligible institutions' admissions response to merit grant program implementation. The institutions in our sample do not have highly competitive admissions standards. There is one public four-year and 23 private four-year institutions with a within-institution average admissions rate below 20% in our sample. If institutions did respond to higher demand generated by merit grant programs by raising their admissions standards, this response would cause our estimates of ϵ to be attenuated relative to the counterfactual where institutions do not change their admissions policies in response to changes in demand. With these threats to identification in mind, we move to presenting empirical tests of the identifying assumptions and our main results.

Also crucial for our approach is that we hold the supply curve fixed while demand is changing. Other factors that may shift the supply curve are technological changes, changes in input costs, government regulations, and changes in expectations. The shift to offering courses virtually represents both a technological change and a decrease in the costs of providing instruction. We do not believe online courses shift the supply curve given that our sample ends at the 2018-19 academic year, before the COVID pandemic induced many institutions to increase their virtual course offerings, and because we are studying residential institutions that primarily offer in-person instruction. Many institutions have shifted their faculty composition towards more contingent faculty (Hemelt et al. 2021) to reduce instructional costs. We control for total instructional expenditures in our analysis to account for changing faculty composition. We also control for the state unemployment rate to control for variation in non-instructional labor costs. We are focusing on government regulation in the form of subsidies to students but other policy changes may be occurring during our sample window. We control for per-student Pell Grant expenditures in our analysis as the Pell Grant is the largest grant program by dollars spent. Finally, we are not aware of events during our sample window that changed institutions' expectations.

6 Results

6.1 Evaluating Identifying Assumptions

We start by showing results of empirical tests of the identifying assumptions. Since our first stage equation is a difference-in-differences design, we empirically test the parallel trends by estimating equation **??**. In Figure 2 we plot the estimates of the event time coefficients from the generalized difference-in-differences model in equation **??** and from Borusyak, Jaravel, and Spiess (2024) separately for four-year public and four-year not-for-profit private schools.[19] Point estimates of pre-treatment differences in net tuition and resident enrollments at public and private four-year institutions are all statistically insignificant, supporting the parallel trends assumption. However, estimates are also insignificant in post treatment periods, suggesting that merit grant programs did not increase resident enrollments.[20]

19. The estimates and their corresponding standard errors illustrated in Figure 2 may be found in appendix Table A4.

20. Readers may notice that estimates on lagged periods for enrollments in panels C and D vary period to period. This occurs because we only observe resident enrollments in even years.

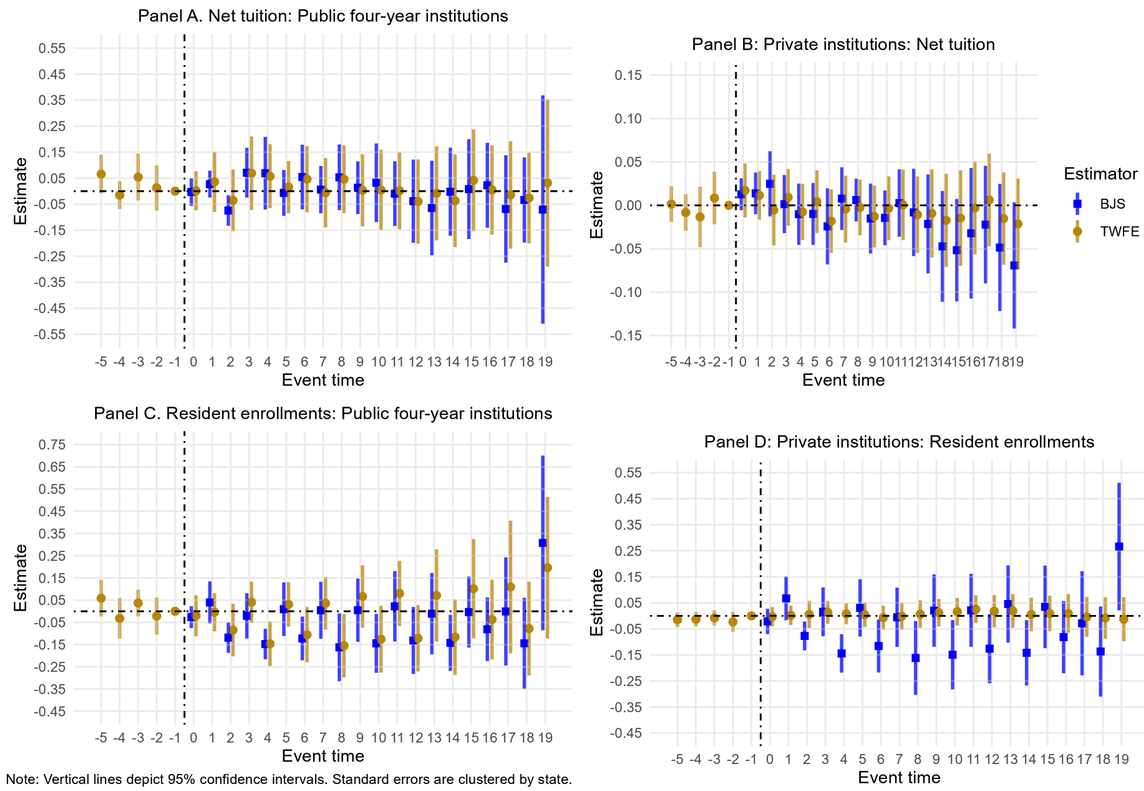


Figure 2: Tests of parallel trends.
Note: TWFE is short for two-way fixed effects and BJS references Borusyak, Jaravel, and Spiess (2024).

Also following Borusyak, Jaravel, and Spiess (2024), we impute counterfactual enrollment and net tuition and plot trends in potential outcomes for treated and untreated units in Figure 3. While this exercise is not a statistical test to evaluate the parallel trends assumption, plotting the trends in observed and counterfactual outcomes among treated and untreated institutions does create a useful visual aid for assessing the validity of the parallel trends assumption. Figure 3 plots the outcomes of institutions in merit-adopting states in red and the outcomes of institutions in non-adopting states in black. The solid red and black lines plot the observed outcomes of institutions in merit-adopting and non-adopting states, respectively. The dashed red and black lines plot the predicted untreated outcomes of merit-exposed and untreated institutions, respectively. The parallel trends assumption requires that the two solid lines follow the same trend prior to treatment and the two dashed lines follow the same trend as well. Figure 3 illustrates that trends in observed and estimated counterfactual outcomes at institutions in merit-adopting and non-adopting states follow the same time trend prior to treatment.

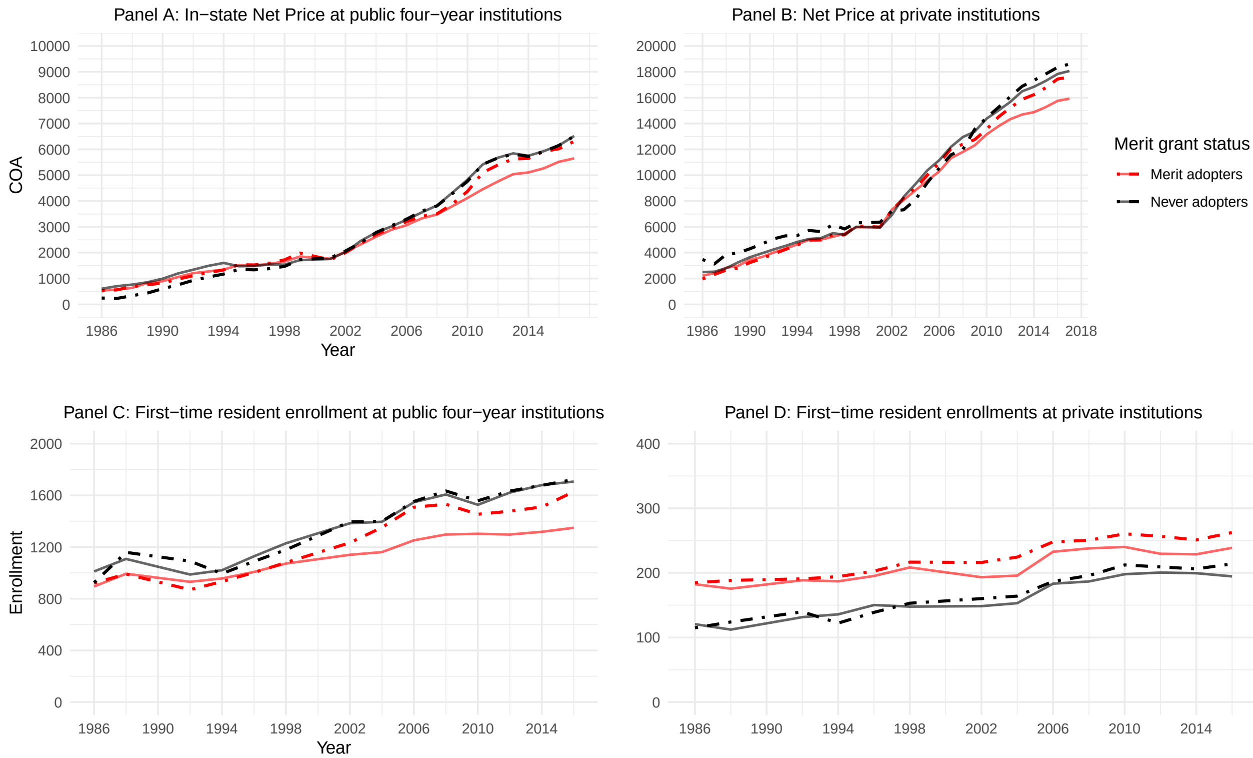


Figure 3: Average annual observed and predicted counterfactual net prices and resident enrollments
Note: Solid lines depict observed outcomes. Dashed lines depict predicted untreated potential outcomes. Counterfactual outcomes are predicted following Borusyak, Jaravel, and Spiess (2024). Vertical axes vary in each panel.

Table 2 presents the results from estimating equation 5 and show that there are, at best, only small changes in the racial and gender composition of student bodies at merit grant eligible institutions after the imposition of the merit grant program. We find a similar pattern of changes in student body composition at private not-for-profit institutions. These results do suggest that the student body compositions at merit-eligible institutions changed following program implementation but that the magnitude of these changes is small.

IPEDS does not collect data on student specific prices with which we could more precisely quantify how these changes in student body composition influence average net price, but we do not view these changes in student body composition as meaningfully impacting prices.[21] A critique of merit grant programs is that they predominantly go to students from well off families (Dynarski

21. In results not reported, we re-estimate equation 5 where Y is now the number of students in each group rather than the group's proportion of the student body. The change in the number of Asian, Black, and female students associated with merit grant implementation are all statistically insignificant at both institution types. The number of White students increases by 465 and 505 at public and private institutions, respectively.

2004). If merit grants induce more students with higher incomes to attend merit eligible institutions, these institutions may charge them higher net prices. This scenario would cause our estimates of ϵ to be biased downwards.

Table 2: The Impact of State Merit Grants on Student Body Composition

	Percent Asian	Percent Black	Percent White	Percent Female
Public 4-year	0.0034	-0.0082	0.0176	0.0034
	(0.0009)	(0.0034)	(0.004)	(0.0009)
Private 4-year	0.0026	-0.0047	0.0157	0.0026
	(0.0009)	(0.0038)	(0.0045)	(0.0009)

Note: Standard errors are clustered by state-year and are presented in parentheses. Point estimates are estimates of β from equation 5.

6.2 Main results

Next we present results from estimating Equation 1 via OLS. In this specification, the elasticity of supply is identified under the strong assumption that net prices and enrollments vary independently of each other. Our preferred model is the two stage least squares model which yields our baseline estimates; we begin here to facilitate comparisons between results from "naively" estimating ϵ via OLS versus estimating ϵ via TSLS. Results from estimating Equation 1 suggest that, on average, institutions across all sectors have inelastic supply curves. Point estimates and their standard errors are presented below in Table 3.[22] Public four-year and private institutions have similar estimated elasticities of supply of about 0.45 and 0.43. The estimate for public four- year institutions suggests that first-time first-year resident enrollment increases by 0.45% when net price increases by 1%. This set of results suggest that public four-year and private institutions face similarly shaped supply curves. Public two-year institutions have an estimated elasticity of -0.06 which is not statistically different from zero at the 95% confidence level. These results may be biased if enrollment and net tuition are simultaneously determined. Taken at face value, this set of OLS estimates of ϵ suggests that institutions have inelastic supply curves.

22. Estimates and standard errors for the full set of independent variables are shown in Appendix Table A2.

Table 3: OLS Estimates of ϵ from Equation 1

	Public 4-years	Private 4-years	Public 2-years
Point estimate	0.451	0.425	-0.062
Standard error	0.040	0.042	0.043

Note: Standard errors are clustered at the state-year level.

The point estimates of ϵ via TSLS, shown in Table 4 are larger for all sectors than those from Table 3.[23,24] All are statistically different from zero as well as from the corresponding OLS estimates. The estimated elasticity among public four-year institutions is 2.1, indicating that first-time first-year resident enrollments at public four-year institutions increase by 2.1% when net price increases by 1%. Private institutions have an estimated elasticity of supply of 1.31. As opposed to the OLS estimates which suggest inelastic supply responses, the results from estimating ϵ via TSLS suggest that both public four-year and private not-for-profit four-year institutions have elastic supply curves. It appears that public four-year institutions have a more elastic supply curve than private institutions, although the difference between the two estimates is not statistically significant. The estimated elasticity for public two-year institutions is 1.33 although after adjusting standard errors for the strength of the instrument in the first-stage (Lee et al. 2022) the estimate is not statistically significant. The first stage F-statistic for the public two-year results indicate that per-student merit grant expenditures may be a weak instrument for demand for public two-year institutions. It may be that merit grant awards induce recipients to attend a four-year institution over a two-year institution or that merit grant recipients are likely to attend a four-year institution with or without a merit grant award. These results suggest that four-year institutions have elastic supply curves, at least locally around the pre-merit grant program equilibria. Montiel Olea-Pflueger F-statistics from the first stages do not suggest that per-student merit grant expenditures is a weak instrument among the four-year sectors.

23. Point estimates are not statistically significant when standard errors are clustered by state or not clustered at all.

24. Results from the first stage regressions are shown in Appendix Table A4 and full results from the second stage regression are in Appendix Table A5.

Table 4: TSLS Estimates of ϵ from Equations 2 and 3

	Public 4-years	Private 4-years	Public 2-years
Point estimate	2.102	1.312	1.356
Standard error	0.570	0.339	0.587
0.05 tF std. error	0.665	0.488	1.137
First stage F-statistic	34.378	15.549	8.471

Note: Point estimates are the inverse of β_2 from equation 3. Unadjusted standard errors are estimated using the delta method. We follow Lee et al.'s suggestion of linearly interpolating between critical values to calculate the adjustment factor.

6.3 Robustness

We begin probing the robustness of our results by examining how our estimates change with alternate measures of quantity. Our baseline results measure quantity as the number of first-time full-time resident enrollees who graduated high school in the past 12 months. Institutions can target a certain level of enrollment by choosing how many applicants to offer admission but finalized enrollments are co-determined by students' enrollment decisions. Dorm capacity may be a more direct measure of an institution's desired enrollment level. Results from estimating equations 2 and 3 with dorm capacity as the quantity measurement are presented in Table 5. We only conduct this specification for the four-year sectors given that most public two-year institutions have large commuter populations or do not provide on-campus housing to their students. Estimates of ϵ are similar to the baseline estimates and the differences are not statistically different from each other. One reason that the short-run supply response to a financial aid expansion would be inelastic is that institutions may face frictions in expanding enrollment capacity. While expanding capacity involves more than building dorms, it appears that institutional dorm capacity is relatively responsive to changes in students' ability to pay.

Table 5: TSLS Estimates of ϵ with Dorm Capacity as Quantity

	Public 4-years	Private 4-years
Point estimate	2.013	1.600
Standard error	0.599	0.279
0.05 tF std. error	0.703	0.323
First stage F-statistic	21.979	35.285

Note: Point estimates are the inverse of β_2 from equation 3. Unadjusted standard errors are estimated using the delta method. We follow Lee et al.'s suggestion of linearly interpolating between critical values to calculate the adjustment factor.

We examine whether our results are robust to different parameterizations of merit grant exposure. We use two alternate measures of merit grant exposure. These results are presented in Table 6. The first is $StrongMerit_{st}$ which is a dummy variable equal to 1 if state s has a strong merit grant program in place in year t and 0 otherwise.[25] We parameterize treatment this way to test the robustness of our baseline results to relaxing the assumption that the level of merit grant expenditures is conditionally independent. The cost of relaxing this assumption is losing variation in the first stage equation by not allowing merit grant programs' effect on student demand to vary with their expenditures.

The second alternate way we measure the demand shocks generated by merit grant programs is using within-state average per-student merit grant expenditures $AvgMerit_{st}$. We calculate each state's average per-student merit grant expenditures by taking an average of per-student merit grant expenditures over the years in which the state funded a merit grant program. These averages are in Appendix Table A1. We then set $AvgMerit_{st}$ equal to a state's within-state average spending in the years where the state had a merit grant program in place and to 0 in the years in which a merit grant program was not funded. This specification averages out within-state variation across time in merit grant intensity (except for implementation) and leverages only the cross-state variation in merit grant expenditures to identify ϵ. This approach may yield biased results if the evolution of program generosity over time is endogenous. Our baseline alternate specification of merit grant

25. We focus only on strong merit grants in this analysis since weak merit grants may be only weakly correlated with demand for higher education. In results not shown we conduct this analysis with a dummy variable for whether a state has average per-student expenditures over $100. Point estimates differ slightly from using $StrongMerit_{st}$, but the results are qualitatively unchanged. Results available upon request.

exposure with $AvgMerit_{st}$ addresses this concern by averaging out the time-series variation in merit grant expenditures. Results for both specifications are presented in Table 6.

The point estimates of ϵ for private institutions when using any of the alternate IVs are in line with the baseline estimate of 1.31. All three estimates for the private sector are statistically insignificant when using the tF-adjusted standard errors from Lee et al. (2022). The point estimates for public four-year institutions increase substantially to 7.07 when price is instrumented for with $StrongMerit_{st}$, 3.59 when instrumenting for price with $AvgMerit_{st}$. However, there is a loss of precision when using these instruments and none of the point estimates are statistically different from zero. The first stage F-statistics suggest the alternate IVs are weakly correlated with demand for higher education. Estimates for the public two-year institutions are negative with low first stage F-statistics. Missing tF-adjusted standard errors in Table 6 are omitted because the tF-adjustment factor tends to infinity as the F-statistic approaches 3.84 (Lee et al. 2022) from above and the corresponding F-statistics are below 3.84. Given the variation in point estimates for this sector, we are hesitant to characterize the supply curves of public two-year institutions. While point estimates for four-year private and four-year public institutions vary across specifications they all indicate that these institutions have elastic supply curves.

Table 6: TSLS Estimates of ϵ Using Alternate IVs

		Public 4-years	Private 4-years	Public 2-years
Panel A				
	$StrongMerit_{st}$			
Point estimate		7.074	1.289	-0.047
Standard error		11.51	0.393	0.698
0.05 tF std. error		19.63	0.731	—
First stage F-statistic		10.505	8.565	0.005
Panel B				
	$AvgMerit_{st}$			
Point estimate		3.586	1.175	-0.093
Standard error		2.916	0.418	0.546
0.05 tF std. error		4.711	0.94	—
First stage F-statistic		11.728	6.925	0.005

Note: Point estimates are the inverse of β_2 from equation 3. Standard errors are estimated using the delta method. Adjusted standard errors are calculated following Lee et al. (2022). Omitted values for tF-adjusted standard errors are due to first-stage F-statistics less than 4, for which Lee et al. do not provide critical values for.

We also test the robustness of our baseline results to varying the institutions included in the analysis. We do so in two ways: by excluding institutions from states that implement a weak merit grant from the analysis, and by setting weak merit grant expenditures to zero, moving these institutions into the never-treated group. This exercise has two purposes. First, it tests the robustness of our results to the composition of the sample that is exposed to a merit grant program. Second, since weak merit grant programs typically offer smaller awards to fewer students, they should generate a smaller increase in demand for higher education than the strong, more well-funded, merit grant programs. In this specification, we identify ϵ using the same variation across states and time in merit grant expenditures as in the baseline model, but when we exclude institutions from weak merit grant states we are comparing institutions with zero merit grant exposure to institutions with substantial merit grant exposure.

Table 7: TSLS Estimates of ϵ Using Strong Merit Grant Expenditures Only

	Public 4-years	Private 4-years	Public 2-years
Dropping weak merit states			
Point estimate	1.64	0.711	0.202
Standard error	0.448	0.4	0.443
0.05 tF std. error	0.62	—	—
First stage F-statistic	17.466	3.185	0.22
Setting weak merit grant expenditures = 0			
Point estimate	1.761	1.402	0.619
Standard error	0.587	0.395	0.488
0.05 tF std. error	0.787	0.601	—
First stage F-statistic	19.46	13.446	1.716

Note: Point estimates are the inverse of β_2 from equation 3. Standard errors are estimated using the delta method. Adjusted standard errors are calculated following Lee et al. (2022). Omitted values for tF-adjusted standard errors are due to first-stage F-statistics less than 3.84, for which tF-adjustment factors approach infinity

We present results from varying how weak merit grant programs are treated in Table 7. The first panel of Table 7 presents results when we omit institutions from weak merit grant- adopting states from the analysis entirely. Estimates from this specification are all less than the corresponding baseline estimates. The estimated elasticity of supply for public four-year institutions is 1.64, reaffirming our finding that public four-year institutions have an elastic supply response. Estimates of ϵ for private four-year and public two-year institutions are both statistically insignificant. When zeroing out weak merit grant expenditures and moving institutions in these states into the never-treated group, public four-year institutions have an estimated elasticity of supply of 1.76 and private institutions have an estimated elasticity of supply of 1.4. Both estimates are statistically significant with first-stage F statistics greater than 10. Public two-year institutions have an estimated elasticity of supply of 0.62. This estimate is statistically insignificant with a first-stage F-statistic of only 1.72.

6.4 Heterogeneity

We split the four-year sectors into subsectors to study whether there are heterogeneous responses within the public and private not-for-profit four-year sectors. We do this first by further categorizing institutions by highest degree granted and then by student body size. We conduct these subsector analyses for two reasons. First, public four-year and private four-year not-for-profit are broad categories. Different types of institutions within them may face differing supply curves and so there may be heterogeneity within these sectors. Second, one may question whether it's appropriate to compare, say, a public flagship research institution to a regional public institution that only offers undergraduate courses, both of which are in the public four-year sector. Following the literature, we separate the public and private four-year sectors into doctorate-, masters-, and baccalaureate-granting institutions using the 2015 Carnegie classifications. Table 8 presents these results.

Table 8: TSLS Estimates of ϵ by 2015 Carnegie Classification

	Estimate	Std. err	0.05 tF Std. err	1st-stage F-statistic	n
Public 4-years (Baseline)	1.605	0.493	0.665	21.462	6517
Public Doctoral	3.615	5.536	—	3.892	2414
Public Masters	1.150	0.568	1.288	6.504	3041
Public Baccalaureate	-0.234	0.243	—	0.950	1062
Private 4-years (Baseline)	1.319	0.341	0.488	15.187	9902
Private Doctoral	0.479	0.773	—	0.389	1235
Private Masters	2.500	0.867	1.254	15.386	4316
Private Baccalaureate	1.196	0.423	0.68	11.858	4388

Note: Point estimates are the inverse of β_2 from equation 3. Standard errors are estimated using the delta method. Adjusted standard errors are calculated following Lee et al. (2022). Omitted values for tF-adjusted standard errors are due to first-stage F-statistics less than 3.84, for which tF-adjustment factors approach infinity.

Estimated elasticities for the highest-degree-granted subsectors are imprecise. The point estimate for private masters-granting institutions is 2.5 and is statistically significant at the 90% confidence level. Point estimates for the other subsectors are all statistically insignificant. The results suggest that private doctoral institutions have inelastic supply curves. However, given the large standard errors in this analyses, we cannot distinguish differential supply responses to state merit grant programs by highest degree granted.

We also look for heterogeneity in supply curves by institutional size. We split the public and private four-year sectors by size using the Carnegie classification's size categories. We combine their smallest two categories of less than 1,000 students and at least 1,000 and less than 3,000 students into one category to improve precision. The other two categories are at least 3,000 and less than 10,000 students, and more than 10,000 students. We categorize institutions by taking within-institution averages of their total enrollments measured in the fall semester from IPEDS.

Private institutions with less than 3,000 students have slightly inelastic supply curves. As before, we lack power to conduct heterogeneity analysis as all other point estimates are not statistically significant.

Table 9: TSLS Estimates of ϵ by institution size

Sector	Size	Estimate	Std. err	0.05 tF Std. err	1st-stage F-statistic	n
Public	<3000	0.222	0.253	—	0.816	1274
	3000-9999	1.922	1.922	9.173	4.458	2363
	>10000	1.395	0.497	0.887	9.681	2931
Private	<3000	0.948	0.251	0.382	13.526	8231
	3000-9999	1.019	0.884	—	1.568	192
	>10000	-2.436	2.091	—	3.575	1731

Note: Point estimates are the inverse of β_2 from equation 3. Standard errors are estimated using the delta method.Adjusted standard errors are calculated following Lee et al. (2022). Omitted values for tF-adjusted standard errors are due to first-stage F-statistics less than 3.84, for which tF-adjustment factors approach infinity.

Taken as a whole, our results point towards public and private four-year institutions having elastic supply curves. While we lose precision in robustness checks, point estimates suggest elastic supply responses. Estimates for public two-year institutions vary more and are frequently not statistically different from zero. We refrain from making statements about the elasticity of supply for institutions in this sector given the wide range of ϵ that our results can support. Point estimates suggest that there is further heterogeneity in ϵ within the four-year sectors and across institutional size, although we run into power issues that preclude us from precisely estimating differences in elasticities of supply within the public and private four-year not-for-profit sectors.

7 Conclusion

Financial aid is meant to improve accessibility to higher education for students. Basic demand and supply analysis indicates that one likely outcome from any effort to subsidize the costs of higher education for students is an increase in tuition if institutions experience increasing marginal costs when they increase output to meet the increase in demand. The increase may be particularly large if institutions face frictions that make it difficult for them to expand enrollment capacity in the short run, such as hiring additional instructors or building more student housing, that is if they have an inelastic short-run supply curve. In this case in may be that the bulk of the subsidy accrues to higher education institutions instead of students. Previous work examining institutional responses to financial aid expansions have mostly focused on changes in tuition, perhaps reflecting a belief that institutions' have inelastic short-run supply responses. In contrast, we study the institutional responses by estimating the short-run elasticity of supply, which reflects both short-run changes tuition and enrollment and allows us to assess the relative share of benefits accruing to students and institutions.

To identify the short-run supply elasticity we leverage cross-state variation the generosity of state merit grants programs as plausibly exogenous shifts in demand for higher education while holding the supply curve of higher education fixed. Our baseline results find that both public four-year and private not-for-profit institutions have an estimated elasticity of supply of 2.1 and 1.31, respectively, suggesting that these institutions have elastic short-run supply curves. This indicates that following a increase in financial aid, enrollments increase more relative to net tuition and that students receive a larger share of the benefits from merit-grant programs. When looking at heterogeneity in supply curves within the public and private sectors, estimates of ϵ are not significantly different from zero for public doctoral and baccalaureate and private doctoral institutions, although this result is more likely due to a lack of variation rather than these institutions having perfectly inelastic supply curves. We also study heterogeneous responses by institutional size but lack the variation for precise inference. Our results suggest that the merit grant programs we study here are successful in improving access to higher education for resident students. While there are distributional concerns about merit grant programs (Dynarski 2004; Fitzpatrick and Jones 2016), they do seem to reduce financial barriers to higher education for recipients and induce institutions to expand to educate more students.

The results of this paper should be viewed within the context of its limitations. We have made stronger assumptions than previous studies have about the exogeneity of changes in financial aid (by assuming the conditional independence of merit grant generosity in addition to implementation). A battery of robustness checks suggest that our baseline results are robust to relaxing the identifying assumptions about the exogeneity of merit grant aid as well as varying measurements of quantity. In return for making these stronger assumptions, we are able to identify the elasticity of supply, which previous studies have not estimated. These estimates will be biased if there is non-random selection into merit grant generosity or if institutions respond to these programs directly rather than to changes in student behavior. However, the nature of the rollout of merit grant programs suggest that policymakers were experimenting with a new form of financial aid rather than responding to educational or economic factors (Dynarski 2004), which lessens the concern that the generosity of the programs is a function of institutions or student behavior.

Despite these concerns and the limitations of our analysis, our results do indicate that students receive the bulk of the benefits from state merit-aid programs. These programs appear to reduce the financial barriers to higher education and induce institutions to expand access to more students. More generally, our results are suggestive that financial aid programs that target students instead of institutions can be an effective policy for improving access to higher education.

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Appendix

Parent-Child Reporting in IPEDS

A result of the IPEDS survey scheme is that the unit of observation can be a standalone entity or a group of separate campuses. Some institutions are part of a multicampus Title IV institution and data for these institutions may be aggregated to the system level. For example, the Pennsylvania State University system has 24 campuses which are all part of one multi-campus institution (Jaquette and Parra 2014). Some variables, such as endowment size and assets and liabilities, are reported cumulatively for all campuses under the flagship University Park campus. In contrast, institutions in other state systems, such as the University of Wisconsin (UW) system, each report data separately. As a result it would be a mistake to compare, for example, the reported assets of Pennsylvania State - University Park and UW- Madison. This reporting pattern is called "parent-child" reporting (PCR) since child campuses report data under the "parent" campus. Finance variables are affected by PCR. Child campuses may be missing data and data reported under parent campuses at the system level are inflated. While worth acknowledging, PCR is not overly prevalent in the sample. Only 2.8% of institution-year observations in the sample are flagged as parent institutions and 7% as children.

IPEDS includes identifiers for PCR status and if the institution is a child-reporter, the parent's identifier. These linkages are sometimes missing. We utilize Office of Postsecondary Education IDs (OPEID) to fill in missing linkages. Most of the missing parent-child linkages occur before 1997. We fill in pre-1997 missing parent-child linkages for institutions that have non-missing and unchanging parent-child identifiers in 1997 through 1999. The parent institution of an institutional system can change year to year, but in filling in pre-1997 missing linkages we assume they do not. We allocate finance variables by first calculating total annual system enrollments by summing the number of degree- and certificate-seeking undergraduates at each institution within a system Then we multiply each financial variable by each institution's percent of total system enrollment. Values of finance variables for institutions that are not part of an encompassing system are unchanged since their fraction of total "system" enrollment is one. This enrollment percentage (and any) allocation rule likely induces measurement error, although given the low rate of PCR in the data this potential for measurement error is likely inconsequential.

Tables and Figures

Table A1: Merit Grant Summary Statistics by State

Strong/Weak Program	State	Cohort	Average Merit Grant Award
Strong merit grants	FL	1997	308.00
	GA	1994	757.40
	KY	1999	424.13
	LA	1998	716.46
	NM	1997	389.34
	NV	2000	234.11
	SC	1998	828.78
	TN	1998	609.09
	WV	2002	601.72
Weak merit grants	AK	2011	267.45
	AL	1999	6.99
	AR	1992	357.70
	CO	2001	21.75
	DE	1998	89.25
	IA	1998	2.39
	ID	1998	42.38
	IL	1998	5.64
	IN	1998	1.21
	MA	1998	7.22
	MD	1999	13.06
	MI	2003	59.45
	MO	1998	136.74
	MS	1999	143.71
	MT	1998	20.64
	NC	2005	1.09
	ND	1998	8.18
	NJ	1998	36.28
	NY	1998	10.84
	OH	1998	10.99
	OK	1998	60.67
	PA	2008	3.36
	SD	2005	96.00
	TX	2005	17.37
	UT	1998	18.79
	VT	1998	3.02
	WA	1998	6.88
	WI	1998	10.69

Note: Expenditures are in real 2016 dollars. Per-student expenditures are calculated as total merit grant expenditures divided by the estimated number of enrolled undergraduates in the state-year from the October CPS. We follow Sjoquist and Winters (2015) and Kramer, Ortagus, and Lacy (2018) in defining merit grant programs as strong or weak.

Table A2: Full Results from Estimating Equation 1

	Public 4-years	Private 4-years	Public 2-years
ln(NetPrice)	0.451	0.425	-0.062
	(0.040)	(0.042)	(0.043)
Per-student federal revenues	62.441	25.828	-200.950
	(7.422)	(1.998)	(15.532)
Per-student state + local revenues	1.331		-111.122
	(5.974)		(9.097)
Num. 18-24 year olds	4.010	2.973	-2.155
	(0.883)	(0.994)	(1.355)
State-year unemployment rate	-0.005	-0.006	0.040
	(0.011)	(0.010)	(0.017)
Per-student private revenues		-26.315	
		(2.712)	
Num.Obs.	5560	8019	6864

Note: Standard errors are clustered at the state-year level. All dollar-value variables are re-scaled to thousands of real 2016 dollars. The number of 18-24 year olds in a state-year is measured in tens of thousands.

Table A3: Results from estimating event study models

	Public 4-yr Enrollment		Public 4-yr Tuition		Private 4-yr Enrollment		Private 4-yr Tuition	
Event time	BJS	TWFE	BJS	TWFE	BJS	TWFE	BJS	TWFE
-5		0.059		0.066		-0.015		0.001
		0.042		0.038		0.014		0.01
-4		-0.032		-0.015		-0.013		-0.008
		0.047		0.027		0.014		0.011
-3		0.037		0.054		-0.008		-0.013
		0.031		0.046		0.015		0.018
-2		-0.022		0.012		-0.023		0.008

		0.043		0.045		0.02		0.015
-1		-		-		-		-
		-		-		-		-
0	-0.027	-0.02	-0.003	0.001	-0.021	-0.003	0.013	0.017
	(0.025)	(0.047)	(0.027)	(0.038)	(0.025)	(0.019)	(0.009)	(0.016)
1	0.04	-0.004	0.027	0.036	0.067	0.002	0.014	0.012
	(0.048)	(0.044)	(0.026)	(0.059)	(0.043)	(0.019)	(0.012)	(0.014)
2	-0.119	-0.085	-0.074	-0.036	-0.077	0.006	0.025	-0.006
	(0.035)	(0.06)	(0.03)	(0.06)	(0.028)	(0.026)	(0.019)	(0.021)
3	-0.021	0.041	0.071	0.069	0.015	0.013	0.001	0.009
	(0.052)	(0.047)	(0.049)	(0.072)	(0.048)	(0.022)	(0.017)	(0.017)
4	-0.148	-0.146	0.069	0.057	-0.144	0.008	-0.01	-0.008
	(0.035)	(0.051)	(0.071)	(0.063)	(0.038)	(0.02)	(0.018)	(0.016)
5	0.009	0.031	-0.007	0.016	0.03	0.004	-0.01	0.004
	(0.061)	(0.051)	(0.045)	(0.051)	(0.056)	(0.024)	(0.018)	(0.018)
6	-0.122	-0.106	0.054	0.046	-0.116	-0.007	-0.024	-0.018
	(0.05)	(0.064)	(0.063)	(0.065)	(0.052)	(0.024)	(0.022)	(0.019)
7	0.004	0.036	0.006	-0.006	-0.005	-0.002	0.008	-0.004
	(0.065)	(0.061)	(0.046)	(0.068)	(0.058)	(0.026)	(0.018)	(0.02)
8	-0.162	-0.155	0.053	0.046	-0.162	0.007	0.006	-0.002
	(0.078)	(0.073)	(0.064)	(0.066)	(0.072)	(0.027)	(0.013)	(0.016)
9	0.005	0.067	0.013	0.003	0.02	0.012	-0.015	-0.013
	(0.073)	(0.072)	(0.052)	(0.071)	(0.071)	(0.027)	(0.021)	(0.018)
10	-0.144	-0.126	0.032	0.005	-0.149	0.016	-0.014	-0.003
	(0.068)	(0.077)	(0.077)	(0.079)	(0.068)	(0.027)	(0.016)	(0.019)
11	0.022	0.08	-0.01	0.001	0.021	0.025	0.003	0
	(0.081)	(0.075)	(0.063)	(0.075)	(0.072)	(0.027)	(0.02)	(0.021)
12	-0.132	-0.122	-0.039	-0.04	-0.126	0.018	-0.008	-0.011
	(0.077)	(0.076)	(0.082)	(0.083)	(0.068)	(0.032)	(0.026)	(0.022)

13	-0.011	0.071	-0.065	-0.009	0.046	0.019	-0.021	-0.009
	(0.093)	(0.106)	(0.093)	(0.092)	(0.076)	(0.033)	(0.029)	(0.026)
14	-0.142	-0.117	-0.002	-0.037	-0.142	0.005	-0.047	-0.017
	(0.064)	(0.087)	(0.086)	(0.091)	(0.064)	(0.033)	(0.033)	(0.027)
15	-0.004	0.101	0.008	0.042	0.035	0.009	-0.052	-0.015
	(0.082)	(0.114)	(0.098)	(0.1)	(0.081)	(0.035)	(0.03)	(0.028)
16	-0.081	-0.037	0.023	0.005	-0.082	0.009	-0.032	-0.003
	(0.073)	(0.092)	(0.083)	(0.087)	(0.071)	(0.038)	(0.038)	(0.027)
17	-0.001	0.109	-0.068	-0.015	-0.029	-0.004	-0.022	0.006
	(0.124)	(0.152)	(0.105)	(0.105)	(0.102)	(0.039)	(0.035)	(0.027)
18	-0.144	-0.078	-0.034	-0.026	-0.137	-0.009	-0.049	-0.015
	(0.104)	(0.107)	(0.083)	(0.089)	(0.088)	(0.041)	(0.037)	(0.027)
19	0.308	0.196	-0.071	0.032	0.266	-0.013	-0.069	-0.021
	(0.2)	(0.163)	(0.224)	(0.164)	(0.125)	(0.043)	(0.037)	(0.027)

Note: Standard errors are clustered by state-year and are presented in parentheses.

Table A4: First stage results from estimating equation 2

	Public 4-years	Private 4-years	Public 2-years
Per-student merit expenditures	0.277	0.181	0.171
	(0.047)	(0.046)	(0.059)
Per-student federal revenues	0.019	0.018	-0.212
	(0.008)	(0.004)	(0.020)
Per-student state + local revenues	-0.024		-0.104
	(0.005)		(0.011)
Average Pell Grant award	-0.681	-0.413	0.064
	(0.041)	(0.051)	(0.041)
Per-student instructional expenditures	0.061	0.002	-0.003
	(0.007)	(0.003)	(0.015)
Num. 18-24 year olds	0.006	0.004	-0.001
	(0.001)	(0.001)	(0.001)
State-year unemployment rate	0.000	0.006	0.039
	(0.010)	(0.009)	(0.016)
Per-student private revenues		-0.031	
		(0.006)	
Num.Obs.	6517	9902	8081

Note: Standard errors are clustered at the state-year level. All dollar-value variables are re-scaled to thousands of real 2016 dollars. The number of 18-24 year olds in a state-year is measured in tens of thousands.

Table A5: Second stage results from estimating equation 3

	Public 4-years	Private 4-years	Public 2-years
$\widehat{ln(Q^d_{ist})}$	0.476	0.762	0.737
	(0.129)	(0.197)	(0.319)
Per-student federal revenues	-0.009	-0.020	0.089
	(0.006)	(0.005)	(0.071)
Per-student state + local revenues	0.001		0.043
	(0.006)		(0.036)
Average Pell Grant award	0.209	0.094	0.046
	(0.085)	(0.080)	(0.044)
Per-student instructional expenditures	-0.029	0.002	0.027
	(0.009)	(0.002)	(0.014)
Num. 18-24 year olds	-0.001	-0.001	0.004
	(0.001)	(0.001)	(0.001)
State-year unemployment rate	0.039	0.004	-0.022
	(0.008)	(0.008)	(0.021)
Per-student private revenues		0.012	
		(0.010)	
Num.Obs.	6517	9902	8081

Note: Standard errors are clustered at the state-year level. All dollar-value variables are re-scaled to thousands of real 2016 dollars. The number of 18-24 year olds in a state-year is measured in tens of thousands.