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# Price Regulation, Price Discrimination, and Equality of Opportunity in Higher Education: Evidence from Texas†

*By* Rodney J. Andrews and Kevin M. Stange*

*We assess the importance of price regulation and price discrimination to low-income students' access to opportunities in public higher education. In 2003, Texas shifted tuition-setting authority away from the state legislature to public universities themselves. In response, most institutions raised sticker prices and many began charging more for high-earning majors, such as business and engineering. We find that poor students actually shifted toward higher earning programs following deregulation, relative to non-poor students. Deregulation facilitated more price discrimination through increased grant aid and enabled supply-side enhancements, which may have partially shielded poor students from higher sticker prices.* (*JEL* D63, H75, I22, I23, I24, I28, I32)

Public support for post-secondary educational investment is substantial and long standing. For example, states spent $173 billion on higher education in 2012, permitting public institutions to provide post-secondary education to millions of students at a price well below cost (NASBO 2013). However, tight state budgets have recently challenged states' ability to both ensure broad access to higher education and provide programs of high quality, with large funding cuts particularly during the Great Recession (Barr and Turner 2013). Funding cuts that trigger tuition increases could widen the existing large gaps between high- and low-income students in college enrollment (Bailey and Dynarski 2011), particularly at the most selective institutions. This would be problematic given the large returns to a college education generally (Zimmerman 2014) and for the most selective institutions and majors specifically (Hoekstra 2009; Hastings, Neilson, and Zimmerman 2013; Kirkebøen, Leuven, and Mogstad 2014). Spending cuts that reduce program quality may additionally reduce degree completion (Bound, Lovenheim, and Turner 2010; Cohodes and Goodman 2014). In sum, there are important economic consequences

*Andrews: The University of Texas at Dallas, 800 West Campbell Road, WT21, Richardson, TX 75080 (email: rodney.j.andrews@utdallas.edu); Stange: The University of Michigan, 5130 Weill Hall, 735 S. State Street, Ann Arbor, MI 48109 (email: kstange@umich.edu). John Friedman was coeditor for this article. We thank John Thompson and Pieter DeVlieger for exceptional research assistance and numerous seminar and conference participants for helpful feedback. We also thank the Texas Higher Education Coordinating Board for providing assistance with various data elements and institutional history. Generous funding was provided by the Russell Sage Foundation (83-15-12) and the Spencer Foundation (201450047). Both authors declare that they have no relevant or material financial interests that relate to the research described in this paper. The conclusions of this research do not necessarily reflect the opinions or official position of the Texas Education Agency, the Texas Higher Education Coordinating Board, or the State of Texas.

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to the ways in which public higher education institutions balance their dual access and quality objectives.

We study these questions in Texas, where short-term state spending cuts in 2003 were paired with a permanent shift in tuition-setting authority away from the state legislature to the governing board of each public university, termed "tuition deregulation." Most universities subsequently raised prices, and many began charging more for high-demand or costly undergraduate majors, such as business and engineering (Kim and Stange 2016). The presidents of major research universities claimed that tuition-setting flexibility enables institutions to expand capacity and help students succeed by enhancing program quality (Yudof 2003). Detractors worried that price escalation would limit access to the most selective institutions and most lucrative programs for low-income students (Hamilton 2012). This concern motivated a bundling of deregulation with additional grant aid to partially shield low-income students from price increases, as we describe further below. This study exploits these policy changes, as well as rich administrative data on all high school graduates in the state from 2000–2009 and new measures of tuition and resources at a program level, to assess how tuition deregulation affected the representation of poor students in high-earning institutions and majors.[1]

To establish a baseline, we first document substantial earnings differences across post-secondary programs (both within and across institutions) and show that students are underrepresented in the highest return programs. On average, poor students entered programs that generate earnings gains that are 3.7 percent lower than non-poor students prior to deregulation, after accounting for differences in demographics and achievement test scores. We then show that price increases were largest for the highest return programs following deregulation. This raises the concern that deregulation would exacerbate disparities in poor student representation in these programs given low-income students' greater price responsiveness (Jacob, McCall, and Stange 2018).

Our main analysis estimates the causal effect of these reforms on the relative attendance patterns of poor versus non-poor students using an interrupted time series and event-study strategy. We find that poor students shifted away from the least lucrative programs and increased their representation in higher earning programs relative to non-poor students. This shift is quantitatively important, closing the 3.7 percent gap by more than one-third. This finding is robust to various strategies for ruling out potential confounders, including changes in student characteristics and other policy changes, such as delayed effects of the Top 10 Percent rule, targeted outreach, and affirmative action, that may alter the sorting of students in higher education. We also show that the shift in initial program choice persists for at least two years following initial enrollment, so it is likely to result in real relative improvements in the economic well-being of low-income students. While estimates for longer term outcomes such as graduation and actual earnings are noisy, taken together, they suggest that deregulation did not worsen poor students' outcomes.

[1] Flores and Shepard (2014) is the only study that examines the effects of this policy change. Using aggregate institution-level data, they find that price accelerated at seven Texas institutions following deregulation, but effects on overall enrollment of minority students and Pell Grant recipients were mixed (but underpowered).

Greater income-based price discrimination following deregulation permitted these programs to retain (or even expand) low-income student representation while simultaneously raising sticker price and program quality.[2] We show this may have resulted from considerable increases in need-based grant aid in programs with large price increases, such that the net price that low-income students paid fell relative to that for non-poor students. Two features of the policy and environment helped ensure this result. First, an explicit provision of deregulation required institutions to set aside some incremental revenue for grant aid. Second, the presence of a large need-based state aid program, the Toward EXcellence, Access and Success (TEXAS) Grant, automatically increased grant aid for poor students to cover higher sticker prices. Program resources also improved the most for programs with the highest earnings. The favorable relative change in the price/quality package offered to poor students improved low-income students' access to the most lucrative state university programs.

Our findings contribute to three distinct literatures. First, we provide evidence on the distributional consequences of price discrimination in higher education. Prior work finds that price discrimination can be beneficial to low-income individuals both in higher education (Fillmore 2014) and other industries by lowering relative prices. However, lacking sufficient policy change, this work has been mostly theoretical or based on simulations. There is almost no reduced-form evidence that traces the distributional consequences of a policy change that permits greater price discrimination. Price discrimination means that the greater price and resource differentiation seen among US colleges (Hoxby 2009) does not necessarily adversely affect low-income students. Ours is the first study to look at a broad shift from a regime of broad-based subsidies (low sticker price) to one of specific subsidies (higher sticker price plus greater aid) in higher education.

Second, we provide some of the first evidence on the effects of deregulation, and university autonomy more generally, on the higher education market. Prior work has found that university autonomy is positively associated with research output (Aghion et al. 2010), but the equity or efficiency consequences of greater institutional autonomy (and the resulting differentiation) in undergraduate education have not been previously examined.

Finally, we provide further evidence that heterogeneity of human capital investment opportunities is materially important (Altonji, Blom, and Meghir 2012), even within the context of a public university system in a single state. Thus, the sorting of students across programs materially affects how a state's higher education system alters the intergenerational transmission of income.

This study is both timely and of broad policy importance beyond the state of Texas. Florida and Virginia recently decentralized tuition-setting authority, and several other states (New York, Washington, Wisconsin, Ohio) and Australia have all considered similar proposals (McBain 2010, Camou and Patton 2012). Just

[2] In the absence of multiple "mechanism" quasi-experiments, we cannot separately identify the contribution of each potential channel—for example, sticker price, price discrimination, program resources, and admissions—to the reduced-form sorting patterns we observe without additional structure. So we view the investigation of channels as suggestive. However, since deregulation in Texas and elsewhere is a package of all of these changes, the combined effect is the primary target for policy.

two years ago, voters in Louisiana rejected a plan that was quite similar to Texas' combination of deregulation and grant aid. The Texas experience suggests that deregulation need not widen socioeconomic gaps, as many critics worried. Indeed, our findings echo the experience in England, where the end of free college was associated with increased resources and improvement in college socioeconomic gaps (Murphy, Scott-Clayton, and Wyness 2017). Two key features of tuition deregulation in the Texas case are the requirement that institutions channel some of the revenue generated by deregulation toward need-based aid and the presence of a large state-financed need-based aid program. How deregulation would have evolved absent these features remains an open question. Still, the lessons learned from Texas's deregulation policy are broadly applicable as most proposed deregulation efforts include a package of reforms—pricing independence and additional grant aid—that are similar to those offered by Texas.

This paper proceeds as follows. The next section provides background on tuition deregulation in Texas, its financial aid programs, and prior literature. Section II describes our data, sample, and student earnings across programs. Methods and results are presented in three parts. Section III documents the large price changes following deregulation. Section IV assesses changes in student sorting following deregulation. Section V investigates both price and non-price mechanisms. Section VI concludes.

## I. Background

### A. *Texas Context and Deregulation*

Public university tuition in Texas consists of two components, statutory and designated tuition (Texas Higher Education Coordinating Board [THECB] 2010a). Statutory tuition (authorized under Texas Education Code [TEC] 54.051) is a fixed rate per credit hour that differs only by residency status but is otherwise constant across institutions and programs. Designated tuition is a charge authorized by TEC 54.0513 that permits institutions to impose an additional tuition charge that the governing board of the institution deems appropriate and necessary. Though designated tuition charges are determined by institutions, the legislature historically capped designated tuition at the level of statutory tuition.[3]

Cuts to state appropriations in 2002 led many institutions to advocate for more flexibility in setting tuitions. Flagship universities argued that the existing revenue model did not adequately consider differences between institutions (Yudof 2003). They believed that tuition flexibility would help maintain existing levels of service and increase institutions' ability to respond to educational and economic development needs. In September of 2003, the legislature passed House Bill (HB) 3015, which modified TEC 54.0513 to allow governing boards of public universities to set different designated tuition rates, with no upper limit. Furthermore, institutions

[3] Universities are also allowed to charge mandatory and course fees for costs that are associated with services or activities. In fall 2002, the average mandatory fee in the state was $454, which ranged from $160 (University of Houston–Victoria) to $1,175 (University of Texas–Dallas), while the average course fee charged was $61.

could vary the amount by program, course level, academic period, term, credit load, and any other dimension institutions deem appropriate. Since annual price setting occurs in the prior academic year, the fall 2004 semester was the first semester that universities could fully respond to deregulation. Community colleges and private universities did not experience a similar change in their price-setting capabilities.[4]

Figure 1 depicts the price changes following deregulation. Deregulation was associated with large increases in sticker price level, growth, and differentiation immediately after deregulation. Kim and Stange (2016) demonstrates that these changes are unique to Texas—similar levels of growth were not seen in other states. The 50 percent increase in cross-program variability in tuition partially reflects the adoption of differential pricing across programs, particularly for engineering and business (Kim and Stange 2016). Texas institutions thus followed a national trend of engaging in differential pricing for more costly and/or lucrative majors (Stange 2015). To reduce the likelihood that tuition increases would disproportionately burden low-income students, institutions were required to set aside a share of the additional revenue for financial aid for needy students (which we describe in detail below). The legislature also mandated that institutions show progress toward performance goals (graduation, retention rates, and affordability), though the oversight for this does not appear to have been put in place (McBain 2010).

These abrupt changes in pricing and state support came against a backdrop of several other efforts to affect student choices and success. The Top 10 Percent rule guaranteeing admission to any public institution for students ranked in the top decile of their high school went into effect in 1998 and increased enrollment at the state's flagships (Daugherty, Martorell, and McFarlin 2012). Several targeted financial aid and outreach programs improved access to University of Texas–Austin and Texas A&M among low-income students (Andrews, Ranchhod, and Sathy 2010; Andrews, Imberman, and Lovenheim 2016). Finally, the state's Closing the Gaps initiative was a broad effort to improve access and graduation rates for underrepresented minorities.

### B. *Financial Aid in Texas before and after Deregulation*

The financial impact of deregulation on poor students was a central concern of policymakers. Consequently, several features of the deregulation legislation interacted with the state's financial aid programs to shield low-income students from the resulting price increases. Most directly, the deregulation legislation required that 15 percent of the revenue generated from designated tuition charges in excess of \$46 per semester hour be set aside to provide aid for needy undergraduate or graduate students in the form of grants or scholarships. Institutions have near complete discretion in determining which students receive aid from this source, referred to as "HB 3015 set-asides," with the constraint that recipients must be needy.

[4] Tuition rates for community colleges are determined by each community college taxing district (CCTD), resulting in different tuition rates across CCTDs throughout our analysis period. In 2005, CCTDs were granted the authority to charge different tuition rates for different programs. However, we show in online Appendix Figure A1 that subsequent changes in overall community college prices were modest relative to those at universities. Furthermore, few colleges implemented large changes in differential pricing across programs that were not already reflected in program fees.

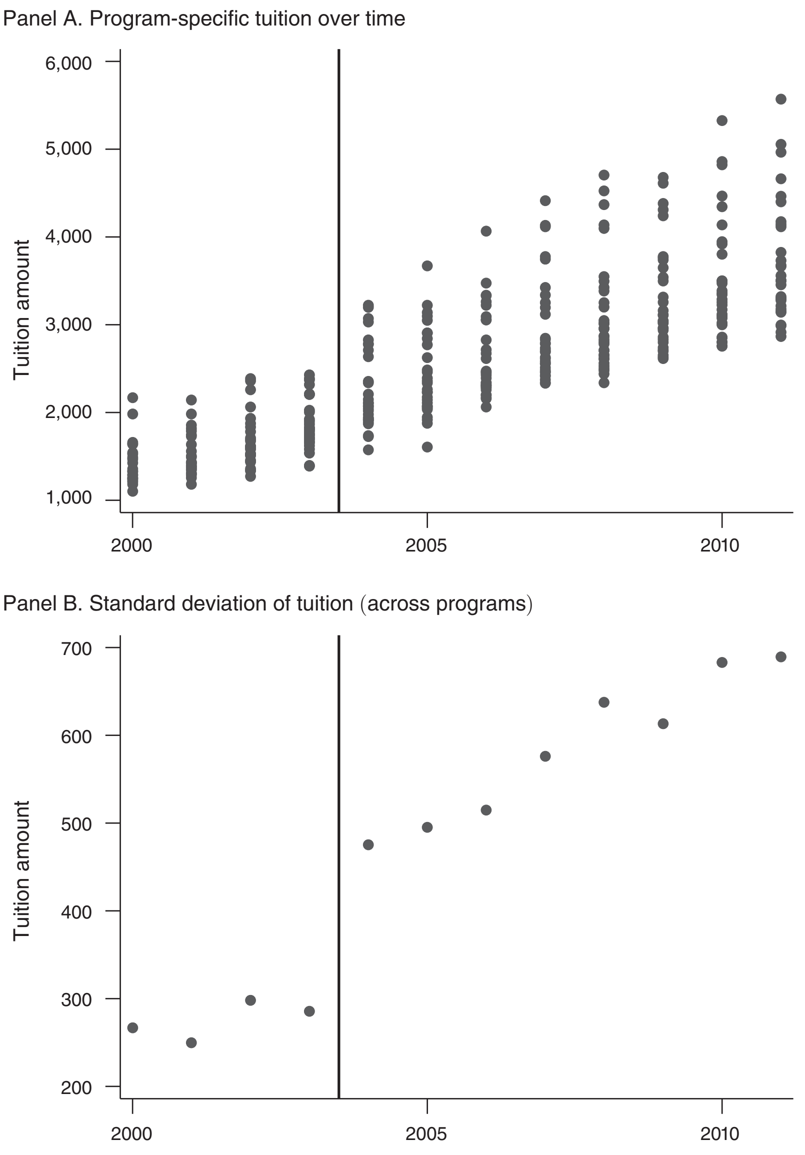


Figure 1. Trends in Fall Tuition over Time (in-state juniors taking 15 semester credit hours)

*Notes:* Sample includes approximately 640 programs observed each year. Panel A plots the actual sticker price for each program each year. Panel B plots the standard deviation of sticker price across all programs in each year. Sticker price was obtained from course catalogs and archival sources and captured separately for each identifiable program (with a distinct tuition or fee), residency status, undergraduate level, academic year, entering cohort, and number of credit hours.

Also important is the TEXAS Grant program, which provided \$193 million to nearly 40,000 needy students in 2009 (THECB 2010b).[5] Eligibility is determined by need and having met high school curricular requirements (for initial grantees) or basic college performance (for continuing grantees). Total TEXAS Grant funds are allocated by the state to each institution annually, but institutions have discretion for determining which eligible students receive awards and how much to give (up to the statutory maximum). Two features of the TEXAS Grant work to shield poor students from tuition price increases. First, the statutory maximum is the statewide average of tuition and fees, so tuition increases raise the maximum award allowed by statute. This maximum does not, however, depend on the institution attended, so it should not be expected to differentially affect some programs more than others. Second, institutions are obligated to provide non-loan aid to cover the student's full tuition and fees up to demonstrated financial need to all TEXAS Grant recipients, regardless of the award amount. Increases in tuition prices thus increase institutions' grant obligations to TEXAS Grant recipients beyond the amount of the TEXAS Grant itself. [6] HB 3015 set-aside funds can be used to close this gap, and our discussions with higher education officials in the state suggested institutions did just that. Deregulation could thus crowd in support from the TEXAS Grant, particularly at programs that increased prices the most. Later we show that the HB 3015 set-asides were particularly large for poor students in programs that became more costly and that TEXAS Grants also expanded slightly more for these programs. Though deregulation occurred amidst a backdrop of increased funding for the TEXAS Grant, we subsequently show the TEXAS Grant (and its expansion) cannot fully explain the patterns in program choice that we document.

Student aid provided through two other large need-based grant programs—the Texas Public Educational Grant (TPEG) and the federal Pell Grant—should have been unaffected by deregulation. TPEG is funded by a 15 percent set-aside from statutory tuition at each institution. The institutions have discretion in selecting which eligible students receive an award. TPEG distributed \$88.4 million to 60,681 college students in Texas in 2009. While TPEG funds could be used to close gaps in aid packages for TEXAS Grant recipients, the funding source (statutory tuition) was unaffected by deregulation with no variation across institutions. The Federal Pell Grant program awarded nearly \$438 million to 135,623 students in Texas's public universities in 2009 (THECB 2010b). While Pell amount eligibility does increase with the cost of attendance (which depends on tuition), in practice many students

[5] Online Appendix Table A1 presents several measures of the TEXAS Grant program, such as number of recipients, award amounts, and expected family contribution (EFC) distribution (within our sample), over time. Funding increased considerably as the maximum and average awards increased, while the composition of students shifted to be slightly more needy.

[6] The TEXAS Grant program is somewhat unique among states. A few states (e.g., Virginia, Colorado) also allocate funds to institutions, which then pass them through to students with some degree of autonomy. Many other states (e.g., California, Minnesota, New York, South Carolina) directly determine awards, removing the institutions from decision making. Tuition set-asides exist in a number of states, requiring institutions to use revenue dollars to fund grant aid in order to offset the effects of tuition increases on poor students. The particular set-aside in Texas, requiring that grant aid for recipients of TEXAS grants cover the full cost of tuition and fees, is more generous than most other states. Some states require that students bear at least some of the cost of tuition and fees (e.g., Minnesota), while others do not address students' unmet need net of grant aid (e.g., California, Illinois, Minnesota). See Baum et al. (2012) for more details.

already receive the federal maximum, so tuition increases are unlikely to increase Pell awards.

These programs together represent a considerable investment in making college affordable for low-income students. The HB 3015 set-asides and TEXAS Grant, in particular, allow the financial aid packages for low-income students to accommodate price increases by tying need-based aid dollars directly to tuition levels.

### C. *Prior Literature*

Prior research has established that there are returns to a college education, even among academically marginal students (Zimmerman 2014). The benefits of a college degree are quite heterogeneous, however, as students who attend better-resourced colleges are both more likely to graduate (Cohodes and Goodman 2014) and have higher earnings (e.g., Hoekstra 2009; Andrews, Li, and Lovenheim 2016; Chetty et al. 2017). Furthermore, there are substantial earnings differences across majors (Hastings, Neilson, Zimmerman, 2013; Kirkebøen, Leuven, and Mogstad 2014), with earnings differences across majors comparable to the earnings gap between high school and college graduates (Altonji, Blom, and Meghir 2012). This suggests that higher education could either narrow or widen economic inequalities depending on the nature of the institutions and programs attended by low-income and non-poor students.

Price (sticker and net) is one factor that prior evidence has demonstrated is closely linked to college enrollment, institutional choice, and persistence (Dynarski 2000; Long 2004; Hemelt and Marcotte 2011; Jacob, McCall, and Stange 2018; Castleman and Long 2016). Stange (2015) found that higher sticker prices for engineering and business is associated with fewer degrees granted in these fields, particularly for women and minorities. However, his analysis examined differential pricing generally (not just due to deregulation) and could not determine whether increased aid or supply-side factors could mitigate any adverse effects of higher price.

Furthermore, prior work has produced mixed evidence on whether tuition is actually higher when public universities have more autonomy (Lowry 2001, Rizzo and Ehrenberg 2004), and this work doesn't examine effects on students. The only exception is Flores and Shepard (2014), who found that at seven Texas institutions, institution-level price accelerated following deregulation, but effects on enrollment of underrepresented minority students was mixed, with increased representation by black students but reductions for Hispanic students. Pell Grant recipients increased their college enrollment rates following deregulation.

A small number of studies have directly examined price discrimination by higher education institutions and its implications for poor students. Using a structural equilibrium model of the college market, Fillmore (2014) finds that reducing institutions' ability to price discriminate lowers prices for middle- and high-income students but raises prices for low-income students, pricing some of them out of elite institutions. Price discrimination is thus beneficial to low-income students. Epple, Romano, and Sieg (2006) also finds that price discrimination significantly affects the equilibrium sorting of students into colleges, though they do not assess differential effects by income directly. Finally, Turner (2014) finds that institutions' price discrimination behavior reveals a willingness to pay for Pell Grant students, particularly at public institutions. Public institutions actually crowd in institutional aid for students receiving the Pell Grant.

## II. Data Sources and Sample

### A. *Student Data and Sample*

Administrative data from the Texas Education Agency (TEA), the THECB, and the Texas Workforce Commission (TWC) are combined to form a longitudinal dataset of all graduates of Texas public high schools from 2000–2009. The data is housed at the University of Texas (UT) at the Dallas Education Research Center.[7]

TEA data include information on students' socioeconomic disadvantage during high school, high school achievement test scores, race, gender, date of high school graduation, and high school attended.[8] Information on college attendance, major in each semester, college application and admissions, and graduation is obtained for all students attending either a community or public four-year college or university in Texas from the THECB. We categorize students as poor based on eligibility for free or reduced-price lunch in (FRPL) twelfth grade, though this also includes students whose family income is at or below the federal poverty line, are eligible for Temporary Assistance for Needy Families (TANF), Pell recipients, Title II eligible, or eligible for food benefits under the Food Stamp Act of 1977. Finally, we obtain quarterly earnings for all students residing in Texas from the TWC, which are drawn from state unemployment insurance (UI) records. Thus, we expect them to be measured with little error, though they only include students who remain in the state of Texas and are covered by UI.[9]

Our main analysis focuses on the choice of first program among students who enroll in a four-year public Texas university within two years of high school graduation. We assign students to the first four-year institution they attend and to the first declared major. Students whose first major is "undeclared" are assigned the first non-undeclared major in their academic record. Students who drop out without ever declaring a major are coded as "liberal arts." Some analysis also includes the full sample of Texas high school graduates. Finally, we drop some individuals with missing values for key covariates. Our final analysis sample includes 1,861,500 unique high school graduates, 580,253 of whom enroll in a Texas pubic four-year college within two years.

Table 1 presents characteristics of these samples. Approximately 30 percent of the full sample (19 percent of the college sample) is economically disadvantaged ("poor"). The middle rows of Table 1 describe the nature of the first program attended by students in our sample. As we describe in more detail later, we rank programs according to the average log earnings of enrollees relative to students who did not attend a public college in Texas. Poor students are underrepresented

[7] We restrict attention to cohorts from 2000 onward because key information about tuition, financial aid, application and admissions, and program resources are only available from 2000 onward.
[8] High school exit exam scores are standardized to mean 0 and standard deviation 1 separately by test year, subject, and test type (as the test changed across cohorts) among all test takers in the state.
[9] Andrews, Li, and Lovenhiem (2016) finds that coverage in the earnings records is quite good.

Table 1—Summary Statistics of Main Student Samples

| | All high school graduates | | Four-year college enrollees | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | All students | | Poor students | | Non-poor students | |
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| Male | 0.482 | 0.500 | 0.451 | 0.498 | 0.423 | 0.494 | 0.458 | 0.498 |
| Black | 0.121 | 0.327 | 0.119 | 0.324 | 0.213 | 0.410 | 0.098 | 0.297 |
| White | 0.512 | 0.500 | 0.582 | 0.493 | 0.119 | 0.323 | 0.689 | 0.463 |
| Hispanic | 0.326 | 0.469 | 0.235 | 0.424 | 0.611 | 0.487 | 0.148 | 0.355 |
| Asian | 0.038 | 0.191 | 0.061 | 0.239 | 0.055 | 0.229 | 0.062 | 0.242 |
| Math test | 0.007 | 0.994 | 0.465 | 0.764 | 0.200 | 0.848 | 0.526 | 0.730 |
| English test | 0.014 | 0.984 | 0.423 | 0.644 | 0.163 | 0.771 | 0.483 | 0.595 |
| Poor | 0.289 | 0.453 | 0.188 | 0.391 | 1.000 | 0.000 | 0.000 | 0.000 |
| Characteristic of first four-year program | | | | | | | | |
| Predicted log earnings | 0.079 | 0.169 | 0.241 | 0.216 | 0.174 | 0.200 | 0.257 | 0.216 |
| Not enrolled | 0.688 | 0.489 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Top 10% | 0.031 | 0.172 | 0.097 | 0.295 | 0.052 | 0.222 | 0.107 | 0.309 |
| Top 15% | 0.042 | 0.201 | 0.134 | 0.340 | 0.076 | 0.265 | 0.147 | 0.354 |
| Top 20% | 0.062 | 0.241 | 0.189 | 0.391 | 0.111 | 0.315 | 0.207 | 0.405 |
| Top 25% | 0.076 | 0.265 | 0.231 | 0.421 | 0.142 | 0.349 | 0.252 | 0.434 |
| Bottom 25% | 0.083 | 0.275 | 0.260 | 0.439 | 0.359 | 0.480 | 0.238 | 0.426 |
| Bottom 20% | 0.065 | 0.246 | 0.204 | 0.403 | 0.277 | 0.448 | 0.187 | 0.390 |
| Bottom 15% | 0.049 | 0.215 | 0.156 | 0.362 | 0.200 | 0.400 | 0.145 | 0.352 |
| Bottom 10% | 0.032 | 0.175 | 0.101 | 0.301 | 0.137 | 0.344 | 0.093 | 0.290 |
| Tuition (in-state junior, 15 credits, $1,000) | | | 2.844 | 0.776 | 2.623 | 0.746 | 2.894 | 0.774 |
| Faculty salary per student ($1,000) | | | 2.886 | 11.325 | 2.961 | 13.517 | 2.870 | 10.770 |
| Need-based grant aid ($1,000) | | | | | | | | |
| Total | | | 0.941 | 1.616 | 2.480 | 1.965 | 0.584 | 1.283 |
| Pell | | | 0.452 | 0.829 | 1.332 | 0.990 | 0.249 | 0.631 |
| HB 3015 | | | 0.043 | 0.208 | 0.073 | 0.272 | 0.036 | 0.189 |
| TEXAS Grant | | | 0.335 | 0.795 | 0.872 | 1.107 | 0.210 | 0.642 |
| TPEG | | | 0.080 | 0.255 | 0.129 | 0.307 | 0.069 | 0.241 |
| SEOG | | | 0.019 | 0.104 | 0.052 | 0.168 | 0.011 | 0.081 |
| Tuition − need grant ($1,000) | | | 1.900 | 1.833 | 0.096 | 2.014 | 2.307 | 1.517 |
| Observations | 1,861,500 | | 580,253 | | 109,070 | | 471,183 | |

*Notes:* Sample includes all high school graduates from public Texas high schools (first two columns) who enrolled in a Texas public four-year college or university within two years of high school graduation (last six columns). Poor indicates eligible for free or reduced-price lunch, family income is at/below the federal poverty line, TANF eligible, Pell recipients, Title II eligible, or eligible for food benefits under the Food Stamp Act of 1977. SEOG stands for the Federal Supplemental Educational Opportunity Grant. TEOG stands for the Texas Educational Opportunity Grant. TPEG stands for the Texas Public Educational Grant. HB 3015 stands for the designated tuition grants associated with HB 3015. Test scores are normalized by year for all takers. Dollar amounts (tuition, grant aid, faculty salary) are per semester and in 2012 dollars. Predicted log earnings and earnings rank of program attended is estimated with equation (1) using 2000–2002 cohorts and applied to all cohorts (see text).

*Source:* Author calculations

among the "top" earnings programs and overrepresented among the lower earning programs. Poor students also attend programs that have lower tuition levels than non-poor students.

We are able to measure need-based grant aid (and thus net price) in students' first year using micro data compiled by the THECB. This micro data consistently contains financial aid award information for all students who both receive need-based aid and are enrolled in a Texas public institution from 2000–2011. We divide this amount in half to convert it to a semester equivalent. Unfortunately, aid received by students who did not perform a needs assessment is not consistently included in the database over time, so we are unable to create measures of net price that incorporate non-need-based aid, such as merit and some categorical grant aid.[10] The bottom of Table 1 describes the need-based grant aid received by students in our sample. Unsurprisingly, poor students receive much larger amounts of need-based grant aid than non-poor students, nearly $2,500 per semester, most prominently the federal Pell Grant ($1,330), TEXAS Grant ($870), and TPEG ($130). Average aid from the HB 3015 set-aside is small ($70), though this is misleading as these grants are mechanically 0 prior to deregulation. Net tuition for poor students is very close to 0 due to need-based grant aid alone.[11]

## B. *Program-Level Data and Sample*

To track changes in price following deregulation, we have assembled information on tuition and fees for each public university in Texas since 2000 separately by major/program, credit load, entering cohort, residency, and undergraduate level. This level of granularity is critical, as many institutions adopted price schedules that vary according to all of these characteristics, and no prior source of data captures these features.[12] Our main price measure is the price faced by in-state juniors taking 15 credit hours, which is the minimum number of credits students would need to take in order to graduate within four years.[13] We convert all tuition prices and spending measures to real 2012 dollars using the consumer price index (CPI).

To measure program-level resources, we use administrative data on both the course sections offered and faculty in each department at each institution since 2000. We construct various measures of resources (faculty salary per student, average class size, faculty per student, average faculty salary) for each program in each year before and after deregulation, measured in the fall. Since the breadth of academic programs vary by institution, we standardize them using two-digit Classification of Institutional Program (CIP) codes, separating economics and nursing from their larger categories (social science and health professions, respectively) as they are sometimes housed in

[10] The target sample for the Financial Aid Database (FAD) expands over time. From 2000–2006, the database includes only students who received any type of need-based aid, or any type of aid that requires a need analysis. From 2007–2009, the database includes students who were enrolled and completed either a free application for federal student aid (FAFSA) or Texas application for state financial aid (TASFA), some of whom may not have received any aid. Since 2010, the database was expanded to also include students who did not apply for need-based aid but received merit or performance-based aid. In order to keep our measures of aid consistent, we first identify students who received a positive amount of grant aid from at least one need-based aid program (Pell, SEOG, TEXAS Grant, TPEG, or HB 3015). Any student who did not receive grant aid from one of these programs or who was not matched to the FAD database is assumed to have zero need-based grants. The number of students with a positive amount of grant aid from one of these sources is relatively constant at about 21,000 students per high school cohort.

[11] As a robustness check, we also examine grants from other sources received by need-eligible students (including categorical aid and merit-based aid). Including these does not alter our estimates much. These items are not consistently available for students who did not also have a needs assessment done.

[12] This information was assembled from various sources, including university websites, archives, and course catalogs. Kim and Stange (2016) describes the price data in more detail.

[13] Unfortunately prices are only available for credit loads of 9, 12, and 15, so we are not able to construct price for the average credit load. Nonetheless, using tuition price for a different credit load will rescale our price estimates but have no substantive impact on our analysis.

units that price differently. We restrict our analysis to programs (defined by two-digit CIP codes) that enroll at least one student from each high school cohort from 2000–2009. Our final program-level sample includes 641 programs tracked over 10 years, for a total sample size of 6,410.[14] A description of how the program-level resource measures were constructed is included in online Appendix B. The average program spends nearly $3,000 on faculty salary per student, pays its main instructor $30,500 per semester, and has about 30 students per course section.

### C. *Program-Level Earnings*

We characterize each program at each institution by the average post-college earnings of its enrollees prior to deregulation, controlling for student selection into particular majors. For all individuals who both graduated from a public high school in Texas from 2000–2002 and were observed working in the state ten years later, we estimate

$$LogEarnings_{ijk} = \beta_0 + \gamma_{jk} + \beta_1 CommColl_i + \beta_2 \mathbf{X}_i + \varepsilon_{ijk}, \tag{1}$$

where $\gamma_{jk}$ is a full set of fixed effects for each program (major $j$ and institution $k$) and $CommColl_i$ denotes students who enroll in a community college but do not transfer to a four-year institution within two years. The term $\mathbf{X}_i$ is a vector of student characteristics: achievement test scores, race/ethnicity, limited English proficient, and economically disadvantaged. The outcome $LogEarnings_{ijk}$ is the average log quarterly earnings residual for person $i$ ten or more years after high school graduation, after netting out both year and quarter fixed effects. The set of program fixed effects provides an estimate of the average earnings of each program (relative to the earnings of high school graduates who did not attend public higher education in Texas) purged of any differences in student characteristics. Since we focus on initial (rather than final) program, estimates of $\gamma_{jk}$ should be interpreted as the ex ante expected returns from enrolling in each program, which include any earnings effects that operate through changes in the likelihood of graduating.

Figure 2 shows how program earnings vary by field and institution.[15] Students in engineering, business, math, and nursing programs typically have the highest earnings. For example, students in the median engineering program in the state experience earnings gains three times as large as the gains experienced by students in the median biology program. Earnings are also highest at the state's research institutions, though again there is variation across programs within the same institution. Seven of the top ten programs with the highest predicted earnings are at Texas A&M and the University of Texas at Austin. Programs associated with the lowest earnings are mainly from less selective institutions and include visual/performing

[14] We exclude programs that are introduced or discontinued during our analysis window or that have a very small number of students. In practice, this restriction drops fewer than 5 percent of the student sample across all cohorts.

[15] Online Appendix Figure A4 shows the distribution of predicted program-level earnings, weighted by enrollment in 2000. Online Appendix Figure A5 depicts the median program earnings for each field and institution with different sets of controls. The ranking of fields and institutions by earnings are generally not sensitive to the controls used.

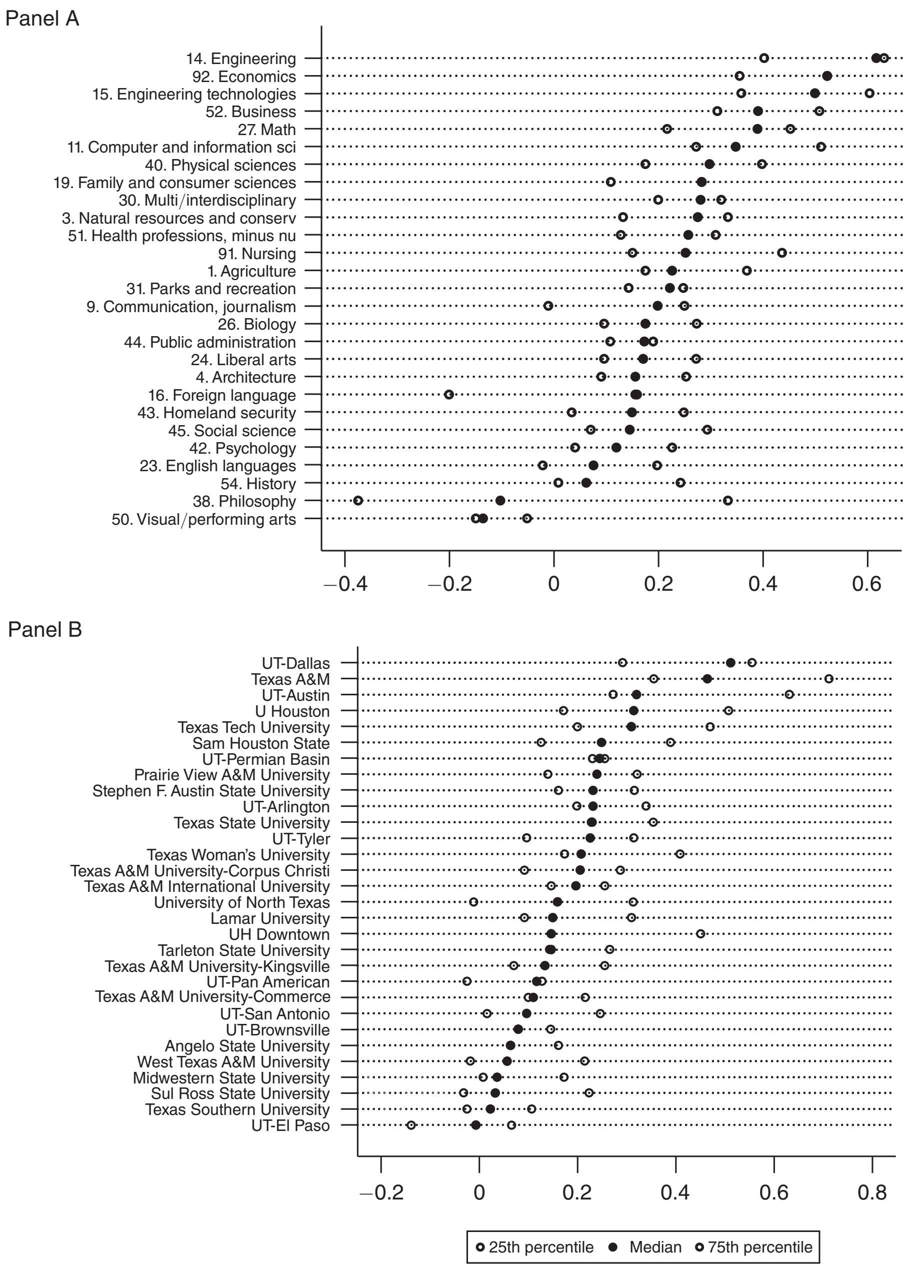


Figure 2. Predicted Earnings by Field and Institution, 2000

*Notes:* The full sample includes 641 programs, though this graph omits 68 programs that have fewer than 5 students enrolled from the 2000 cohort and also does not display any fields or institutions with fewer than 10 observations. The reference group consists of Texas high school students who do not attend any Texas public four-year university within two years of high school graduation. Programs are weighted by number of enrollees from 2000 cohort when computing twenty-fifth, fiftieth, and seventy-fifth percentiles.

arts, English language, and the social sciences (excluding economics). Since labor market outcomes vary so much across programs, disparities in program access could impact economic inequality.

To characterize choices among the 641 programs more easily, we assign each program to one of 20 quantiles based on the program's predicted student earnings impact. Since quantiles are constructed with student-level data, each ventile accounts for approximately 5 percent of all enrollment.[16] An additional benefit of grouping programs into equally sized ventiles is that this accounts for size differences across programs that can make interpretation difficult.

## III. Sticker Price Changes

The direct effect of deregulation was to induce substantial price increases for public bachelor's degree programs in Texas. Panel A of Figure 3 presents event-study estimates, comparing the post-deregulation growth in sticker price for programs in the top versus bottom quartile of predicted earnings. While the price of both is growing prior to deregulation (consistent with national trends), the sticker price jumps immediately following deregulation, particularly for the most lucrative programs. Panel B plots ventile-specific price changes, with the bottom ventile omitted and serving as the reference category. Indeed, the price increase was largest for the most lucrative programs. Programs in the top half of the earnings distribution all increased tuition by a larger amount than those in the lower half, with particularly large increases among the top 15 percent of programs, which increased tuition by more than \$400. Similarly, large increases were also seen in ventile 12, which includes the University of Texas at Austin liberal arts program. This is a large increase relative to the overall average tuition of \$2,160 prior to deregulation. We also estimate models that interact $Post_t$ with the predicted earnings for program $jk$.[17] Programs with high predicted earnings (1 log point) increased their tuition price by \$728 more than those whose enrollees earn no more than high school graduates. We also let high returns programs have a different initial and post-deregulation growth rate. Price increased immediately post-deregulation for the most lucrative programs (by \$441), and also grew at a faster rate (\$57 more per year, though insignificant) following deregulation relative to the preexisting trend.

## IV. Did Student Sorting Change Following Deregulation?

### A. *Assessing Changes in Student Sorting*

Table 1 demonstrated that poor students are overrepresented in programs in the bottom earnings quartile and are much less likely to enroll in one of the more lucrative programs. To assess how deregulation altered the distribution of programs attended by poor and non-poor students, we estimate models of the form

[16] Table A3 in the online Appendix lists the specific programs with the highest and lowest earnings gains, while Table A4 lists the specific programs contained in each ventile.

[17] These results are reported later in Table 6.

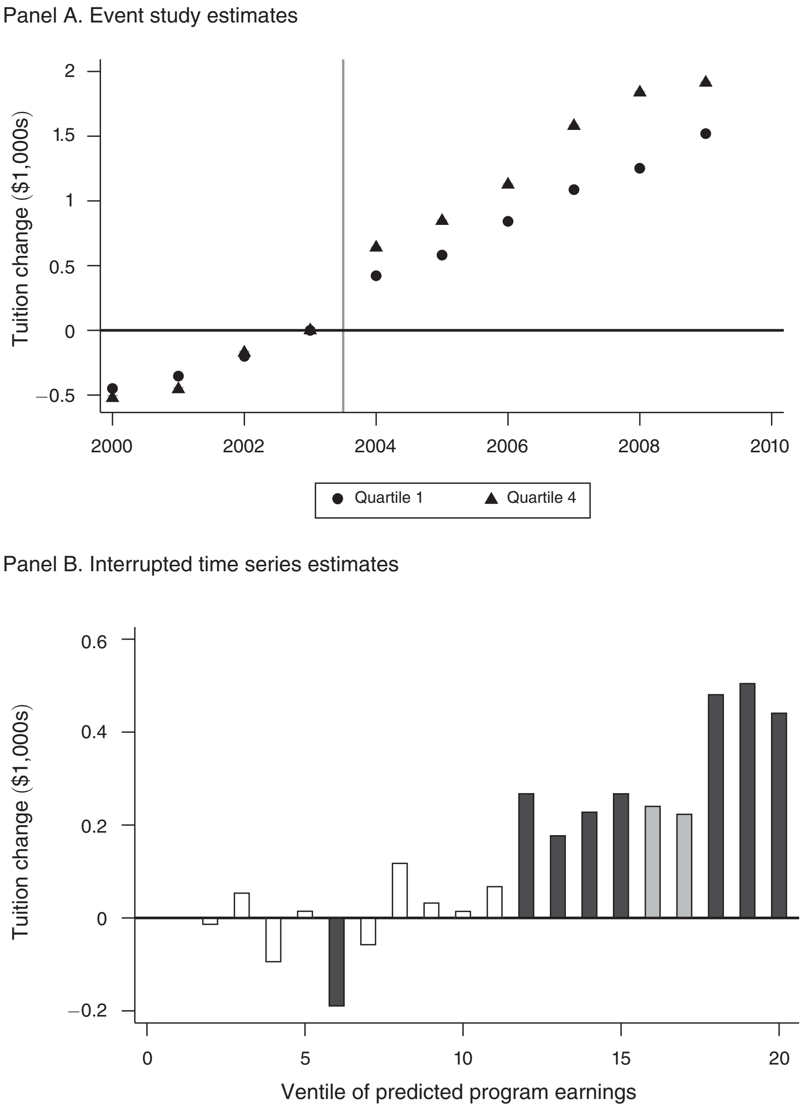


Figure 3. Sticker Price Change Post-deregulation, by Program Earnings (in-state juniors, 15 semester credit hours, fall)

*Notes:* Panel A plots the change in sticker price (per semester) relative to 2003 separately for programs in the top and bottom quartile of the predicted earnings distribution. We regress sticker price on year fixed effects (omitting 2003) and program fixed effects, weighting by enrollment, separately for programs in the top and bottom quartile of the earnings distribution. The figure plots the year fixed effects. Panel B plots the change in sticker price (per semester) following deregulation by predicted earnings ventile, estimated by the coefficient on the interaction between a post indicator and indicators for each ventile. Bottom ventile is omitted and serves as a reference category. Black bars are significant at a 5 percent level and gray bars are significant at a 10 percent level. All models include program fixed effects. The full sample includes 643 programs over 10 years, though analysis sample is smaller due to missing data. Standard errors are clustered by program.

$$Outcome_{jk(it)} = \beta_0 + \beta_1 Poor_{it} + \beta_2 Post_t \times Poor_{it} + \beta_3 Time_t + \beta_4 Post_t + \beta_5 \mathbf{X}_{it} + e_{it}, \tag{2}$$

where $Outcome_{jk(it)}$ captures the earnings potential of the program (major $j$ at institution $k$) in which individual $i$ from cohort $t$ enrolled. While we initially examine indicators for college enrollment, our primary outcome is $PredEarn_{jk(it)}$, the predicted earnings of the program chosen by individual $i$ in cohort $t$. The coefficient $\beta_1$ measures the difference in the earnings potential of programs entered by poor versus non-poor students prior to deregulation. Our main parameter of interest is $\beta_2$, the differential change in average predicted earnings of the programs attended by poor students relative to non-poor students following deregulation. To describe where in the distribution of programs changes occur, we also estimated models with the outcome $VentQ_{jk(it)}$, an indicator for individual $i$ in cohort $t$ enrolling in a program $jk$ whose predicted earnings place it in the $Q$th ventile. For instance, $Vent20_{jk(it)}$ indicates enrollment in programs that have the highest 5 percent (enrollment weighted) of predicted earnings. In this case, $\beta_2$ captures any differential change in the likelihood of poor students enrolling in such programs relative to non-poor students following deregulation. To account for differential changes in the characteristics of poor and non-poor students, we control for achievement test scores, race/ethnicity, and whether the student is limited English proficient. Models that include a set of cohort fixed effects in place of the linear time trend and $Post_t$ dummy are quite similar, so we mostly focus on the more parsimonious specification. To account for the possibility that state-wide shocks may affect all students making college choices at the same time, we cluster standard errors by high school cohort.[18]

To interpret our estimates as the causal effect of deregulation on the sorting of students across programs, we require that there be no trends or simultaneous policy changes that differentially affect poor versus non-poor students and more versus less lucrative programs following deregulation. State-wide economic shocks or broad initiatives to increase post-secondary participation among all students will be absorbed by year fixed effects or time trends and are not a source of bias. However, delayed effects of other policies such as the Top 10 Percent rule or targeted scholarship and recruitment policies, for example, the Longhorn Scholars program at UT–Austin, could potentially confound our estimates of the effects of deregulation.

To address this issue, we also estimate event-study models. These models include an indicator for poor, poor interacted with a set of cohort fixed effects (omitting 2003), and a full set of cohort fixed effects and individual controls:

$$Outcome_{jk(it)} = \beta_0 + \beta_1 Poor_{it} + \sum_{c=2000}^{2009} \beta_c \mathbf{1}(Cohort = c) \times Poor_{it} + CohortFE_t + \beta_5 X_{it} + e_{it}. \tag{3}$$

[18] Other methods of clustering produce similar levels of inference. Our main estimates have $p$-values of 0.09 or lower if we instead cluster by cohort $\times$ poor or institution or use block or wild-bootstrap procedures (Cameron, Gelbach, and Miller 2008). These results are reported in online Appendix Table A5.

Table 2—Effect of Deregulation on College Enrollment and Program Choice

| | Attend any public Texas college or university (mean = 0.504) | | Attend four-year public college in balanced program (mean = 0.26) | | Program choice: log(predicted earnings) of first program | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Poor | −0.137 | −0.0860 | −0.153 | −0.0745 | −0.0604 | −0.0861 | −0.0256 | −0.0370 |
| | (0.00202) | (0.00277) | (0.00297) | (0.00330) | (0.00199) | (0.0018) | (0.00175) | (0.0019) |
| Post | 0.0159 | 0.0148 | 0.00377 | 0.000114 | 0.000306 | −0.0029 | −0.00115 | −0.0060 |
| | (0.0287) | (0.0295) | (0.0223) | (0.0246) | (0.00759) | (0.0066) | (0.00805) | (0.0091) |
| Post × Poor | −0.00769 | −0.00571 | −0.000780 | 0.00646 | 0.00103 | 0.0057 | 0.00424 | 0.0129 |
| | (0.00390) | (0.00379) | (0.00401) | (0.00406) | (0.00240) | (0.0023) | (0.00224) | (0.0018) |
| Controls | No | Yes | No | Yes | No | No | Yes | Yes |
| Sample | All HS grads | All HS grads | All HS grads | All HS grads | All HS grads | 4-year enrollees | All HS grads | 4-year enrollees |
| Outcome mean | 0.504 | 0.504 | 0.26 | 0.26 | 0.079 | 0.241 | 0.079 | 0.241 |
| Observations | 1,861,500 | 1,861,500 | 1,861,500 | 1,861,500 | 1,861,500 | 580,253 | 1,861,500 | 580,253 |
| $R^2$ | 0.018 | 0.046 | 0.022 | 0.122 | 0.026 | 0.0223 | 0.113 | 0.1205 |

*Notes:* Linear regression models include indicator for poor, post-deregulation, the interaction between post-deregulation and poor, and time (linearly). Controls include gender, race/ethnic indicators and indicator for limited English, and scaled reading and math scores. Sample includes all students in the high school classes of 2000–2009 from public high schools in Texas. College enrollment is measured within two years of high school graduation. Students that attend both two-year and four-year colleges are counted as four-year college attendees. Balanced program refers to the 643 programs that have nonzero enrollment during the sample period. Outcome for the third panel is the log predicted earnings of the university program (institution × major) the student first enrolled in, which is estimated with equation (1) using 2000–2002 cohorts and applied to all cohorts (see text). Standard errors are clustered by high school cohort.

The coefficients, $\beta_c$, can be interpreted as the change in poor student representation relative to non-poor students in $c$ relative to the year prior to deregulation (2003). For $c = 2000$, 2001, and 2002, these coefficients measure any pre-trends in the outcomes that couldn't possibly be due to deregulation. Whether these pre-deregulation coefficients are equal to 0 provides a suggestive test of the main assumption of specification (2) that allows for a causal interpretation.

### B. *Overall Enrollment and Initial Program Choice*

Before examining program choice, we first examine whether deregulation is associated with overall changes in college enrollment. These results are shown in the first four columns of Table 2. We see little effect of deregulation on students' likelihood of attending any public college in Texas (including community colleges) or any four-year public institution after controlling for a simple linear time trend, with or without other controls.[19] Deregulation does not appear to have affected overall college enrollment or students' choice between two-year and four-year institutions, given that the former was not subject to deregulation. Furthermore, we believe that changes in sample selection have little impact on our analysis of program choice.

The final two columns of Table 2 present our main results on choice of initial program for the entire sample of high school graduates (column 7) and the subset

[19] Results for any four-year public program and a public four-year program included in our analysis sample are quite similar, so we show the latter because this directly speaks to the importance of sample selection for our subsequent analysis on program choice.

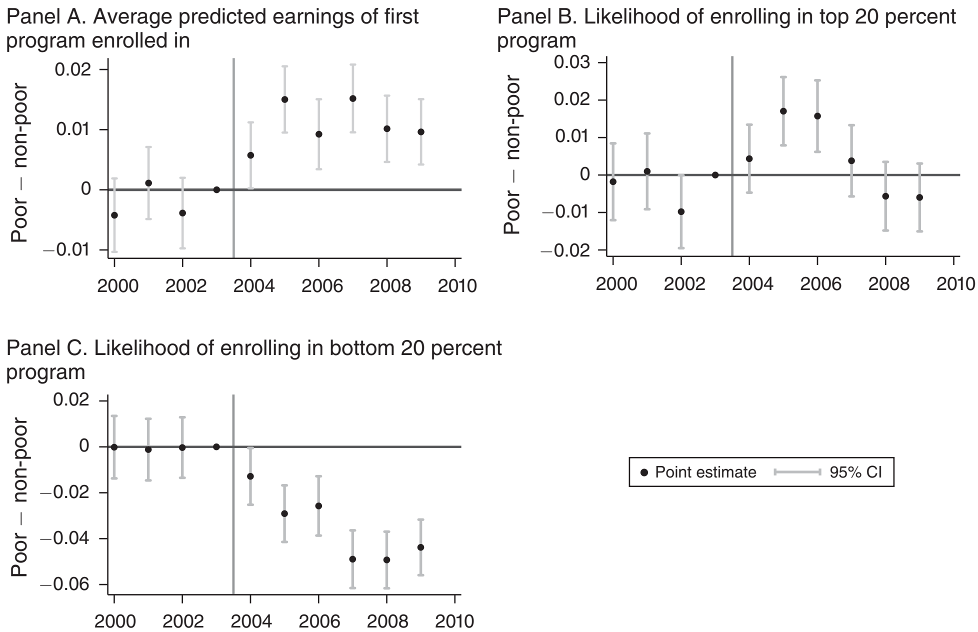


Figure 4. Event-Study Estimates

*Notes:* Figures plot the coefficients on the interactions between a poor indicator and indicators for each year, as described in equation (3). The year 2003 interaction is omitted and serves as the reference category. Model also includes a full set of year fixed effects, a dummy for poor, race/ethnic indicators, indicator for limited English, and scaled reading and math scores. Outcomes are predicted earnings of the university program in which the student first enrolled (panel A) and indicators for this program being in the top (panel B) or bottom (panel C) 20 percent of predicted student earnings. Standard errors are clustered by high school cohort.

of students that enrolls in four-year colleges (column 8). On average, poor students enter programs that generate earnings gains 3.7 percent lower than non-poor students, after controlling for demographics and achievement test scores. This gap closes by more than one-third following deregulation. Estimates are still positive but attenuated when we include all high school graduates (including non-attendees) in Table 2, column 7.[20] Results are directionally similar, though weaker and less precise, when we do not control for changes in student characteristics.

Figure 4 presents event-study estimates as described in equation (3). There is no noticeable trend in average program earnings of poor relative to non-poor students leading up to deregulation, but a noticeable and persistent uptick afterward (Figure 4, panel A). Similarly, we see no preexisting trends in the difference between poor and non-poor students in the likelihood of enrolling in a top 20 percent or bottom 20 percent program (Figure 4, panels B and C), but clear shifts following deregulation. This gives us confidence that our interrupted time-series estimates are not merely

[20] Since non-attendees are the baseline group against which earnings are compared, these students all receive a 0 for the predicted earnings outcome. Including them in the analysis (with no detectable change in behavior) attenuates the overall effect toward 0. The point estimate in (7) is very close to what would be expected with no behavioral response from non-enrollees: $(0) \times (68.8 \text{ percent non-enrollees}) + (0.0129) \times (31.2 \text{ percent enrollees}) = 0.00402$.

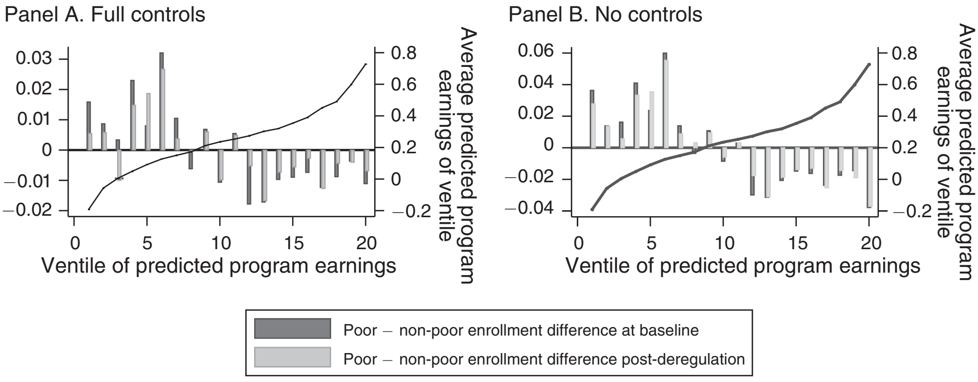


Figure 5. Initial Difference and Change in Enrollment of Poor versus Non-poor Students across Programs

*Notes:* Estimates in each panel come from 20 separate regressions of indicators for enrolling in a program in each ventile on a dummy for *Poor*, *Post* × *Poor*, *Time* (linearly), *Post*, and student controls (panel A only), as described in equation (2). Dark bars plot the coefficient on *Poor*. Light bars plot the coefficients on the *Post* × *Poor* interaction added to the coefficient on *Poor*. Standard errors are clustered by high school cohort. Solid line plots the average predicted log earnings of programs in each ventile (right axis).

picking up the effects of preexisting trends. The gains come from a clear relative movement of poor students away from the least lucrative programs—a reduction of 3.5 percentage points in the relative likelihood of enrolling in a bottom quintile program. Some of this movement may be to programs in the top quintile, though the magnitude does depend on controls for student test scores. There is no evidence that the representation of low-income students declined in top programs following deregulation. Estimates without controls (available from the authors) are qualitatively similar, though more noisy. Online Appendix Figure A8 shows these trends in levels for poor and non-poor students (rather than the difference). While both groups experience similar trends prior to deregulation (toward less lucrative programs), poor students move to more lucrative programs in absolute terms, while the enrollment pattern of non-poor students is relatively more stable after deregulation.

Figure 5 examines student sorting across the whole distribution of programs. To better understand how earnings differ across this distribution, the figure plots the average predicted earnings for each ventile. Other than the tails, log predicted earnings is quite linear. Thus, even shifts in students across programs in the middle of the distribution will have important consequences for predicted earnings. The dark bars show that the unequal distribution of students across programs remains even after controlling for differences in student demographics and achievement test scores. Poor students are 1–2 percentage points more likely to enroll in programs in each of the bottom six ventiles and, consequently, much less likely to enroll in programs with medium to high predicted earnings. However, this pattern changed in the years following deregulation (light bars). Relative to non-poor students, poor students shift away from these low-earning programs after 2004 and make gains throughout the rest of the distribution. Large gains are seen particularly in ventile 12, which includes liberal arts at

UT–Austin, one of the largest programs in our data. However, important gains are made at many other programs with above-median earnings potential.[21]

### C. *Robustness and Alternative Explanations*

The broad pattern of sizable shifts away from the bottom of the distribution is remarkably robust to the inclusion of different student controls or alternative specifications, as shown in panel B of Figure 5 and in Table 3.[22] High school fixed effects account for the possibility that college goers are coming from different types of high schools before versus after deregulation in a way that correlates with program choice. We also control for application and admissions behavior by including a large set of indicators for all the Texas public universities to which the student applied and was accepted, which may pick up some unobservable student traits (Dale and Krueger 2002). Neither addition impacts our estimates, though we exclude these controls from our baseline for reasons of statistical power and interpretability, respectively.[23] Given the unimportance of controlling for these observed characteristics, this gives us confidence that the results may be robust to changes in unobserved characteristics as well.

In Table 3, columns 4–7, we systematically rule out several of the most well-known policies that might differentially affect poor versus non-poor students following deregulation. It's worth noting that most of these policies were enacted several years prior to deregulation, so they would only be a source of bias if they had delayed effects on the relative program enrollment of poor and non-poor students. Encouragingly, all of our main results are qualitatively (and often quantitatively) unaffected by these sample restrictions. Thus, we conclude that these other major policy shifts that altered the enrollment of low-income students are unlikely to explain the large shift we observe that coincides with tuition deregulation.

In column 4, we drop all students from the 110 high schools that participated in the Longhorn Opportunity Scholars (LOS) or Century Scholars (CS) programs that provided financial aid and enhanced support services for poor students attending UT–Austin and Texas A&M, respectively. Though these programs started in 1999 and 2000, respectively, delayed effects could be a source of bias since the LOS has been shown to have large impacts on attendance and completion at UT–Austin (Andrews, Imberman, and Lovenheim 2016). HB 1403, otherwise known as the Dream Act, granted in-state tuition to undocumented students in Texas and was associated with an increase in college enrollment among foreign-born noncitizen Latino/a students (Flores 2010). Specification (5) drops the small number of limited English proficient-classified students in our sample. This is an imperfect proxy

[21] Online Appendix Figure A6 shows raw histograms for poor and non-poor students in 2000 and 2008. The relative gains of poor versus non-poor students are driven both by shifts in where poor students enroll (e.g., away from the lowest earnings programs) and the enrollment choices of non-poor students.

[22] Online Appendix Figure A7 presents estimates for models with fewer or richer controls than our base model. The only place where controls alter the qualitative result is for programs at the very top of the distribution. Controlling for achievement test scores attenuates a negative shift at ventile 19 and turns a negligible change at the very top ventile into a sizable positive one when controls are included. Because of the importance of controls at these two ventiles, we are cautious about making strong conclusions about movements at the very top.

[23] Including controls for application and admission behavior may be "over-controlling" for the treatment of deregulation if one of the mechanisms is through students' application behavior.

Table 3—Effect of Deregulation on Predicted Earnings of Undergraduate Program Chosen

| | Base model: log(predicted earnings) college enrollees full controls (1) | Varying controls: Full + high school FEs (2) | Varying controls: Full + application and admissions (3) | Restricted sample to rule out other policies: Drop LOS/CS schools (4) | Drop LEP students (5) | Drop top 30% at each high school (6) | White students only (7) |
|---|---|---|---|---|---|---|---|
| Poor | −0.0370 | −0.0165 | −0.0182 | −0.0420 | −0.0372 | −0.0331 | −0.0657 |
| | (0.0019) | (0.0018) | (0.0015) | (0.0021) | (0.0019) | (0.0023) | (0.0019) |
| Post × Poor | 0.0129 | 0.0116 | 0.0073 | 0.0135 | 0.0124 | 0.0129 | 0.0109 |
| | (0.0018) | (0.0020) | (0.0017) | (0.0022) | (0.0019) | (0.0028) | (0.0023) |
| Observations | 580,253 | 580,253 | 580,253 | 534,366 | 569,664 | 306,645 | 337,721 |

| | Identifying poor students: Poor = always FRPL during high school (8) | Poor = Pell recipient (9) | Poor: Pell and EFC > 0 Very Poor: EFC = 0 (10) | Outcome = level of predicted quarterly earnings (11) |
|---|---|---|---|---|
| Poor | −0.0257 | −0.0386 | −0.0318 | −193.87 |
| | (0.0024) | (0.0009) | (0.0014) | (13.30) |
| Post × Poor | 0.0114 | 0.0142 | 0.0117 | 87.07 |
| | (0.0023) | (0.0017) | (0.0022) | (16.27) |
| Very Poor | | | −0.0476 | |
| | | | (0.0017) | |
| Post × Very Poor | | | 0.0173 | |
| | | | (0.0026) | |
| Observations | 580,253 | 580,253 | 580,253 | 580,253 |

*Notes:* Linear regression models include indicator for poor, post-deregulation, the interaction between post-deregulation and poor, and time (linearly). All models also include controls for gender, race/ethnic indicators and indicator for limited English, and scaled reading and math scores. Full sample includes students in the high school classes of 2000–2009 that enrolled in a Texas public university within two years of high school graduation. Outcome is the log predicted earnings of the university program (institution × major) the student first enrolled in, which is estimated with equation (1) using 2000–2002 cohorts and applied to all cohorts (see text). Outcome in column 11 is predicted earnings level, estimated similarly. Standard errors are clustered by high school cohort.

*Source:* Author calculations

for students most likely to be affected by HB 1403; citizenship status is not available in our data.

After 1998, the Top 10 Percent rule guaranteed admission to any public institution in Texas for residents who graduate in the top decile of their high school class and increased enrollment at the state's flagships (Daugherty, Martorell, and McFarlin 2012; Long, Saenz, and Tienda 2010). Though we do not possess high school grades (or rank), in specification (6) we drop students who scored in the top 30 percent of their high school on the high school exit exam. This restriction likely drops most students admitted under the Top 10 given the positive correlation between high school test scores and grades.[24] Finally, in Table 3, column 7, we restrict our sample only to white students, who should not benefit from the restoration of race-conscious admissions at UT–Austin in 2003.

[24] Tables A7 and A8 in the online Appendix show how the sample of institutions and majors chosen by our sample changes with this restriction. As expected, dropping students in the top 30 percent of each high school's exit exam score distribution greatly reduces the representation of UT–Austin and Texas A&M in the analysis sample (from 32 percent to 11 percent) and also reduces the share of students in engineering and biology (from 22 percent to 11 percent).

Our results are quite similar regardless of how we identify "poor" students in our sample, including persistent eligibility for free- or reduced-price lunch (FRPL), as suggested by Michelmore and Dynarski (2017), or with Pell Grant receipt. This is important, as we use Pell Grant receipt as a marker for poor in supplemental analysis when free or reduced-price lunch status is unavailable. Furthermore, in Table 3, column 10, we distinguish very poor students (EFC of 0) from moderately poor students (Pell eligible but EFC $> 0$). Though point estimates are larger for the poorest students in our sample, the share of the gap closed after deregulation was identical between these two groups. Gains are thus experienced by both the poorest and modestly poor students who attend four-year college. Finally, our results are qualitatively similar if we use the level of predicted earnings as our measure of program value added, where the level includes observations with 0 earnings (Table 3, column 11). Poor students are enrolled in programs with lower levels of expected earnings, but this gap closes quite a bit following deregulation. Though not shown, these results are also robust to the set of controls used to construct earnings estimates for each program.[25] We also performed our main analyses on a restricted sample of students who enrolled in a four-year university directly after high school. The results are quite similar, both qualitatively and quantitatively.

Our single-state analysis cannot account for any national trends or policy changes that alter the representation of poor students relative to non-poor students at high-earning programs and institutions. For instance, if poor students were making relative inroads at high-earnings programs around the country because of Pell Grant expansions, our Texas-specific estimates will overstate the gains experienced due to tuition deregulation. To address this, we complement our main analysis with a cross-state comparison between Texas and other states. We find that the difference in predicted earnings of four-year public institutions attended by Pell students and non-Pell students shrinks in Texas following deregulation, while actually widening modestly in other states. This analysis suggests that our main within-Texas comparison is not conflating deregulation with national trends. If anything, our results are strengthened by including other states as a comparison group. Simply put, Texas is unusual in having the poor–non-poor gap close following deregulation relative to other states that did not deregulate tuition.[26]

## D. *Medium-Term Outcomes*

One concern is that poor students may not ultimately benefit from initially attending better programs because they do not persist, graduate, or actually experience higher earnings. Table 4 investigates several of these medium-term outcomes. Column 2 reports sorting results for the program students attend in their third year after initial enrollment, where continuing enrollment and dropout are distinct

[25] The coefficient on *Post* $\times$ *Poor* in panel A is 0.0192, 0.0177, and 0.0112 when the earnings equation has no controls, only demographic controls, or full controls + application dummies, respectively. These are all significant at the 1 percent level and are quite similar to our base model estimate of 0.0129.

[26] This supplemental analysis is described in online Appendix C. The results are robust to various sets of control states, including using the synthetic control approach of Abadie, Diamond, and Hainmueller (2010).

Table 4—Effect of Deregulation on Medium-Term Outcomes

| | Dependent variable | | | | |
|---|---|---|---|---|---|
| | Predicted log earnings of program enrolled in first year (1) | Predicted log earnings of program enrolled in third year (2) | Graduate within six years of college entry (3) | Earnings percentile eight-years post HS grad (4) | Earnings percentile ten-years post HS grad (5) |
| *Panel A. Sample includes all four-year enrollees within two years* | | | | | |
| Poor | −0.0370 | −0.0556 | −0.1020 | −1.4513 | −1.5023 |
| | (0.0019) | (0.0020) | (0.0047) | (0.2780) | (0.1878) |
| Post × Poor | 0.0129 | 0.0121 | 0.0007 | 0.0206 | 0.1917 |
| | (0.0018) | (0.0025) | (0.0052) | (0.3150) | (0.4087) |
| Observations | 580,253 | 580,253 | 510,046 | 519,694 | 400,778 |
| *Panel B. Sample includes all Texas high school graduates* | | | | | |
| Poor | | | | −2.0358 | −2.1410 |
| | | | | (0.2123) | (0.2037) |
| Poor × Attend | | | | 0.2809 | 0.6931 |
| | | | | (0.1740) | (0.1527) |
| Post × Poor | | | | −0.9422 | −1.0933 |
| | | | | (0.2881) | (0.3576) |
| Post × Poor × Attend | | | | 0.6410 | 0.8941 |
| | | | | (0.2943) | (0.5159) |
| Observations | | | | 1,660,825 | 1,286,798 |

*Notes:* Linear regression models include indicators for poor, post-deregulation, the interaction between post-deregulation and poor, and time (linearly). All models also include controls for gender, race/ethnic indicators, indicator for limited English, and scaled reading and math scores. Sample includes students in the high school classes of 2000–2009 overall (panel B) and those who enrolled in a Texas public university within two years of high school graduation (panel A). Earnings and graduation models include fewer observations because the outcome is not available for later cohorts. Outcome in (1) and (2) is the log predicted earnings of the university program (institution × major) the student enrolled in (initially or at year three), which is estimated with equation (1) using 2000–2002 cohorts and applied to all cohorts (see text). Separate predicted earnings estimates are constructed for dropouts and continued enrollees for each program when looking at third-year program outcomes. Earnings percentiles are defined relative to high school graduates in the same high school cohort and include all quarterly earnings over the focal year. Zeros are included in calculating percentiles. Standard errors are clustered by high school cohort.

*Source:* Author calculations

outcomes for each program.[27] The patterns are quite similar to those for initial program enrollment (column 1). On average, poor students are in programs that generate earnings gains 5.6 percent lower than non-poor students 2 years after initial enrollment, after controlling for demographics and achievement test scores. This gap closes by more than one-fifth following deregulation. These results suggest that deregulation induces poor students to not only enter more lucrative programs but to also remain and persist in them.[28]

[27] We estimate predicted earnings for each program separately for students who are still enrolled and those that have dropped out, using a modified version of equation (1) that interacts each program dummy with whether the student is still enrolled in college after two years. Predicted earnings estimates are qualitatively similar to those that do not distinguish between continued enrollees and dropouts; students in engineering and business programs and at the most selective institutions have the highest post-college earnings among both persisting and non-persisting students. Students who persist through two years have higher earnings than those in the same programs that do not persist.

[28] Table A6 in the online Appendix shows that these results are also robust to the various sample restrictions.

Column 3 examines the likelihood of graduating within six years of college entry.[29] Estimates are very imprecise zeros, but directionally consistent with our conclusion that deregulation is not associated with reduced attainment. Finally, in Table 4, columns 4 and 5, we examine whether deregulation is associated with an improvement in the relative position of poor students in the earnings distribution following high school and college. We calculate earnings percentiles relative to high school graduates in the same high school cohort and include all in-state quarterly earnings over the focal year, including quarters with 0 earnings. Examining actual earnings raises a number of challenges, so we view analysis of this outcome with caution.[30] Nonetheless, poor students modestly closed some of the gap in their earnings rank relative to non-poor students following deregulation. One particular concern is that any long-run trends affecting poor versus non-poor workers in the labor market in the years following deregulation may confound our estimates. To address this, Panel B of Table 4 presents an estimate for which we use non-attendees to control for such a trend. These estimates are even larger, though also imprecise. That is, the poor versus non-poor gap in earnings widens for those who do not attend four-year college, but poor college attendees are mostly shielded from this trend. While these medium-term outcomes are noisy, they point in the direction of poor students who attend four-year colleges being slightly better off following deregulation.

## V. Possible Channels

### A. *Price Mechanisms*

To address concerns that tuition increases would burden low-income students, 15 percent of incremental tuition revenue generated by deregulation was required to be set aside for need-based grant aid administered by the institutions. More price discrimination—a higher sticker price combined with more aid for low-income students—could potentially increase the representation of low-income students in more costly programs by lowering net price.[31] Figure 6 demonstrates the extent of income-based price discrimination before and after deregulation.[32] Each panel plots the poor versus non-poor difference in need-based grant receipt each year, with the gap normalized to 0 in the year before deregulation. Since poor and non-poor students face the same sticker price for each program, differences in grant aid map directly to price discrimination. Poor students experience a large increase in total

[29] Unfortunately, we lose the last two cohorts of our sample when looking at six years after initial enrollment.

[30] Specifically, (i) coverage is incomplete for later cohorts; (ii) earnings at young age may not fully reflect long-run outcomes; (iii) using actual earnings as an outcome raises a whole host of issues related to differential selection into the earnings sample; and (iv) outcomes that are quite distant from the policy change we are exploring may be more susceptible to other influences.

[31] Approximately half of all programs have a poor student share that is 15 percent or lower. These programs should be able to perfectly offset tuition increases with additional grant aid for poor students via the 15 percent HB 3015 set-asides, keeping net price for poor students constant or even lower. TEXAS Grants can be used to offset tuition increases even further. Institutional discretion means that the offsets we find may not reflect this theoretical ideal in practice.

[32] The following financial aid results should be interpreted cautiously, however, as data limitations require us to exclude non-need-based aid, which disproportionately benefits non-poor students. There is no specific provision of deregulation that would cause merit aid to change following deregulation, but we cannot entirely rule this out.

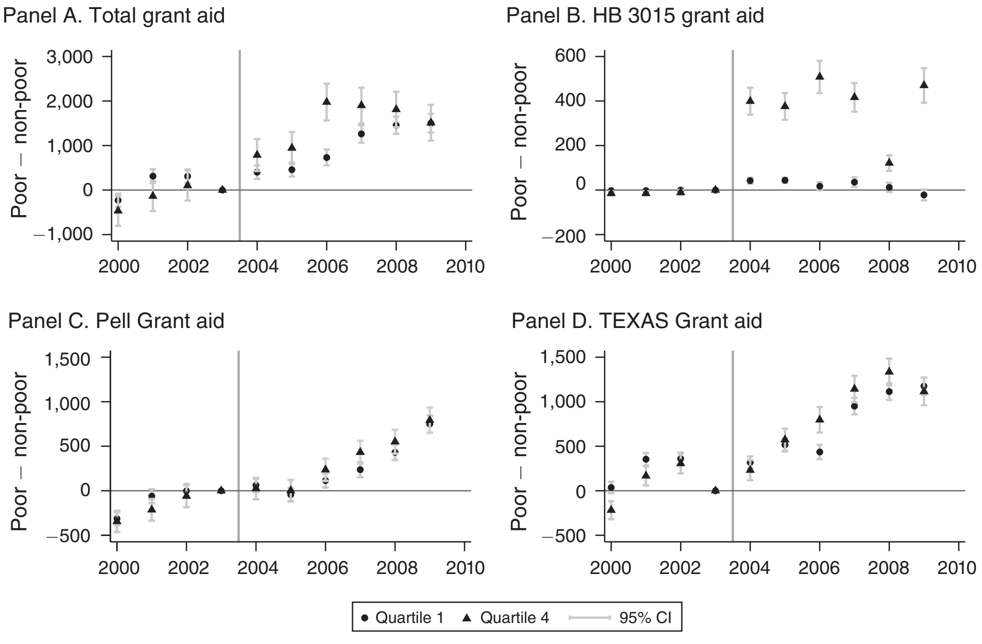


Figure 6. Income-Based Price Discrimination

*Notes:* Estimates in each panel come from regression of each type of grant aid amount on year dummies, year dummies interacted with *Poor* (with the 2003 interaction omitted), program fixed effects, and student demographic and achievement controls. Models are estimated separately for programs in the top and bottom quartile of predicted earnings. The *Poor*-year dummy interaction coefficients are plotted. Standard errors are clustered by high school cohort.

need-based grant aid (relative to non-poor students) immediately after deregulation (Figure 4, panel A). The increase is particularly large at top quartile programs, but still noticeable at bottom quartile programs too. Subsequent panels show the contribution of each of the largest components of need-based grant aid in Texas. HB 3015 set-aside grants increased dramatically following deregulation, but only for students in the highest return programs, which experienced the largest sticker price increases (Figure 4, panel B). Federal Pell Grants expanded modestly following deregulation, though increases were similar for low- and high-return programs (Figure 4, panel C). Furthermore, our cross-state analysis described in online Appendix C suggests a minor role for the national Pell Grant expansion, as similar resorting patterns are not seen in other states that also experienced it.

TEXAS Grants also increased considerably across the board, particularly for students in the highest return programs (Figure 4, panel D). This is partially by design; the maximum TEXAS Grant is pegged to average tuition in the state, and institutions must fully cover tuition and required fees for any TEXAS Grant recipients with non-loan sources, though institutions can choose not to provide TEXAS Grants to qualified students. Taken together, panels B and panel D of Figure 4 demonstrate how HB 3015 set asides along with the TEXAS Grant permit institutions to price discriminate, shielding recipients from sticker price increases.

Table 5—Importance of TEXAS Grant to Sorting Results

| | *Panel A. Predicted earnings* | | |
|---|---|---|---|
| | All observations (1) | EFC +/− 6,000 of TEXAS Grant eligible (2) | EFC +/ − 3,000 of TEXAS Grant eligible (3) |
| TEXAS Grant eligibility | −0.0349 | −0.0189 | −0.0075 |
| | (0.0010) | (0.0015) | (0.0009) |
| TEXAS Grant eligibility × post | 0.0110 | 0.0041 | 0.0028 |
| | (0.0014) | (0.0021) | (0.0013) |

| | *Panel B. Total grant awards* | | | *Panel C. TEXAS Grant awards* | | |
|---|---|---|---|---|---|---|
| | All observations (4) | EFC +/− 6,000 of TEXAS Grant eligible (5) | EFC +/− 3,000 of TEXAS Grant eligible (6) | All observations (7) | EFC +/− 6,000 of TEXAS Grant eligible (8) | EFC +/− 3,000 of TEXAS Grant eligible (9) |
| TEXAS Grant eligibility | 4,649 | 2,584 | 1,924 | 1,281 | 601 | 456 |
| | (194.9) | (82.2) | (101.9) | (190.1) | (64.3) | (57.7) |
| TEXAS Grant eligibility × post | 2,042 | 1,967 | 1,898 | 1,179 | 1,619 | 1,678 |
| | (483.5) | (327.6) | (259.2) | (325.2) | (228.7) | (220.0) |
| Observations | 580,253 | 234,608 | 98,139 | 580,253 | 234,608 | 98,139 |

*Notes:* Linear regression models include indicator for imputed TEXAS Grant eligibility, post-deregulation, the interaction between post-deregulation and TEXAS Grant eligibility, and time (linearly). All models also include controls for gender, race/ethnic indicators, indicator for limited English, and scaled reading and math scores. Full sample includes students in the high school classes of 2000–2009 who enroll in a Texas public university within two years of high school graduation. Outcome in panel A is the log predicted earnings of the university program (institution × major) the student first enrolled in, which is estimated with equation (1) using 2000–2002 cohorts and applied to all cohorts (see text). Outcomes in panels B and C are total need-based aid and TEXAS Grant aid, respectively. TEXAS Grant eligibility criteria is as of 2005: expected family contribution is less than $4,000, enrolled within 16 months of high school graduation, and demonstrated financial need (cost of attendance greater than expected family contribution). Robust standard errors in parentheses are clustered by cohort.

*Source:* Author calculations

To better understand the contribution of the TEXAS Grant specifically to our sorting results, in Table 5 we first repeat our main analysis replacing poor with an indicator for Texas Grant eligibility (based on the criteria as of 2005); sorting results are quite similar to our base estimates (column 1).[33] We isolate the contribution of the TEXAS Grant by restricting analysis in Table 5, columns 2 and 3 to students who are close to the eligibility threshold. These columns reflect the "pure" effect of the TEXAS Grant; differences between Table 5, columns 1 and 2 or 3 reflect channels other than the TEXAS Grant. Using the narrowest bandwidth, estimates are about one-quarter as large as with the full sample, though the proportionate narrowing of the predicted earnings gap is the same. Columns 4–9 confirm that poor students did indeed experience greater total and TEXAS Grant aid following deregulation across the eligibility threshold. We conclude that the TEXAS Grant program played an

[33] We fix eligibility using 2005 rules since time-varying eligibility and TEXAS Grant receipt is endogenous to deregulation.

important role in expanding opportunities to low-income students following deregulation, though it cannot explain the full improvement.

The net result of these aid expansions is a widening of the gap in net tuition between non-poor and poor students following deregulation, particularly at higher return programs. In fact, poor students actually experienced a decrease in net tuition following deregulation at several programs, while non-poor students saw increases of almost \$1,000 per semester.[34] Are programs that experienced the greatest increase in price discrimination also the programs that experienced the largest increase in poor students' representation? To answer this, we estimate program-specific versions of equation (2) separately for each program for net price and an indicator for enrolling in the specific program. Using these program-level estimates, we find that each \$1,000 decrease in the net price that poor students pay (relative to that paid by non-poor students) following deregulation is associated with a 4 percent increase in the likelihood that a poor student enrolls in a specific program (relative to the time pattern for non-poor students). Thus, changes in net price are a plausible mechanism through which the sorting of students across programs changes following deregulation.[35]

Note that this analysis likely understates the effect of deregulation on need-based aid, as institutions were not required to spend additional aid revenue for students in the programs that generated it. For instance, additional aid dollars generated by higher business program prices could have been used to subsidize students in liberal arts.

## B. *Non-price Mechanisms*

Institutions that supported deregulation hoped to use the additional revenue to improve program quality, which may also have affected the sorting of students across programs. We investigate supply-side channels in Table 6. We estimate models interacting $Post_t$ with $PredEarn_{jk}$, the predicted earnings (in 2000) for program $jk$. A useful summary measure of program resources is total salary of all faculty per student enrollment (Tabel 6, column 2), as improvements in several dimensions—more faculty, more highly paid faculty, more tenure-track faculty, smaller class sizes—would be reflected in this measure.[36] Estimates suggest that total salary per enrollment increased most for the more lucrative programs following deregulation. This was accomplished both via expanding the total faculty size, by increasing pay for instructors (either by shifting to a more expensive rank of instructor or increasing pay within rank), and reducing class sizes. These same qualitative

[34] Figure A9 in the online Appendix plots the net tuition for poor and non-poor students separately by program ventile.

[35] We do not place more emphasis on program-specific estimates for two primary reasons. First, programs are of very different sizes and thus enrollment changes are of such different scales that they are difficult to compare. This motivates our normalization by the program-specific poor student enrollment share and also our focus on ventile-specific estimates, since these have comparable scales. Second, program choice is inherently a multinomial decision, and thus the attributes of all alternative programs should also enter students' choices. Program-specific estimates do not account for the attributes of other programs.

[36] Per student resource measures are divided by (number of course enrollments divided by five) to be comparable to unique students, which assumes each student takes approximately five classes.

Table 6—Changes in Sticker Price and Resources Following Deregulation

| | Tuition ($1,000) for in-state junior with 15 SCH (1) | Total salary per enrollment (trimmed) (2) | Total faculty per enrollment (trimmed) (3) | Average FTE salary (4) | Average class size (5) |
|---|---|---|---|---|---|
| Outcome mean | 2.165 | 2,719 | 0.09 | 30,626 | 30.69 |
| *Panel A. Program fixed effects and year fixed effects, no pre-trends* | | | | | |
| Predicted earnings × post | 0.7283 | 524.82 | 0.0124 | 2,167 | −4.75 |
| | (0.0942) | (263.23) | (0.01) | (1,925) | (2.91) |
| Constant | 2.0046 | 2,965.26 | 0.1006 | 30,869 | 30.79 |
| | (0.0179) | (162.97) | (0.01) | (384) | (0.90) |
| *Panel B. Program fixed effects with linear time trends and pre-trends* | | | | | |
| Predicted earnings × post | 0.4407 | 461.42 | 0.0107 | −1,418 | −3.44 |
| | (0.1866) | (291.40) | (0.01) | (1,271) | (1.63) |
| Time | 0.1303 | −64.2 | −0.0023 | −160 | −0.06 |
| | (0.0095) | (65.96) | (0.00) | (191) | (0.27) |
| Post | 0.2861 | −78.14 | −0.0032 | −543 | 1.31 |
| | (0.0409) | (151.99) | (0.01) | (826) | (0.55) |
| Post × time | 0.0099 | 87.98 | 0.0029 | 303 | −0.13 |
| | (0.0116) | (68.58) | (0.00) | (170) | (0.28) |
| Predicted earnings × time | 0.0286 | −144.34 | −0.0008 | 739 | −0.05 |
| | (0.0459) | (154.17) | (0.00) | (777) | (1.02) |
| Predicted earnings × time × post | 0.0574 | 313.86 | 0.0023 | −40 | −0.42 |
| | (0.0510) | (173.13) | (0.00) | (752) | (1.02) |
| Constant | 2.4802 | 2,479.86 | 0.0884 | 30,677 | 30.32 |
| | (0.0239) | (120.20) | (0.00) | (395) | (0.40) |
| Observations | 5,519 | 5,913 | 5,913 | 6,027 | 6,098 |

*Notes:* Linear regression models in panel A include log predicted program earnings, the interaction between post-deregulation and log predicted earnings, program fixed effects, and year fixed effects. Panel B replaces year fixed effects with time (linearly), an indicator for post-deregulation, and time interacted with post-deregulation, as well as interactions between log predicted earnings and time and time × post. The full sample includes 643 programs over 10 years, though the analysis sample is smaller due to missing tuition and resource measures for some programs in some years. Program-specific predicted earnings control for student demographics and test scores. Program resource measures are constructed by aggregating class section and faculty-level data to each department in each year (see online Appendix B). Trimmed outcomes drop observations in the top or bottom 5 percent of values. Regressions are weighted by number of students enrolled from the 2000 high school cohort. Standard errors are clustered by program. SCH = Student Credit Hours.

*Source:* Author calculations

patterns remain even when we let high-returns programs have a different initial and post-deregulation growth rate in Table 6, panel B. Some measures demonstrate improvement immediately following deregulation, while others also improve at a faster rate following deregulation. These greater levels of instructional inputs may partially offset the detrimental effects of the price increases used to generate them.

To determine how much of the deregulation-induced resorting operates via shifts across versus within-institution, we re-estimate equation (2) but with institution- or major-average predicted earnings as the outcome (rather than institution-major predicted earnings). Estimates using institution-average predicted earnings are quite similar to our main model, suggesting that almost all of the change can be explained by gains in the relative quality of institutions attended by poor students, while cross-major shifts explain little.[37] One channel through which institutions could mitigate adverse effects of price increases on poor students is by changing admissions processes to favor poor students or by encouraging more to apply. We are not aware of any systematic changes in admissions policies that differentially affected poor versus non-poor students at the time (other than those discussed earlier), but we also assessed this quantitatively by estimating institution-specific versions of equation (2).[38] We examine both the unconditional likelihood of enrolling or applying to each institution and the likelihood of being admitted (conditional on applying). There is a clear relative increase in the likelihood that poor students enroll at higher return institutions following deregulation and a corresponding decrease at lower return institutions. However, these gains do not appear to be systematically related to increases in the relative likelihood that poor students are admitted to these institutions (conditional on applying). Furthermore, some programs (most often business) within institutions practice selective admissions. The stated GPA cutoffs for admissions to these programs do not change following deregulation.[39] While we cannot rule out other non-price channels as important, such as marketing or targeted outreach, we can say that our results are not due to the biggest outreach programs operated by the two flagship institutions.[40]

Finally, we examine changes in program size as a potential mechanism through which these shifts occurred (reported in online Appendix D). Total enrollment in low-earning programs grew throughout our analysis period, but did not experience above-trend growth following deregulation. Enrollment in more lucrative programs was mostly stagnant both before and after deregulation. For the most lucrative programs, the lack of any aggregate enrollment change suggests poor students are (modestly) displacing their non-poor counterparts. For less lucrative programs, there is growth in the enrollment of poor students and non-poor students, but enrollment for non-poor students is occurring at a faster rate.

### C. *Separating the Contribution of Different Channels*

We are not able to isolate the contribution of each individual channel to the resorting that occurs following deregulation, though evidence suggests that both price and quality channels could be important, particularly if program quality is not well known. Prior work has consistently demonstrated that students are very sensitive to both sticker and net price in their enrollment, institution, and major choices (Dynarski 2000, Long 2004, Hemelt and Marcotte 2011, Stange 2015), with

[37] The results are included in online Appendix Table A9. We also estimated our base model, but included first school and first major fixed effects separately, with a similar conclusion. Including first school fixed effects completely eliminates the deregulation effect but major fixed effects (without school fixed effects) does not.

[38] Results are reported in Table A10. Admissions data is incomplete for our first cohort, so this analysis only includes the 2001–2009 high school cohorts. Online Appendix Table A11 reports means for all the outcomes.

[39] The required GPA for admissions to the undergraduate business programs at the UT–Austin (GPA = 3.0), Texas A&M (3.0), the University of Houston (2.75), and Texas Tech (2.75) remained constant from 2003–2005. UT–Arlington increased from 2.0 to 2.5 in this time period. Texas A&M's engineering admission standard also remained constant (at 2.0).

[40] As shown in Table 3, column 4, our results hold even after excluding high schools targeted by the Longhorn Opportunity Scholars (UT–Austin) and Century Scholars (Texas A&M) programs.

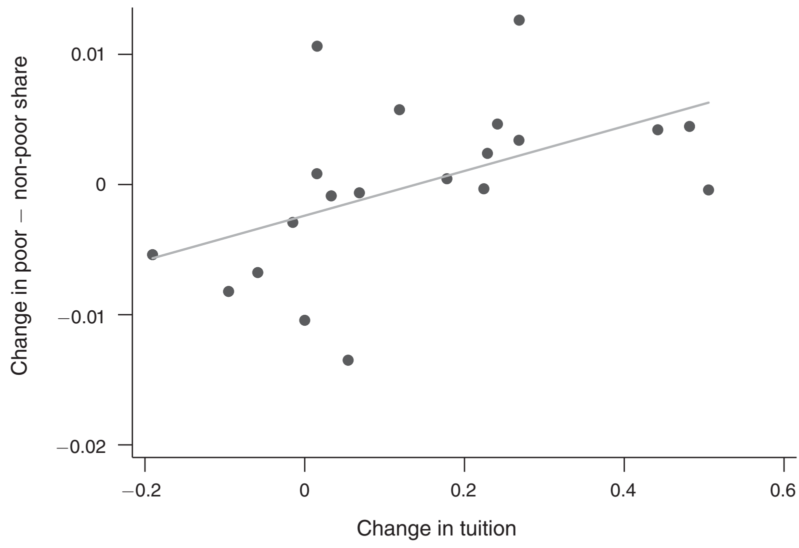


Figure 7. Enrollment Changes versus Tuition Changes for Each Ventile of Predicted Program Earnings

*Notes:* Each dot represents an estimate of the change in poor versus non-poor share and change in tuition for a single ventile. The vertical axis is the ventile-specific coefficient on *Poor* × *Post* depicted in Figure 5 and the horizontal axis is the ventile-specific coefficient on *Post* depicted in Figure 3B.

low-socioeconomic status (SES) students being particularly price sensitive (Jacob, McCall, and Stange 2018). However, evidence on responsiveness to program quality is less clear. Students are attracted to more selective institutions and high-paying majors (Dillon and Smith 2017; Beffy, Fougère, and Maurel 2012; Wiswall and Zafar 2015), though appear to be less sensitive to financial resources specifically (Jacob, McCall, and Stange 2018). Furthermore, quality differences are not well known, particularly to low-SES students (Hastings, Neilson, and Zimmerman 2017; Huntington-Klein 2016). It is possible that deregulation made quality differences more salient, with sticker price serving as a signal of quality (e.g., Wolinsky 1983). Increasing the salience of program quality can improve the program choices of low-SES students in particular (Hastings, Neilson, and Zimmerman 2017).

To further explore the role of price and quality channels, we compare ventile-specific estimates of the change in poor student representation, tuition costs, resources, and grant aid. A benefit of such a ventile-specific analysis is that it accounts for size differences across programs that can make it difficult to interpret magnitudes for program-level analysis. Figure 7 demonstrates that the ventiles that experienced the greatest sticker price increase following deregulation—those with higher than average returns—also saw the greatest increase in the relative share of poor students. Panel A of Figure 8 shows the "first-stage" relationship between these tuition increases and two key mechanisms: program-level resources and need-based aid provided to poor students (relative to non-poor students).[41]

[41] Since sticker price for poor and non-poor students is the same within program, this latter measure captures the extent of price discrimination practiced by institutions.

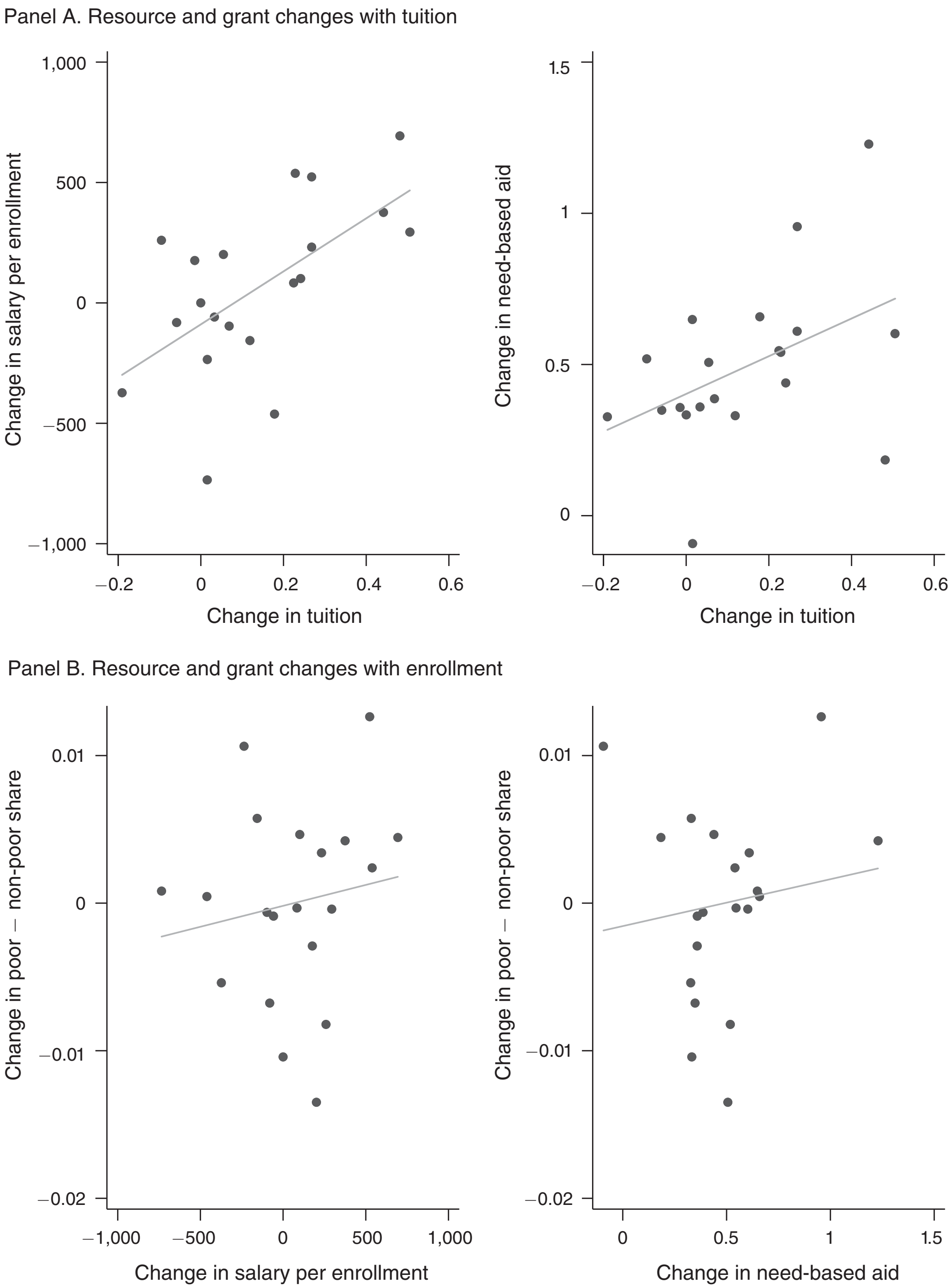


Figure 8. Resource and Grant Changes versus Tuition and Enrollment Changes

*Notes:* Each dot represents an estimate of the change in two outcomes for a single ventile. Estimates for sticker prices and salary per enrollment use program-level data and are normalized to 0 in the lowest ventile. Estimates for the change in poor–non-poor share use micro student data and come from Figure 5.

Increases in resources and price discrimination were largest for programs that had the largest tuition increases following deregulation.[42] Figure 8, panel B shows the "structural" relationship between changes in resources and grant aid and poor students' representation in these programs. Though noisy, the results do suggest that programs that saw the greatest increase in resources and price discrimination also saw the largest gains in the representation of low-income students. Thus, greater price discrimination (increased need-based grant aid for poor students) and resource improvements appear to be important mechanisms for the shifts we observe.

## VI. Conclusion

This paper assesses the consequences of a shift in price-setting authority for undergraduate education in Texas from the state legislature to the institutions themselves. Texas's public colleges and universities responded to this new autonomy by increasing price levels and variation across programs, with particularly sharp increases for the highest earning programs, such as business and engineering at the state flagships. Contrary to the fears of deregulation opponents, we find no evidence that tuition deregulation reduced the representation of poor students in these programs. In fact, poor students shifted relative to non-poor students away from the least lucrative programs into more lucrative programs throughout the distribution. Importantly, these shifts in initial program choices are persistent, as we see similar improvements in the relative quality of programs that poor students are enrolled in two years after initial enrollment.

The Texas experience illustrates a way that higher education institutions can raise tuition revenue and improve quality without magnifying the existing inequalities that already plague higher education (Chetty et al. 2017). Two countervailing responses appear to have partially offset the detrimental effects of price increases on demand by poor students. First, a significant share of deregulation-induced tuition revenue was channeled back into financial aid for needy students, shielding them from price increases. Second, additional revenue enabled supply-side improvements that made lucrative programs more desirable even as they became more expensive. These results underscore the importance of examining the use of funds generated by tuition increases when assessing effects on students. These findings echo those of Deming and Walters (2017) who finds that state subsidies have a larger impact on student enrollment and degree production at unselective colleges when used to boost aid and quality than if used for sticker price reduction. Changes appear concentrated in students' choice of institution (rather than the decision to enroll at all or in choice of major). One possible explanation is that the students make college decisions in stages: any enrollment, then institutional choice, then major choice. The price, aid, and resource changes that affect these decisions may be most salient (or influential) at the institutional choice stage.

[42] Figures A10 and A11 in the online Appendix show that multiple resource measures improve most for programs that saw the greatest increase in tuition and that only expansions in HB 3015 and TEXAS Grant programs are related to tuition increases, as expected.

How likely is it that other states or countries would experience a similar pattern if they were to adopt a similar tuition-setting model? Our results likely generalize to other settings where tuition increases are tied to additional grant aid (via set-asides). Direct set-asides were the main mechanism through which the relative prices of different programs were altered for poor and non-poor students. Such set-asides are not unusual, as several recent deregulation proposals combine both pricing autonomy and additional grant aid. We speculate that deregulation would have had quite different effects if this provision were removed. A second factor affecting generalizability is the TEXAS Grant, the state's large need-based grant program. Some grant programs in other states similarly have institutional autonomy over its dispersion, though Texas appears to be unusual in combining this autonomy with features that make the program particularly effective at shielding poor students from tuition increases. However, the analysis suggests that the TEXAS Grant cannot explain all of the resorting we document, as much of it occurs among students who are not on the margin of TEXAS Grant eligibility. Nonetheless, the uncertain role of the large and generous state need-based grant program warrants some caution in extrapolating our results to other settings that lack such a program.

Our reduced-form results highlight three directions where more research is clearly needed. First, the analysis has not isolated the independent contribution of the various possible mechanisms to the sorting of students to programs following deregulation. Several attributes changed following deregulation, so their contribution is difficult to separate with reduced-form methods. Future work should aim to quantify the role of various mechanisms and to perform simulations of counterfactual changes in these program attributes. Such analysis would say, for instance, what the sorting of students would have looked like in the absence of changes in need-based grant aid, which would greatly aid generalizability. Second, we have taken institutions' pricing and resource allocation decisions as exogenous. Modeling the supply-side responses to this large change in the regulatory and economic environment as an endogenous process could shed light on the objectives of public universities, their production process, and the constraints they face. The fact that the institutions took some steps to partially shield low-income students from price increases suggests a desire to maintain some socioeconomic diversity at these institutions. Finally, how these countervailing factors—prices and resources—impact the success of students actually enrolling in these programs or student loan debt are questions with important welfare implications. While we find no adverse effects on the medium-run outcomes for poor students, future work should examine these long-run consequences too.

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