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THE IMPACT OF PRICE CAPS AND SPENDING CUTS ON U.S. POSTSECONDARY  
ATTAINMENT

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The Impact of Price Caps and Spending Cuts on U.S. Postsecondary Attainment  
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**ABSTRACT**

Increasing the postsecondary attainment rate of college-age youth is an important economic priority in the U.S. and in other developed countries. Yet little is known about whether different forms of public subsidy can increase degree completion. In this paper, we compare the impact of the marginal taxpayer dollar on postsecondary attainment when it is spent on lowering tuition prices versus increasing the quality of the college experience. We do so by estimating the causal impact of changes in tuition and spending on enrollment and degree completion in U.S. public postsecondary institutions between 1990 and 2013. We estimate these impacts using a newly assembled data set of legislative tuition caps and freezes, combined with variation in exposure to state budget shocks that is driven by differences in historical reliance on state appropriations. We find large impacts of spending on enrollment and degree completion. In contrast, we find no impact of price changes. Our estimates suggest that spending increases are more effective per-dollar than price cuts as a means of increasing postsecondary attainment.

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A data appendix is available at <http://www.nber.org/data-appendix/w23736>

# 1 Introduction

Postsecondary attainment rates across countries and sub-country regions are strongly related to economic growth (Hanushek and Woessmann, 2008; Gennaioli et al., 2013; Hanushek et al., forthcoming). Yet the share of college-educated youth in the U.S. has grown only slowly over the last several decades, compared to more rapid growth rates in other developed countries (OECD, 2013; Autor, 2014). Thus increasing degree completion rates among U.S. college-age youth is an important national economic priority.

College costs are frequently cited as a key reason for the slow growth of U.S. educational attainment, and tuition and fees at U.S. public institutions have risen much faster than the rate of inflation over the last thirty years (Dynarski, 2008; Deming and Dynarski, 2010; Baum and Ma, 2014). The Federal government has responded to rising college prices by tripling expenditures on Federal financial aid between 1995 and 2015, from \$50 billion to over \$150 billion in constant 2015 dollars, and many U.S. states introduced state merit aid scholarships and need-based aid programs over the same period (Dynarski, 2008; Fitzpatrick and Jones, 2012; Baum and Ma, 2014).

Yet despite dramatic increases in financial aid, the share of U.S. youth age 25-29 who complete a bachelor's degree or higher has risen only modestly in recent years, from 27.9 percent in 2000 to 30.2 percent in 2013 (National Center for Education Statistics, 2015a). Why have large increases in financial aid not led to large increases in college degree attainment? One possibility is that college quality has declined. Between 2000 and 2014, inflation-adjusted state appropriations per full-time equivalent student fell by 28 percent, and total per-student spending fell by 16 percent (Baum and Ma, 2014).<sup>1</sup>

While there is a strong positive correlation between per student spending and rates of degree completion in U.S. public postsecondary institutions, there exists little causal evidence of the impact of changes in per-student spending on degree completion (e.g. Bound and Turner, 2007; Deming, 2017). One view is that higher education spending pays for administrative bloat and consumption amenities, in which case lower levels of spending may be cost-effective (see, e.g., Ginsberg, 2011; Ehrenberg, 2012; and Jacob et al., 2013). On the other hand, spending cuts may reduce degree completion by harming the quality of instruction, limiting the number and variety of course offerings, increasing class size, or moving students into non-credit-bearing remedial courses (Bettinger and Long, 2009; Bahr, 2014).

We use data from the Integrated Postsecondary Education Data System (IPEDS) to study the impact of changes in spending and tuition at U.S. public postsecondary institutions between 1990 and 2013. The thought experiment that motivates our analysis is as follows. Suppose a U.S. public institution receives an unexpected increase in state appropriations of \$1,000 per student. One option is to pass this extra \$1,000 on to students in the form of tuition cuts or scholarships. This would reduce the price of college, but keep per-student spending constant. Alternatively, the college might elect to keep tuition constant and spend the

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<sup>1</sup>Authors' calculations using the Integrated Postsecondary Education Data System (IPEDS). Another possible reason for declining college attainment rates is that recent cohorts of students are less academically prepared for college. Yet math and reading achievement levels of U.S. students have remained constant or even increased slightly in recent years (National Center for Education Statistics, 2015b).

extra \$1,000 per student on smaller classes, fancier athletic facilities, or other educational inputs or amenities. We seek to determine which of these choices would lead to a larger impact on student enrollment and degree completion. In other words, if the goal is increasing postsecondary attainment, should the marginal public dollar be spent on lowering tuition prices or increasing college quality?

Our analysis exploits cross-sectional variation in institutions' historical reliance on state appropriations combined with state-level changes in funding over time to identify impacts of state funding on spending, tuition and enrollment. State appropriations for higher education fluctuate with the business cycle, and policy decisions about higher education funding generally operate at the state level (Kane et al., 2003; Barr and Turner, 2013). Yet the impact of a state-level budget cut is likely to be greater for institutions that rely more heavily on state appropriations as a source of revenue. This idea motivates an instrumental variables (IV) strategy that uses the interaction between total state appropriations for higher education and each institution's baseline dependence on state support to instrument for state funding. Our IV strategy discards variation in funding driven by policymakers' decisions about which institutions to support in particular years, and allows us to control for permanent differences across institutions, changes in outcomes common to all institutions within a state, and important time-varying determinants of the demand for higher education such as state and local unemployment rates. This approach parallels identification strategies commonly used in studies of local labor markets, immigration, and trade, which also construct instruments combining cross-sectional exposure with aggregate changes in the treatment of interest (Bartik, 1991; Blanchard and Katz, 1992; Card, 2001; Autor et al., 2013).

Schools can respond to budget cuts by reducing spending or raising tuition. The reduced form effect of a budget shock on enrollment measures the net impact of adjustments on both margins. Identification of the partial effects of prices and spending requires an additional instrument that induces independent variation in these two variables. To this end, we construct instruments for tuition using a newly assembled data source of tuition caps and freezes imposed by state legislatures. Using the budget shock and price cap instruments together in a two-stage least squares (2SLS) framework allows us to separately estimate the impact of a price change holding spending constant, and a spending change holding price constant.

We find large impacts of spending on student enrollment. Our estimates imply that a 10 percent increase in spending increases current enrollment by 3 percent. In contrast, estimated impacts of price changes are statistically insignificant. While we cannot reject modest price elasticities, our estimates are precise enough to reject equal elasticities of enrollment with respect to prices and spending at the five percent level. We find positive and statistically significant impacts of spending on enrollment for both two-year and four-year institutions, although the elasticities for two-year institutions are somewhat larger.

We also find positive and statistically significant causal impacts of spending on degree completion, including bachelor's degrees. Our estimates imply that the marginal cost of an additional bachelor's degree is between \$102,532 and \$155,451, an effect that passes a benefit-cost test under conservative assumptions about the private labor market returns to college completion. We present evidence that spending impacts are

driven mostly by increased persistence and degree completion among already-enrolled students, rather than increases in initial college matriculation. We also find that only about 15 percent of the enrollment impact of spending at public institutions can be explained by crowd-out of private school enrollment. Moreover, while do we find some evidence of significant crowdout for associate’s degrees, increased public funding of postsecondary institutions appears to increase total certificate and bachelor’s degree completion, rather than simply shifting students across sectors. Finally, we find no evidence that price or spending changes affect financial aid, suggesting that changes in sticker price also lead to changes in the net price paid by students.

To our knowledge, this is the first paper to directly demonstrate a causal link between spending and degree attainment in U.S. postsecondary institutions. The most closely related paper is Bound and Turner (2007), who show that larger state cohorts have lower degree attainment rates. While they argue that lower public subsidies per student are the key causal mechanism, they do not directly measure changes in public spending on higher education, nor do they use institution-level data on student outcomes. More broadly, our results relate to studies of “cohort crowding” and college quality, which draw linkages between changes in college resources, declining completion rates and increased time to degree over the last twenty years (Turner, 2004; Dynarski, 2008; Bound and Turner, 2007; Bound et al., 2010, 2012).

We find that changes in state funding lead to large impacts on core academic spending. Instruction alone accounts for about 40 cents of every dollar increase in spending. Moreover, we find that academic support spending - including tutoring, advising and mentoring - is particularly responsive to state budget shocks. This is consistent with a number of recent studies finding large impacts of student supports on persistence and degree completion (Angrist et al., 2009; Bettinger and Baker, 2011; Carrell and Sacerdote, 2013; Barrow et al., 2014; Scrivener et al., 2015). While ultimately the mechanisms are only suggestive, we argue that our results are most consistent with spending improving quality by lifting informal capacity constraints such as course waitlists and inadequate advising (e.g. Bound et al., 2012).<sup>2</sup> Further afield, our findings are also consistent with evidence that increased resources at primary and secondary schools boost educational attainment and other outcomes (Card and Krueger, 1992; Jackson et al., 2016; Lafortune et al., 2016).

The rest of the paper proceeds as follows. Section 2 describes the data and provides some background information on the state higher education budgeting and tuition regulation processes. Section 3 lays out our identification strategy for estimating causal effects of spending and tuition, and explores the validity of our approach. Section 4 presents our main results - estimates of the effects of price and spending changes on enrollment and degree completion. Section 5 describes additional results and discusses some policy implications of our findings. Section 6 concludes.

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<sup>2</sup>In principle, spending cuts could lead to formal capacity constraints through admissions quotas. We think this is unlikely to explain our results, for two reasons. First, most of the colleges in our sample (and nearly all of the community colleges) accept every student who applies and meets minimum academic qualifications. Second, a web search revealed the existence of formal capacity constraints in only a handful of states and years. Our results are robust to excluding schools that accept fewer than 50 or 75 percent of applicants, and they are nearly unchanged when we exclude states and years with formal capacity constraints from the analysis.

## 2 Data and Background

### 2.1 Data Description

The data used here come from the Integrated Postsecondary Education Data System (IPEDS). IPEDS is a survey of colleges, universities and vocational institutions conducted annually by the U.S. Department of Education (DOE). The Higher Education Act requires postsecondary institutions to participate in IPEDS to retain eligibility to administer Federal Title IV student aid (Pell Grants and Stafford Loans).<sup>3</sup> IPEDS collects information on student enrollment overall and by race, gender, age and student status (part-time/full-time, freshman/continuing student, undergraduate/graduate, degree-seeking), as well as degree completion by level (certificate, associate’s, bachelor’s) and field of study. IPEDS also collects detailed information on institutional finances, including revenues and expenditures by source. IPEDS data on all variables relevant for our study are available back to 1990.

IPEDS collects data at the campus level using a unique longitudinal identifier, and includes basic information about each campus such as level, control, degree-granting status and geographic location. Campus-level data allows us to separate enrollment and finances for branch campuses of university systems.<sup>4</sup> We supplement the IPEDS data with state legislative appropriations data from Grapevine, an annual survey compilation of data on state support for higher education administered by the State Higher Education Executive Officers (SHEEO) Association and the Center for the Study of Education Policy at Illinois State University.<sup>5</sup> We also match the IPEDS to publicly available data on state and county unemployment rates collected by the Bureau of Labor Statistics, as well as annual data on state tax receipts and other forms of state government spending such as Medicaid. Finally, we match IPEDS to state- and county-level data from the Census and the American Community Survey (ACS).<sup>6</sup>

Table 1 presents descriptive statistics for our analytic sample. The first ten rows display financial information for public institutions by sector and selectivity. Selective public four-year universities are those ranked “Most Competitive” or “Highly Competitive” - the two highest categories - in the 2009 Barron’s Profile of American Colleges. All financial data are in per-student terms, weighted by enrollment and adjusted for

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<sup>3</sup>Postsecondary institutions that do not administer Title IV aid are not required to report to IPEDS. There are many of these institutions, but they are very small and generally offer non-degree certificate programs and other short courses. Nearly all non-Title IV institutions are for-profit - see Cellini (2010) for a more detailed discussion.

<sup>4</sup>To ensure the accuracy and consistency of our data over time, we compare the raw IPEDS data to data from the Delta Cost Project (DCP), a collaboration between the DOE and the American Institutes for Research that makes IPEDS longitudinally consistent and matches branch campuses of individual institutions together. Jaquette and Parra (2016) show that the DCP database collapses multiple institutions within some public university systems into a single administrative unit, which would be problematic for our analysis. We therefore use the raw IPEDS data rather than the DCP, although results are generally similar in either case.

<sup>5</sup>The Grapevine data can be found at <https://education.illinoisstate.edu/grapevine/historical/>. We measure appropriations from Grapevine rather than IPEDS because of concerns about duplicate reporting of state funding across campus branches of institutions, as well as errors in administrator survey responses. IPEDS appropriations aggregated to the state-year level are similar to corresponding measures in Grapevine (the correlation equals 0.83).

<sup>6</sup>County identifiers appear in the IPEDS from 1990 to 1999, and then again from 2008 onward. For over 95 percent of institutions and 99 percent of publics, county codes were consistent in 1999 and 2008, and alternative measures of geography (MSA, address, city, zip code) also did not change. In these cases we interpolate county IDs for the intervening years. In the small number of cases where location changed or was missing, we use other institutions with the same alternative geography to fill in missing values. Geographic information is left missing in cases where we cannot reliably locate an institution.

inflation using the Higher Education Price Index. Financial data are collected as of the fiscal year, which usually begins in July. Enrollment data are counted for the fall of each calendar year.

The sample is restricted to institutions in the 50 U.S. states (excluding Puerto Rico) and excludes schools with missing values for state appropriations in 1990. To address concerns about reporting and measurement error in IPEDS spending data, we also eliminate the less than 3 percent of institutions with per capita spending of less than \$100 or more than \$100,000 per college-age student in any year in the panel. Note that total spending includes capital costs such as operation and maintenance in addition to core academic functions.

Table 1 reveals four key facts. First, per-student spending in public institutions derives largely from three sources: state (and local) appropriations, tuition and fees, and financial aid grant programs (mainly Federal Pell Grants, but also including state and local grants). This is particularly true for less selective schools. In 1990, the combined contributions of these three categories to total spending at selective four-year, less selective four-year, and two-year institutions equaled 64, 78 and 90 percent, respectively. The relatively small contributions of other categories such as endowment spending means that budgets are strongly affected by cuts in state funding, especially for less selective institutions.

Second, more than half of total spending is accounted for by the core categories of instruction, academic support, and student services, and the share of total spending devoted to these core categories has grown over time. Core academic spending is a higher share of total spending for less selective institutions. Third, while less selective schools receive much lower levels of state funding in dollar terms, they rely more heavily on state support. In 1990, state and local appropriations represented about 44 percent of total spending for selective four-year institutions, compared to 51 percent for less-selective four-year institutions and 62 percent for two-year institutions. This means that a state budget cut should have greater relative impacts on less selective institutions. Fourth, all types of public institutions received less public support and became more reliant on tuition revenue between 1990 and 2013.

## 2.2 Higher Education Appropriations and Tuition

Our description of state legislative higher education funding relies on a SHEEO survey of state budgetary processes (Parmley et al., 2009). While the details differ across states, a typical budgetary process unfolds as follows:<sup>7</sup>

1. One to two years in advance of the fiscal year, a state higher education coordinating board develops a budget request that covers all public institutions in the state.

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<sup>7</sup>The SHEEO survey, which received responses from 43 states, covers topics such as how budget requests are developed, how funds are administered across campuses, and which organizations have primary authority over spending and tuition setting. All but one surveyed state had a coordinating board of some kind. Institutions submit budget requests individually in only six states. Most respondents indicated that budget requests are based on past and future projections of enrollment, changes in costs such as salaries, and any special projects or initiatives. In 14 cases, respondents indicated that governors vetoed or reduced specific budget line items. The executive branch fully funded the initial budget request in about half of cases, and that number is slightly lower for the legislative budget. Open-ended responses to the survey indicated that economic conditions and other legislative priorities were key reasons that higher education budget requests were not fully funded.

2. The governor proposes a budget based on the request. The legislature then enacts its preferred budget, and the governor can subtract individual items or veto the budget entirely, while the legislature can accept (or override in some cases) the modified budget. Protracted negotiations are common and can become quite complicated.
3. The end result is a budget that is typically ratified in the spring and takes effect the following fall. A key source of uncertainty in this process is the possibility that budget requests will not be fully funded, and this is especially likely when tax revenues are less than expected.

An important difference between state and Federal budgeting processes is that states are mostly unable to smooth business cycle fluctuations in tax revenue. Nearly all states have some sort of balanced budget requirement, and higher education spending often serves as the “balance wheel” used to meet these requirements when tax revenues fall short of projections (Kane et al., 2003; Delaney and Doyle, 2011; Barr and Turner, 2013).

States differ markedly in their support for higher education. Appendix Figure A1 presents trends in per capita appropriations in four large states. The figure shows substantial differences in levels of funding and in trends over time. For example, in California real per-capita state appropriations for higher education grew from around \$5,000 in 1990 to over \$6,000 in the early 2000s, before plummeting to less than \$4,000 in 2013. In contrast, public support for higher education in Texas remained roughly constant at around \$4,000 per student over this period. These heterogeneous patterns are typical of the wide variation in public higher education spending across states, even those with similar demographics.<sup>8</sup>

State policies influence both spending and tuition at public institutions. As noted by Bell et al. (2011), states vary in the extent of legislative control over tuition setting.<sup>9</sup> Tuition setting is generally less centralized than the state appropriations process, with institutions setting tuition either completely on their own or in non-binding consultation with the higher education coordinating board. However, state legislatures can exert formal control over prices through tuition caps, defined as statutory limits on increases in public college tuition from one year to the next. Tuition caps affect higher education budgets by shutting down one margin of adjustment. Institutions operating under a legislative tuition cap cannot respond to appropriations shocks by increasing prices, and thus are more likely to cut spending.

Seventeen states imposed formal price controls on public institutions at least once between 1990 and 2013. The complete list appears in Appendix Table A1. We compiled these data by referencing official sources when available, combined with Lexis-Nexis searches of state newspapers going back to 1990. Some states

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<sup>8</sup>For example, appropriations per capita were about 50 percent higher in New Mexico compared to Arizona, and that gap has widened considerably since 2010. Arkansas, Tennessee and Kentucky all spent about \$3,100 per capita in 1990. In the early 2000s Kentucky had the highest per capita spending (around \$5,600, compared to \$4,300 in Arkansas and \$3,500 in Tennessee), but by 2013 Arkansas was spending the most (\$5,500, compared to \$4,700 in Kentucky and only \$3,000 in Tennessee).

<sup>9</sup>Bell et al. (2011) describe a SHEEO survey of tuition, fee and financial assistance policies, which received responses from 35 states. Only four states indicated that formal tuition-setting authority for either the two-year or four-year sector lies with the legislature. In nearly all other states, tuition is set by the coordinating board in consultation with individual institutions, and changes in tuition are not necessarily uniform across institutions. In all but six of the states that responded, tuition revenue is controlled and retained by individual campuses.



impose uniform tuition caps, while others single out particular sectors or even institutions. Tuition caps appear to be more frequent in economic boom times (e.g. 1994-2000, 2006-2009), but there is no obvious geographic or demographic pattern in which states impose price caps. The five states with the highest (enrollment-weighted) exposure to tuition caps are Ohio, Idaho, Virginia, New York and Maryland. Across all years in our sample, about 9 percent of students were enrolled in public institutions operating under a legislative tuition cap or freeze.

### 2.3 Naive Estimates of Enrollment Impacts

Reverse causality is the central challenge in estimating the impacts of price and spending changes on enrollment. Policymakers might award additional funding and push for lower prices in colleges that are already expanding their enrollment - perhaps because of increasing local demand for higher education. Changing demand would lead to reverse causality, with enrollment affecting tuition prices rather than tuition prices affecting enrollment. Similarly, spending may increase in anticipation of growth in demand, and increased enrollment tends to generate higher tuition revenue and therefore more spending.

To illustrate this concern, we show how naive estimates of enrollment impacts are biased by reverse causality. Consider the following panel regression of enrollment on spending or tuition:

$$Y_{i,t} = \gamma_i + \psi_t + \sum_{\ell=-L}^L \delta_\ell X_{i,t-\ell} + u_{i,t}, \quad (1)$$

where  $Y_{i,t}$  represents log enrollment for institution  $i$  in year  $t$ ,  $\gamma_i$  and  $\psi_t$  are institution and year fixed effects, and  $X_{i,t}$  is log total spending or log tuition. The coefficient  $\delta_\ell$  describes the relationship between enrollment in year  $t$  and tuition or spending  $\ell$  years earlier, controlling for permanent differences across institutions, changes over time common to all institutions, and tuition or spending in other years. Standard errors are clustered by institution.

Figure 1 plots estimates of equation (1), with coefficients arranged in event time so that positive indices correspond to lagged spending and tuition. The top panel shows results for log spending. Spending increases are correlated with contemporaneous increases in enrollment. The base year coefficient suggests that a 10 percent increase in spending in year  $t$  is associated with a 3.0 percent increase in enrollment in this year. Current spending is also associated with smaller increases in enrollment in subsequent years. The bottom panel shows results for log tuition. A 10 percent tuition cut in year  $t$  is associated with a 0.24 percent enrollment increase in the same year. This estimate is somewhat smaller than estimates reported by Hemelt and Marcotte (2011), a study that uses a similar panel framework with additional controls and finds an elasticity of enrollment with respect to tuition equal to -0.08 for non-selective public institutions.

However, the bottom panel of Figure 1 shows that higher tuition in year  $t + 1$  is associated with higher enrollment in year  $t$ , which suggests that institutions lower tuition both before and after increases in enrollment. Likewise, we find a statistically significant and positive pre-trend coefficient for the association

between spending in year  $t + 1$  and enrollment in year  $t$ . Overall, the patterns in Figure 1 suggest that while enrollment tends to change contemporaneously with spending and tuition, naive regressions of enrollment on changes in spending and price should not be interpreted causally.

A possible solution to this endogeneity problem is to study changes in spending and prices that are induced by state-level policy changes. Figure 2 presents estimates of equation (1) with log total spending, log tuition and log enrollment as the dependent variables, and log state appropriations as the key right-hand side variable. As above, these models include institution and year fixed effects.

The top panel shows that state appropriations are strongly linked to current spending. A 10 percent increase in appropriations in year  $t$  is associated with a 3.1 percent increase in spending in the same year. The middle panel shows that state funding is negatively correlated with tuition prices, with a 10 percent increase in appropriations linked to a price cut of about 0.6 percent. However, the bottom panel indicates that while increases in appropriations are linked to contemporaneous and later increases in enrollment, there is evidence of significant pre-trends.

Figure 2 shows that there are at least two problems with using changes in state appropriations to estimate the impacts of spending on college enrollment. First, as shown in the bottom panel, future appropriations changes predict current enrollment. This pattern may reflect funding decisions that anticipate changes in the demand for higher education. For example, state legislatures may allocate more total funds for higher education when enrollment is projected to grow quickly, or target extra funds to institutions within a state where demand is growing especially fast.

A second problem highlighted by Figure 2 is that even exogenous changes in state funding would not be sufficient to identify the effects of spending and tuition on enrollment. The top and middle panels show that institutions appear to adjust both spending and tuition in response to higher appropriations. The impact of an increase in state funding therefore measures the combined effect of both potential adjustments. This simultaneity between spending and price responses complicates the interpretation of studies that seek to study the effects of tuition or school resources in isolation (e.g., Hemelt and Marcotte, 2011). In the next section, we develop an instrumental variables strategy that combines a plausibly exogenous component of state funding with price variation from legislative caps and freezes to separate the impacts of spending and tuition.

## 3 Identification Strategy

### 3.1 Budget Shocks and Tuition Caps

As discussed above, state budget changes are typically - but not always - made “across the board” (e.g. all institutions in the state receive 90 percent of their funding requests).<sup>10</sup> However, an across the board

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<sup>10</sup>Despite the multiple rounds of negotiation and attention to specific line items, more than half of respondents to the 2009 SHEEO survey indicated that appropriations are distributed as lump sum amounts rather than individual line items that are earmarked for a particular use (Parmley et al. 2009). In some cases, appropriations are distributed to the coordinating board

budget cut is likely to have a greater proportionate impact on institutions that rely on state appropriations for a larger share of operating revenue. Thus some schools' budgets are more sensitive to state-level funding fluctuations than others. We exploit *ex ante* differences across institutions in their reliance on state revenue to estimate the impact of funding changes.

Specifically, we construct a budget shock instrumental variable (IV) that multiplies yearly state appropriations by each public institution's share of total revenue from state appropriations in 1990, the first year that IPEDS data are available. This instrument is constructed as

$$Z_{i,t} = \left( \frac{Approp_{i,90}}{Rev_{i,90}} \right) \times \left( \frac{StApprop_{s(i),t}}{Pop_{s(i),t}} \right), \quad (2)$$

where  $Approp_{i,90}$  and  $Rev_{i,90}$  measure state appropriations and total revenue for institution  $i$  in 1990,  $s(i)$  denotes state for institution  $i$ , and  $StApprop_{s,t}$  and  $Pop_{s,t}$  represent total appropriations and college-age population for state  $s$  in year  $t$ . The first factor in (2) is each institution's revenue from state appropriations divided by total revenue in 1990. This captures a school's dependence on state funds at baseline. Using the 1990 revenue shares shuts down variation in exposure to state budget shocks that might be driven by endogenous institutional responses. For example, institutions might become more or less dependent on state appropriations over time based on changing selectivity, increased ability to attract out-of-state students, or other sources of unobserved heterogeneity.

The second factor in (2) calculates state appropriations per college-age (age 19-23) student in each state and year, using Grapevine data rather than institution-level appropriations from IPEDS. Restricting variation in state appropriations to the state-year level addresses the concern that schools receiving more or less funding within a particular state and year may differ in unobserved ways. For example, a budget cut for an individual institution may be more or less severe depending on the current political influence of its leadership. State legislatures might allocate additional funds to colleges in labor markets that have been hit particularly hard by economic downturns.

Our second source of identifying variation leverages legislatively imposed tuition caps and freezes. We use the information in Table A1 to code two instruments related to tuition caps. The first,  $TuitCap_{i,t}$ , equals one if institution  $i$  is subject to a cap or freeze in year  $t$ . The second,  $TuitMax_{i,t}$ , equals the maximum percentage increase allowed by the state legislature between years  $t - 1$  and  $t$  for institution  $i$ . For example, this variable equals zero for institutions subject to tuition freezes, and 0.1 for institutions where tuition growth is constrained to no more than 10 percent. We include both of these variables in our estimating equations and code  $TuitMax_{i,t}$  to zero for cases where  $TuitCap_{i,t} = 0$ . Thus the coefficient on  $TuitCap_{i,t}$  can be interpreted as the impact of a tuition freeze, i.e. when the maximum percentage increase allowed equals zero. The combination of these two variables allows us to exploit variation in both the existence and intensity of tuition cap legislation.

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to disburse to schools, while in other cases funding goes directly to schools.

To get a sense for which colleges are most affected by state budget shocks, Appendix Table A2 presents estimates of the correlation between institutional characteristics and baseline dependence on state appropriations (the first term in  $Z_{i,t}$  above) in a regression framework. Four-year, less-selective institutions are most reliant on state appropriations, for two reasons. First, many two-year colleges also receive funding from property taxes and other local sources. Second, selective four-year institutions are generally larger and have other sources of revenue such as research grants and endowment spending. Dependence on tuition revenue is also positively correlated with dependence on state appropriations, which is consistent with less-selective institutions having fewer margins of adjustment in response to a budget shock. Finally, larger institutions (as measured both by total spending and enrollment) draw a lower share of total revenue from state appropriations.

### 3.2 Assessing the Validity of the Instruments

We explore the validity of the instruments by testing for pre-trends with the following reduced form model:

$$Y_{i,t} = \alpha_i + \mu_t + \zeta_{s(i)} \times t + W'_{i,t} \theta + \sum_{\ell=-L}^L [\rho_{\ell}^Z Z_{i,t-\ell} + \rho_{\ell}^C TuitCap_{i,t-\ell} + \rho_{\ell}^M TuitMax_{i,t-\ell}] + v_{i,t}. \quad (3)$$

The vector  $W_{i,t}$  includes state and county unemployment rates by year, time-varying institution characteristics such as highest degree offered and eligibility to distribute Federal financial aid, county average demographic and economic characteristics, and interactions of these variables with time.<sup>11</sup> We also control for state-specific linear time trends, represented by  $\zeta_{s(i)} \times t$ . Standard errors for this analysis are clustered at the state-year level. Here and in our subsequent results, we divide  $Z_{i,t}$  by 1,000 for ease of interpretation. The coefficients on the leads of the policy variables check whether there are differential prior trends in outcomes for institutions that later experience differential changes in the instruments. Our baseline model includes 4 leads and 5 lags of each instrument and both unemployment rates (for ten years total), although our results are not sensitive to this particular number of years.

Figure 3 presents estimates of equation (3) for log enrollment, our key outcome of interest. The top panel shows coefficients and 95 percent confidence intervals for the budget shock instrument, while the middle and bottom panels show results for the tuition cap indicator and the maximum tuition increase. In contrast to the results using actual appropriations in Figure 2, we find no evidence of pre-trends in the relationship between the budget shock and log enrollment. The coefficients on all four leads are precisely estimated, near zero and not statistically significant. We fail to reject the hypothesis that all four pre-trend coefficients are jointly equal to zero ( $p = 0.64$ ). Additionally, we find a positive impact of the budget shock on log enrollment

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<sup>11</sup>The institutional covariates are sector, highest degree offered, Title IV eligibility, degree-granting status, urban status and indicators for missing values of these covariates in each year. These covariates rarely change within institutions over time, but we include them for completeness. The county covariates are log population, percent black, percent hispanic, percent male, percent below the poverty line, log median income, share with some college education, and share with bachelor's degree. County covariates are only available from the U.S. Census for 1990 and 2000, and from the ACS for 2005 and onward. To complete the county data, we linearly interpolate values for missing years.

in the same year. This estimate, which is statistically significant at the one percent level, implies that a \$1,000 increase in the instrument increases enrollment in year  $t$  by 2.8 percent. We also find statistically significant and positive impacts of the budget shock on enrollment in future years.

The second and third panels of Figure 3 show no evidence of pre-trends in the relationship between the tuition cap instruments and log enrollment. All of the pre-trend coefficients are close to zero and statistically insignificant, and we fail to reject the joint hypothesis that they are jointly equal to zero in both cases ( $p = 0.73$  and  $0.47$  for the tuition cap indicator and maximum tuition increase). Unlike the budget shock, however, we find no reduced form impact of tuition caps on log enrollment.

Figure 4 repeats the exercise above for another key outcome - the log of total degrees and certificates awarded. While the contemporaneous impact of the budget shock instrument on degrees and certificates is small, we find a large and statistically significant and positive impacts of a budget shock in year  $t$  on log awards in year  $t + 1$ . Moreover, the post-shock coefficients are mostly positive, and we can strongly reject the joint hypothesis that the coefficients on degrees and certificates in years  $t$  through  $t + 5$  all equal zero ( $p = 0.001$ ). Additionally, we fail to reject the joint hypothesis that the pre-trend coefficients are equal to zero ( $p = 0.35$ ), and there is no visual evidence of pre-trends. As with enrollment, the middle and bottom panels of Figure 4 show no consistent impact of tuition caps on total awards. While there is some evidence of a negative impact of a tuition freeze in later years, we fail to reject the hypothesis that all coefficients in years  $t$  through  $t + 5$  equal zero ( $p = 0.17$ ). Together, the results in Figures 3 and 4 show that our policy instruments are unrelated to pre-trends in key outcomes.<sup>12</sup>

The analysis to follow uses budget shocks and tuition caps as instruments for tuition and spending. To explore the first stage relationships underlying this strategy, Figures 5 and 6 present estimates of equation (3) for log total spending and log tuition. The top panel of Figure 5 shows clear evidence that the budget shock IV affects total spending. The coefficients in Panel A imply that a \$1,000 increase in the instrument increases spending in year  $t$  by 6 percent. We also find smaller but still statistically significant impacts on spending in the second and fifth years following the budget shock.

The middle and bottom panels of Figure 5 show some evidence that tuition caps affect spending. However, we also find negative coefficients on the leads of tuition caps and positive coefficients on the leads of the maximum tuition cap amount, which suggests that the impact of tuition caps on log spending may reflect a longer-run trend rather than a sharp policy response. This is less worrisome than a pre-trend for enrollment, because the key exclusion restriction required for our approach is that the instruments are conditionally uncorrelated with potential enrollment and degree outcomes. Nonetheless, the pre-trends in Figure 5 suggest that the tuition cap instruments are not as good as random conditional on our control variables. We address this issue in part by estimating first-differenced models, which focus on sharper yearly changes in the policy IVs and endogenous variables.

Figure 6 presents results for tuition prices. We find that a \$1,000 increase in the budget shock in year

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<sup>12</sup>Figures A2 and A3 assess the sensitivity of these findings to functional form by reestimating equation (3) with enrollment and awards in levels rather than logs. These analyses yield similar conclusions.

$t$  decreases tuition in year  $t$  by 4.8 percent and year  $t + 1$  by 5.8 percent. The impact is closer to zero in subsequent years. This suggests that public institutions may react to state budget cuts in part by increasing tuition to make up for lost revenue. The middle and bottom panels of Figure 6 show that the tuition cap instruments have bite: prices are significantly lower following the imposition of a tuition freeze. Moreover, conditional on the presence of a cap, the level of the cap is strongly predictive of yearly tuition increases. There is a strong and immediate impact of the instruments in all three panels of Figure 6.<sup>13</sup>

### 3.3 Two-stage Least Squares Framework

Public institutions can respond to budget cuts through a combination of price increases and spending cuts. Tuition caps generate independent variation in tuition prices. Thus we can use the budget shock together with the price cap instruments to disentangle the causal impacts of spending and tuition. To this end, we estimate 2SLS models relating changes in outcomes to changes in spending and tuition, instrumented with policy variables. The first stage equation for log spending is:

$$\Delta \log spend_{i,t} = \phi_{s(i)} + \omega_t + \Delta W'_{i,t} \lambda + \pi_1 \Delta Z_{i,t} + \pi_2 \Delta TuitCap_{i,t} + \pi_3 \Delta TuitMax_{i,t} + \eta_{i,t}. \quad (4)$$

This equation relates the change in spending relative to the previous year to changes in the budget shock and tuition cap instruments, controlling for state and year fixed effects and changes in covariates. The first stage equation for changes in tuition replaces the change in log spending with the change in log tuition on the left-hand side of (4).

The second stage equation is:

$$\Delta Y_{i,t} = \Phi_{s(i)} + \Omega_t + \Delta W'_{i,t} \Lambda + \beta_1 \Delta \log \widehat{spend}_{i,t} + \beta_2 \Delta \log \widehat{tuition}_{i,t} + \epsilon_{i,t}, \quad (5)$$

where  $\Delta \log \widehat{spend}_{i,t}$  and  $\Delta \log \widehat{tuition}_{i,t}$  are predicted changes in log spending and log tuition from the first stage. Compared to the fixed effects specifications discussed in Section 4.3, the first-differences specifications in (4) and (5) focus on the impacts of sharp yearly changes in the instruments on the endogenous variables and outcomes. When using only two years of data, the two approaches are equivalent; in our longer panel, the first differences model may be less efficient but captures causal effects under less restrictive assumptions. In general, our results are similar when we estimate 2SLS models in first differences or in levels with institution fixed effects, year fixed effects and state-specific linear trends as in equation (3).<sup>14</sup>

<sup>13</sup>The middle panel of Figure 6 shows some evidence of a pre-trend in the relationship between tuition caps and tuition - but in the opposite direction of the estimated impact of imposing a cap. This suggests that tuition caps may be imposed in response to particularly high tuition in a prior year. Since pre-trends can be interpreted as evidence of anticipation effects (e.g. Malani and Reif 2015), one possible concern is that students may time enrollment decisions in response to anticipated price changes. We address this concern in two ways. First, we estimate 2SLS models in first differences, which focuses attention only on sharp yearly changes in tuition. Second, we address the sensitivity of the differences specification by excluding the first lead and using the difference between the 2nd lead and the base year as the endogenous first stage variable in Section 4. This leaves the results nearly unchanged.

<sup>14</sup>Appendix Table A6 presents results with 2SLS models in levels rather than first differences, for log enrollment (our main outcome). The results are qualitatively similar - although somewhat larger in magnitude - to our main results.

## 4 Effects of Price and Spending Subsidies

### 4.1 Two-stage Least Squares Estimates for Enrollment

Table 2 reports first-stage impacts of the budget shock and tuition cap instruments on log spending and tuition, along with second-stage impacts on log enrollment. The first stage effect of the budget shock on spending, reported in column 1, equals 0.061. This implies that a \$1,000 increase in the instrument increases total spending by about 6.1 percent. Since mean total spending is about \$104 million per year for the institutions in our analysis sample, a 6.1 percent increase is about \$6.34 million in additional spending. In contrast, the tuition cap variables are weakly related to log spending.

As shown in column 2, a \$1,000 increase in the budget shock decreases log tuition by 8.1 percent, or about \$303. Tuition freezes lower in-state tuition by about 3 percent. The stringency of the cap strongly predicts the size of the tuition change: a ten percentage point increase in maximum tuition growth leads to a 3 percent increase in tuition. Both instruments are jointly highly statistically significant, and Angrist and Pischke (2009) partial  $F$ -statistics indicate that the instruments generate substantial independent variation in log tuition and log spending.

The second-stage estimates imply that enrollment is more sensitive to spending than to tuition. As can be seen in column 3 of Table 2, a 10 percent increase in log spending increases current-year enrollment by 3 percent, and this estimate is statistically significant at the 5 percent level. In contrast, the estimated elasticity of enrollment with respect to tuition is close to zero and statistically insignificant. The standard errors allow us to reject impacts of a 10 percent tuition cut larger than about 1.7 percent. We can also reject that the elasticities of enrollment with respect to tuition and spending are equal in absolute value ( $p < 0.01$ ). Importantly, this null result for tuition holds despite the strong first stage predictive power of the instruments shown in column 2.

### 4.2 Enrollment Effects Over Time

Table 3 reports impacts on log enrollment for up to three years after changes in tuition and spending. Columns 1 through 4 report separate estimates for years  $t$  through  $t + 3$  based on the 2SLS framework in equations (4) and (5). Differences for the outcomes here are taken relative to the base year before the policy variables are measured, so that the specification for year  $t + 1$  uses  $Y_{i,t+1} - Y_{i,t-1}$  as the dependent variable, and similarly for other years. Panel A shows results for the full sample of institutions. Overall, we find that spending increases have larger impacts on enrollment in subsequent years. The coefficient on log enrollment increases from 0.304 in the base year to 0.796 one year after the change ( $p < 0.001$ ), with modest increases thereafter. This suggests that the new students induced to attend by changes in state funding persist for multiple years of enrollment. In contrast, the coefficients on tuition in subsequent years are small, never statistically significant, and sometimes wrong-signed.

Panels B and C show results separately for two- and four-year institutions. In both cases, we find large

and statistically significant impacts of spending on enrollment, but little or no impact of price. We find larger impacts of spending at two-year institutions than at four-year institutions. The coefficients in Panel B for two-year institutions suggest that a 10 percent increase in total spending in year  $t$  boosts enrollment by around 10 percent in years  $t + 1$  through  $t + 3$ . Corresponding estimates for four-year institutions are smaller but still substantial: a 10 percent increase in spending in year  $t$  increases enrollment by 2.4 percent in year  $t$ , 4.7 percent in year  $t + 1$ , 6.6 percent in year  $t + 2$  and 5.7 percent in year  $t + 3$ . All four of these estimates are statistically significant at the one percent level.

While these estimates are large, it is important to note that they reflect increases in total enrollment-years, not individual students. If changes in spending induce students to matriculate or to persist through college, this will increase enrollment counts in multiple years. Additionally, the log differences specification implies that a spending shock in year  $t$  leads to a persistent 8 percent enrollment gain relative to year  $t - 1$ , not continued growth of 8 percent in each subsequent year. A related issue is that spending shocks are serially dependent, so that changes in state funding are likely to persist for multiple years. We discuss this issue further in Section 5.

The outcomes in Table 3 capture total fall enrollment, including part-time, non-degree and graduate students. Appendix Table A3 presents additional results for part-time and full-time undergraduate enrollment. Undergraduates account for about 92 percent of total enrollment in the sample. About 45 percent of undergraduates are enrolled part-time, and this breaks down sharply by institution type, with 63 percent of undergraduates enrolled part-time at two-year colleges compared to only 24 percent at four-year colleges. We find that the large impacts of spending at two-year institutions are driven by increases in part-time enrollment, while impacts at four-year institutions are due to increases in both part-time and full-time undergraduate enrollment. Moreover, while most of the estimated tuition elasticities are small and statistically insignificant, Appendix Table A3 shows statistically significant effects of price changes on part-time enrollment at four-year institutions. This is consistent with results reported by Seftor and Turner (2002), a study that finds greater price sensitivity among older, independent students who are more likely to be enrolled part-time.

Appendix Table A4 presents enrollment results by race, with nonwhite students (pooled together) in Panel A and white students in Panel B. Overall, we find very similar impacts of price and spending changes by student race. Unfortunately, data on degree completion by race is unavailable.

### 4.3 Impacts on Certificates and Degrees

Table 4 present results for log total awards. Panel A shows results for all institutions, while Panels B and C present results for two-year and four-year institutions respectively. We find that a 10 percent increase in total spending in year  $t$  leads to only a 2 percent increase in total awards in year  $t$ , but larger increases of 7.8, 9.4 and 6.4 percent in years  $t + 1$  through  $t + 3$ , respectively. The impacts on log total awards in columns 2 through 4 of Panel A are all statistically significant at the 5 percent level.



Panel B presents results for total awards in two-year institutions, which are comprised predominantly of certificates and associate’s degrees. We find especially large elasticities in years  $t + 1$  and  $t + 2$ , where a 10 percent increase in total spending increases awards by 14.5 and 14.6 percent respectively. In results not reported, we find modestly higher elasticities for certificates, although both award types are individually statistically significant and not distinguishable from each other.

Panel C reports results for log total awards in four-year institutions. Because we exclude graduate degrees from our analysis, awards in four-year institutions are comprised almost entirely of bachelor’s degrees.<sup>15</sup> We find positive impacts of spending on four-year college degrees that grow modestly with time. The results for years  $t + 2$  and  $t + 3$  are statistically significant at the 5 percent and 10 percent level respectively. The coefficients imply that a 10 percent increase in total spending in year  $t$  increases the number of four-year degrees awarded by 4.6 percent in year  $t + 2$  and 4.5 percent in year  $t + 3$ . While not shown, the coefficients remain stable in later years, although the standard errors grow as the sample size shrinks.

The four-year institutions in our sample spend about \$231 million and give out an average of 1,508 bachelor’s degrees (and 1,660 total awards) per year. Thus if we combine the coefficients in columns 1 through 4 of Panel C to obtain the cumulative impact of a spending change in year  $t$ , the implied cost per bachelor’s degree is \$102,532. In contrast, there is no statistically significant impact of price changes on log total awards in any year, either for the pooled specification of all institutions (Panel A) or for two-year or four-year institutions individually (Panels B and C). Appendix Table A5 presents results for log enrollment and log total awards in four-year colleges, dividing the sample relatively evenly by selectivity.<sup>16</sup> We find modestly larger results for less selective four-year institutions, although the results are statistically indistinguishable from each other.

#### 4.4 Other Robustness Checks

In Appendix Table A6 we assess the robustness of the results to a variety of different specification choices. For purposes of comparison, the first row replicates the baseline results for log enrollment from Table 2. The second row adds state-specific linear trends, the third row replaces the trends with state-by-four year intervals (1991 to 1994, 1995 to 1998, and so on), and the fourth row includes institution fixed effects. All three additions have little impact on the main results. Rows 5 through 7 estimate the 2SLS system in equations (4) and (5) but in levels rather than changes, similar to the event study specification in equation

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<sup>15</sup>About 91 percent of all awards in four-year institutions (excluding graduate degrees) are bachelor’s degrees. A small number of institutions in our sample are classified as four-year institutions yet give out sub-baccalaureate credentials as well. In some cases, these institutions switch from awarding only one type of degree to multiple types and vice versa. This causes some instability in results that are disaggregated by degree type and logged (although the results are quite stable in levels). Thus for simplicity we sum over all awards in four-year institutions. Models that restrict the sample to institutions which only ever award bachelor’s degrees are yield very similar results to those in Table 4.

<sup>16</sup>The definition of selectivity in Table 1 includes mostly flagship and relatively selective public universities. Appendix Table A5 defines selective institutions as those with a Barron’s ranking of “Most Competitive” or “Highly Competitive”, an in-state enrollment share of less than 90 percent, and an admissions rate of less than 60 percent. This splits the four-year sample roughly in half. In-state enrollment data are available from IPEDS in every other year starting in 1994. Admission rates are available from IPEDS beginning only in 2002. Due to these data limitations, we adopt the simpler definition of selectivity in the results in the main paper.

(3). Row 5 estimates a simple panel data model with institution and year fixed effects, while row 6 adds state-specific linear trends and row 7 adds state-by-four year fixed effects. Overall, our results are robust to many different specification choices.

As a final sanity check, we estimate models replacing the budget shock instrument with actual appropriations awarded to institution  $i$  in year  $t$ , despite the endogeneity concerns discussed in Section 2.3. Appendix Table A7 shows results using actual appropriations for our two main outcomes - log total enrollment and log total awards. While the point estimates differ somewhat from our main results (and the standard errors are much smaller), the basic pattern of large impacts of spending changes but no impact of price changes holds in this specification as well.

## 5 Additional Results and Discussion

### 5.1 Spillover Impacts on Other Institutions

A key question is whether price and spending changes in public institutions have spillover impacts on nearby institutions. Our estimates could reflect net changes in enrollment and degree receipt in response to funding shocks, but they could also be driven by movement of students across institutions.

We first test for spillovers by estimating the impact of public budget shocks and tuition caps on enrollment in private institutions. Specifically, we compute the average (enrollment-weighted) budget shock for all public institutions in a county, and create an indicator for whether any public institution in the county was operating under a tuition cap. We then estimate the differences model in equation (4) for private institutions with these modified policy variables on the right-hand side. Unfortunately, data limitations preclude us from studying spillovers for any outcome prior to 2000 due to inconsistent data collection from private institutions in earlier years. We therefore estimate impacts of public spending and tuition policies on enrollment, degrees and prices at private schools beginning in the year 2000.

The results in Table 5 show limited evidence of spillovers. Panel A shows impacts on log total enrollment in private (not-for-profit and for-profit) institutions. A comparison of the year  $t$  enrollment impact in column 1 of Panel A to the reduced form impact of the budget shock IV on public enrollment suggests that crowd-out of private school enrollment accounts for approximately two thirds of the effect of the shock on current enrollment. However, this same calculation for years  $t + 1$  and later yields spillover effects that are near zero and sometimes in the wrong direction. Summing the coefficients in columns 1 through 4 of Table and comparing them to the sum of the reduced form impacts from columns 1 through 4 of Table 3 (estimated on a post-2000 sample for consistency) yields an implied spillover of about 15 percent.

This pattern of results is quite similar to Cellini (2009), who finds that for-profit college enrollment responds to changes in local funding of community colleges in California. Cellini (2009) finds a large private response in the first year of bond passage, but no evidence of consistent impact after the first year. Interestingly, we do find somewhat larger impacts on private institutions when we restrict the sample to California.

However, our confidence intervals for the full sample do not allow us to reject estimates that are equal to the magnitudes in Cellini (2009).

Panel B shows impacts on log total awards for private institutions. Here we find some consistent evidence of spillovers. The coefficients in Panel B, while not always statistically significant, are consistently negative in sign. A comparison of the impacts in Panel B to the reduced form impacts of the budget shock IV on log total awards in public institutions (estimated on a post-2000 sample) implies that about 35 percent of the impacts on public institutions reflects crowd-out of private institutions. Appendix Table A8 - which shows separate results by degree type - reveals that nearly all of the crowdout of total awards is driven by associate's degrees, with a small role for certificates and no impact at all on bachelor's degrees.

We find some suggestive evidence that public tuition caps lower private enrollment and degree receipt in later years, although the magnitudes are small and the point estimates are usually not statistically significant. Finally, in Panel C we find no impact of public budget shocks or tuition caps on tuition in private institutions.

In principle, spillovers of budget shocks could also occur across nearby public institutions in the same area. In Appendix Table A9 we estimate models with the county-average budget shock described above but for public institutions. This effectively treats the county as the market in which budget shocks operate. These results are very similar to our main results, although somewhat larger in magnitude. This suggests that our findings are not driven by students sorting across nearby public institutions in response to funding shocks.

## 5.2 Impacts On Financial Aid and Spending by Category

Our price cap instruments affect tuition sticker prices, which are not necessarily equal to the net prices offered to students. Thus one possible explanation for our small estimated tuition effects is that colleges adjust sticker and net prices differently in response to tuition caps. For example, institutions might offset overall price changes by targeting additional financial aid to students who are most likely to benefit from it. Relatedly, a possible mechanism for the positive impacts of spending increases is that the extra state funds are used to price discriminate more effectively by targeting marginal students.

We test for changes in financial aid in Table 6 by estimating the 2SLS system in equations (4) and (5) with institutional spending and revenue categories as outcomes. Panel A shows results for all institutions, while Panels B and C split the sample into two-year and four-year institutions. The first three columns show results for institutional scholarships, Pell Grants, and total financial aid. We find no statistically significant impacts of changes in prices or spending on financial aid for the full sample of institutions. However, we do find that a statistically significant and positive impact of spending on institutional scholarship aid for four-year institutions. Since scholarship aid is about 4.6 percent of total spending at public institutions, the point estimate of 1.320 implies that a \$1 increase in spending yields about 6.1 cents in additional scholarship funds. Targeted financial aid can therefore account for a small share of the impact of spending on enrollment

and degree completion at four-year institutions.

The null impacts on total financial aid in Column 3 imply that public institutions are generally unable to buffer changes in tuition sticker prices with scholarships and other sources of aid. This suggests that changes in the posted tuition price generally lead to changes in net price, although we cannot rule out the possibility that schools use additional funds and tuition increases to engage in revenue-neutral price discrimination.

Interestingly, we find little impact of changes in price or spending on Pell Grant aid. Although the estimated price effects here are not generally statistically significant, they are consistently negative. This is puzzling, because price increases should be partially offset by an increase in Pell Grant aid dollars. In fact, in a 1987 *New York Times* opinion piece, then-Secretary of Education William Bennett famously claimed that U.S. colleges and universities intentionally raise tuition to capture Federal aid dollars (Bennett, 1987). While Cellini and Goldin (2014) find evidence that for-profit institutions raise tuition in response to increases in Federal aid, Turner (2013) finds that public institutions capture less than 5 percent of Pell Grant increases through price discrimination. One possible explanation is that tuition increases lead to compositional changes in the student body, with fewer Pell-eligible students enrolling and thus a reduction in total Pell Grant dollars.

Columns 4 through 6 of Table 6 present results for selected categories of institutional spending. In general, we find increases in core academic spending categories that are roughly proportional to the total increase in spending. Instructional spending constitutes about 41 percent of total spending across all public institutions in the sample, and so the estimates in Column 4 imply that instructional spending increases by about 40 cents for every dollar of total spending. We find particularly large increases in academic support spending, a category that includes activities such as tutoring, mentoring and counseling services. The estimates here indicate that a 10 percent increase in total spending increases academic support spending by 17.2 percent, which implies an increase of about 11 cents for every dollar. Overall, the results by spending category in Table 6 suggest that changes in state funding have large impacts on core academic spending at non-selective public institutions.

### 5.3 Interpreting Magnitudes

Our results can be used to compare the impact of tuition subsidies to revenue-neutral increases in per-student spending. For illustrative purposes, consider the impact of a \$1,000 per student tuition subsidy on current enrollment. The estimates from Table 2 imply that this would increase enrollment by a total of 40 students, and would cost the average institution about \$8.3 million in revenue.<sup>17</sup> The equivalent dollar amount distributed as a spending subsidy would increase total enrollment-years - among both new and returning students - by 202.<sup>18</sup> Thus spending has a larger per-dollar impact than price on total enrollment,

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<sup>17</sup>Mean enrollment in our sample is 8,428 and mean inflation-adjusted tuition is \$3,796, yielding total revenue of \$32.0 million. A \$1,000 tuition cut lowers tuition to \$2,796 and increases enrollment by  $(1,000/3,796) \times 0.0017 = 0.45$  percent, or about 40 students, yielding total revenue of  $(8,428 + 40) \times (2,796) = \$23.7$  million in year  $t$ .

<sup>18</sup>\$8 million represents a spending increase of about  $(8.3/104) = 8.0$  percent for the mean public institution in our sample. This spending increase multiplied by the coefficient on enrollment in Table 2 yields an enrollment increase of 2.4 percent, or

even contemporaneously. The larger coefficients on spending relative to price in subsequent years shown in Table 4 magnify this conclusion.

The estimates in Table 4 can also be used to compare the cost of increasing degree attainment via public spending to projections of the lifetime earnings gains associated with a college degree. As discussed in Section 4.3, the coefficients in Table 4 yield an implied cost of \$102,532 per additional bachelor's degree. A complication is that budget shocks are serially dependent, so that increased spending in one year translates into further spending in future years. In results not reported, we adjust for the persistence of budget shocks and degree effects by directly estimating the extent of serial correlation, and find a total cost per degree equal to roughly \$155,451.<sup>19</sup> For purposes of comparison, dividing average total yearly spending at non-selective four-year institutions by the average number of bachelor's degrees awarded per year yields an implied cost of \$141,516 per degree. The average cost of a bachelor's degree is therefore relatively close to the implied marginal cost from our estimates.

Avery and Turner (2012) estimate lifetime net returns to bachelor's degree receipt under a variety of assumptions about discount rates and heterogeneous returns to ability. Their most conservative projections imply returns of around \$250,000 in present value, with the median estimate around \$450,000 for women and \$600,000 for men. Thus the private return on public investment in the production of bachelor's degrees passes a benefit-cost test under conservative assumptions.

Another way to assess the magnitude of our estimates is to compare them to the cross-sectional relationship between per-student spending and bachelor's degree completion rates at four-year institutions. We regress degree completion rates for the four-year institutions in our sample on log total spending and log tuition, controlling for other covariates.<sup>20</sup> This regression shows that a 10 percent increase in total spending is associated with a 0.52 percentage point increase in the graduation rate ( $p < 0.01$ ). A 0.52 percentage point increase yields an additional 58.1 bachelor's degrees per year for a four-year institution of average size. By comparison, the estimates in Table 4 imply that a 10 percent increase in spending generates about 55.6 additional bachelor's degrees per year.<sup>21</sup> Thus the cross-sectional relationship between spending and degree completion closely matches the magnitude of the IV estimates in Table 4. This suggests that the observed correlation between spending and graduation rates for non-selective institutions may be largely due to causal effects of spending rather than selection bias.

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about 202 students.

<sup>19</sup>We assess persistence by estimating versions of equation (3) treating future budget shocks as outcomes. This exercise shows that budget shocks are persistent; for example, regressing  $Z_{i,t+1}$  on  $Z_{i,t}$  plus five additional lags and lags of the tuition cap instruments yields a coefficient of 0.55, suggesting that about 55 percent of the budget shock persists into the next year. Summing the coefficients from similar models for years  $t+1$  through  $t+4$  yields a total of 0.94. This suggests that accounting for serial dependence roughly doubles the impact of the budget shock IV on costs. Dividing the sum of the coefficients on bachelor's degrees for years  $t$  through  $t+4$  (1.911) by 1.94 equals 0.985, which implies a cost of \$155,451 per bachelor's degree. The dollar cost per degree would be somewhat smaller if we considered impacts in years  $t+5$  or later.

<sup>20</sup>This exercise uses the 200 percent IPEDS graduation rate file, which computes the share of an initial cohort of full-time, degree-seeking undergraduates that complete a bachelor's degree within 6 years of entry. IPEDS began collecting graduation rate data in 2002. In addition to log total spending and log tuition, we also control for county demographics, institution sector and selectivity, highest degree offered, urban status, and state and year fixed effects.

<sup>21</sup>We obtain this figure by average the coefficients in Panel C of Table 4 together - which yields a coefficient of 0.369 - and multiplying it by the mean number of bachelor's degrees in four-year public institutions in our sample (1508) divided by 10.

## 5.4 Discussion

The pattern of results reported here helps explain several trends and stylized facts in U.S. higher education. In view of the recent decline in spending at public institutions, our findings provide a possible explanation for increases in time to degree at nonselective public institutions, as well as the decline of college completion rates over the last 25 years (Turner, 2004; Dynarski, 2008; Bound et al., 2010, 2012). The importance of spending for increasing graduation rates also explains the dramatic success of programs like the Accelerated Study in Associate Programs (ASAP), which provided comprehensive academic and support services to students in the City University of New York (CUNY) community college system. A randomized evaluation found that CUNY’s ASAP program increased associate’s degree completion rates by 84 percent (Scrivener et al., 2015). However, the program also increased average spending per full-time equivalent student by about 67 percent over two years (Levin et al., 2012). Taken together, results from these other interventions are consistent with the importance of public spending subsidies for degree attainment.

While it is easy to understand why price changes might affect student enrollment choices, the impact of spending is less obvious. One possibility is that students observe spending increases through smaller classes, increased course offerings or other amenities, and make matriculation decisions accordingly. This seems unlikely to be the main explanation for our results, for three reasons. First, while we do find contemporaneous impacts of spending on total enrollment, these are smaller than impacts in subsequent years. This suggests that the impacts of spending on enrollment might be driven by longer-run changes in course staffing or program offerings. Second, since median time to bachelor’s degree completion in the U.S. is about five years, large causal impacts on bachelor’s degrees awarded two and three years after a spending increase suggests that the mechanism is persistence among already-enrolled students. Third, in Appendix Table A10 we present suggestive evidence of larger impacts on enrollment for upper division students compared to freshmen.<sup>22</sup>

Spending cuts could affect persistence and degree completion through formal capacity constraints at overcrowded public institutions. For example, Bound et al. (2012) find that open-access institutions in California have turned students away in recent years because of budget cuts. This mechanism is unlikely to fully explain our results, however. A literature review and Lexis-Nexis newspaper search turned up little evidence of formal capacity constraints, and all of the existing evidence comes from recent years and from a very small number of states. In addition, the fact that we find increases in persistence for currently enrolled students suggests that not all of our results can be explained by formal capacity constraints.

Our results are consistent with the much broader trend of informal capacity constraints in public institutions, including reduced course offerings, long waitlists, little or no student guidance, and larger class sizes (Bahr et al., 2013). Bound and Turner (2007) argue that informal capacity constraints caused by “cohort crowding” dilute college quality, while Bound et al. (2010) argue that resources per student and other supply

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<sup>22</sup>Appendix Table A10 shows significantly larger impacts in year  $t$  on upper division enrollment compared to freshmen. However, these estimates become difficult to interpret over multiple years of data because upper division enrollment of future students is conditional on them first being a freshman. For this reason, the estimates in Appendix Table A10 for later years should be interpreted with caution.

side factors explain a large portion of the decline in college completion rates between 1972 and 1992. Bound et al. (2012) assemble qualitative evidence from 12 states which suggests that inadequate student advising, decreased course availability and overcrowding have contributed to recent increases in time to degree in public universities. Unfortunately, IPEDS data do not contain information about course waitlists or student advising loads. The fact that we find particularly large increases in academic support spending, and that student advising and mentoring programs have been highly successful in other settings, suggests that informal capacity constraints may be an important driver of our results (Angrist et al., 2009; Bettinger and Baker, 2011; Carrell and Sacerdote, 2013; Barrow et al., 2014; Scrivener et al., 2015).

## 6 Conclusion

This paper studies the impacts of changes in prices and spending on U.S. postsecondary attainment. Using differential exposure to state budget shocks across institutions combined with a newly assembled data set of legislative tuition caps, we estimate the joint impacts of changes in prices and spending on enrollment and degree completion. We find large causal impacts of spending - but not tuition - on these outcomes. This pattern holds for both two-year and four-year institutions and across all types of degrees and certificates.

Our results suggest that efforts to reduce college costs - holding spending constant - may have little impact on degree attainment. Broadly speaking, this pattern of results helps to explain why the move of many states over the past several decades toward a lower-spending, lower-tuition equilibrium has led to increases in the share of students who are enrolled part-time, and to higher college dropout rates.

Cohodes and Goodman (2014) show that a Massachusetts scholarship program that induced students to switch from better-resourced private colleges to Massachusetts public institutions caused a decline in graduation rates. This suggests that gaps in outcomes between higher- and lower-spending institutions may partly reflect a causal impact of school resources. Consistent with this evidence, we find a large impact of spending changes *within* institutions on degree completion, and across the broad range of public colleges and universities that are attended by a majority of U.S. students. Recent evidence suggests that mid-tier public institutions in our sample are important mediators of intergenerational mobility in the U.S. (Chetty et al., 2017). Our findings indicate that spending has large effects on educational outcomes for these institutions.

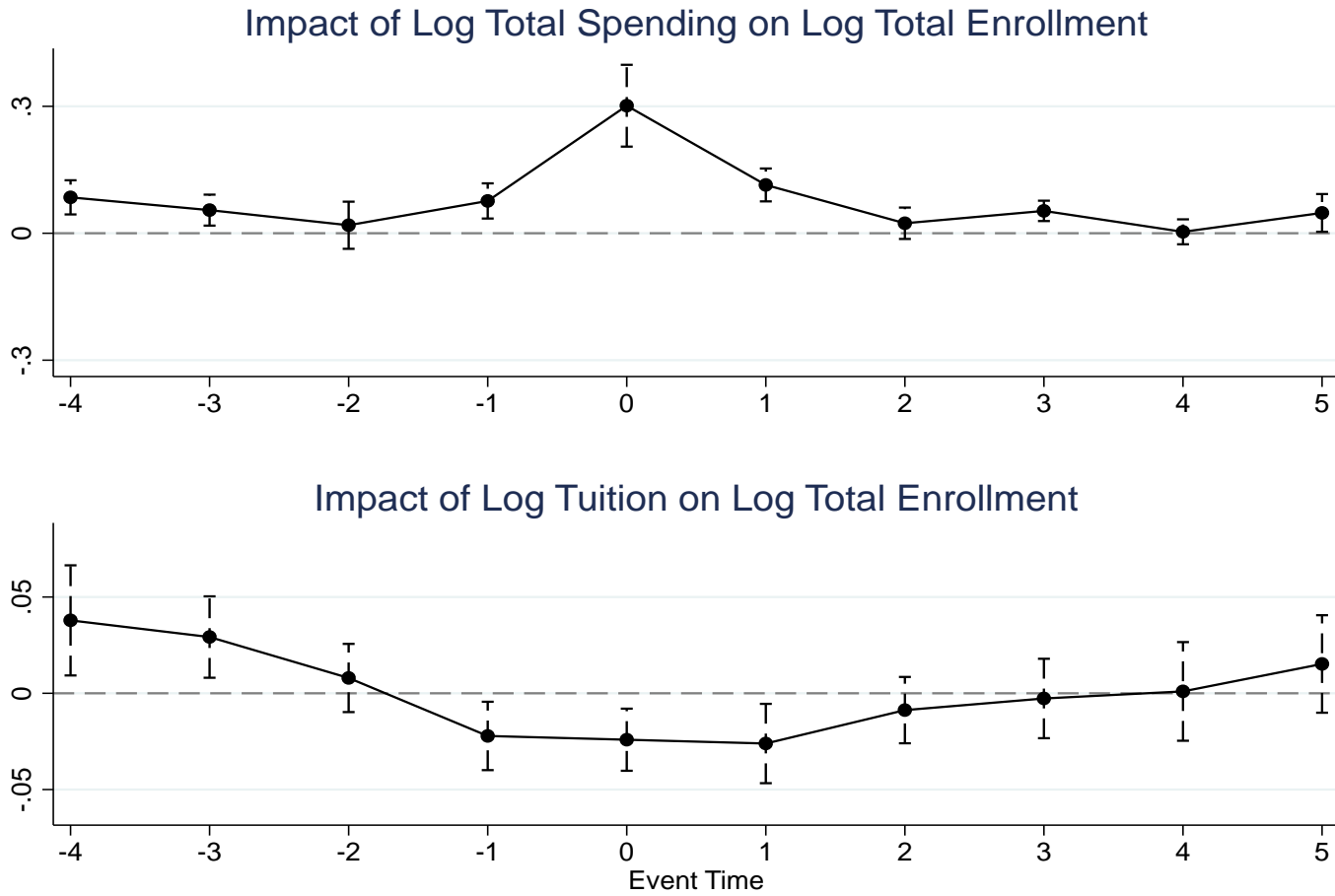
The Federal government has played an increasing role in the financing of higher education since 2000. Nearly all Federal dollars are allocated toward price reduction in the form of Title IV aid, with the Pell Grant becoming much more generous in recent years. Our results suggest that for the purposes of increasing educational attainment, Federal support of higher education might be allocated more effectively as a supply side subsidy provided directly to public institutions, similar to the Federal Title I program for K-12 schooling (see, e.g., Deming, 2017). It is also worth noting that Federal financial aid policy creates incentives for state policymakers to reduce higher education funding, because some share of state budget cuts can be passed on as tuition increases and returned to students in the form of unmet Federal need (Kane et al., 2003). This

contrasts with Federal support of Medicaid, which creates incentives for states to increase spending because it is structured as a matching grant.

Broadly, our findings suggest that government programs aimed at reducing college costs will not increase degree attainment if cost reduction is achieved by reducing per-student spending. Contrary to the narrative of administrative bloat, spending cuts affect core instruction and academic support, generating large downstream impacts on educational attainment. An important caveat is that our results are identified mostly from variation within non-selective public institutions, where per-student spending is relatively low and extravagant consumption amenities are rarely found.

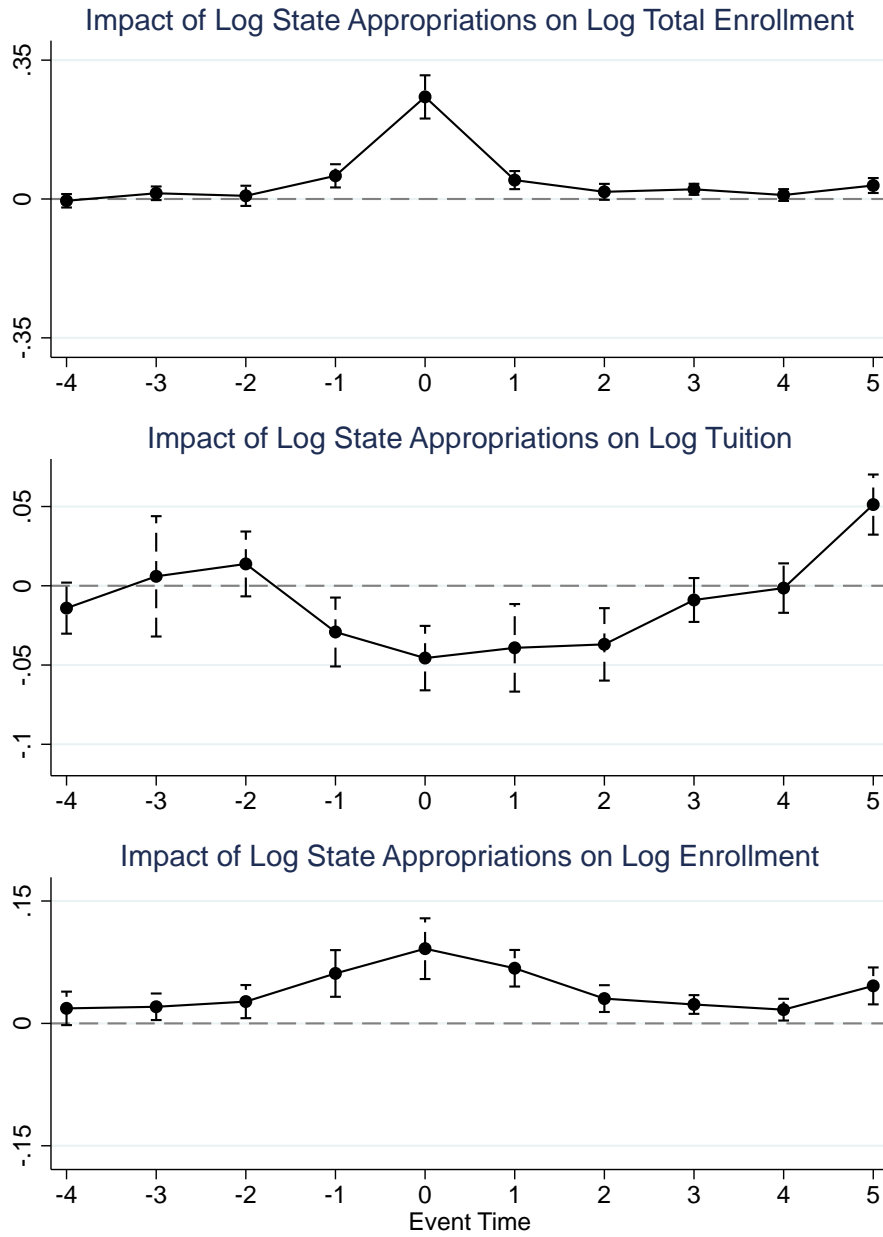


Figure 1



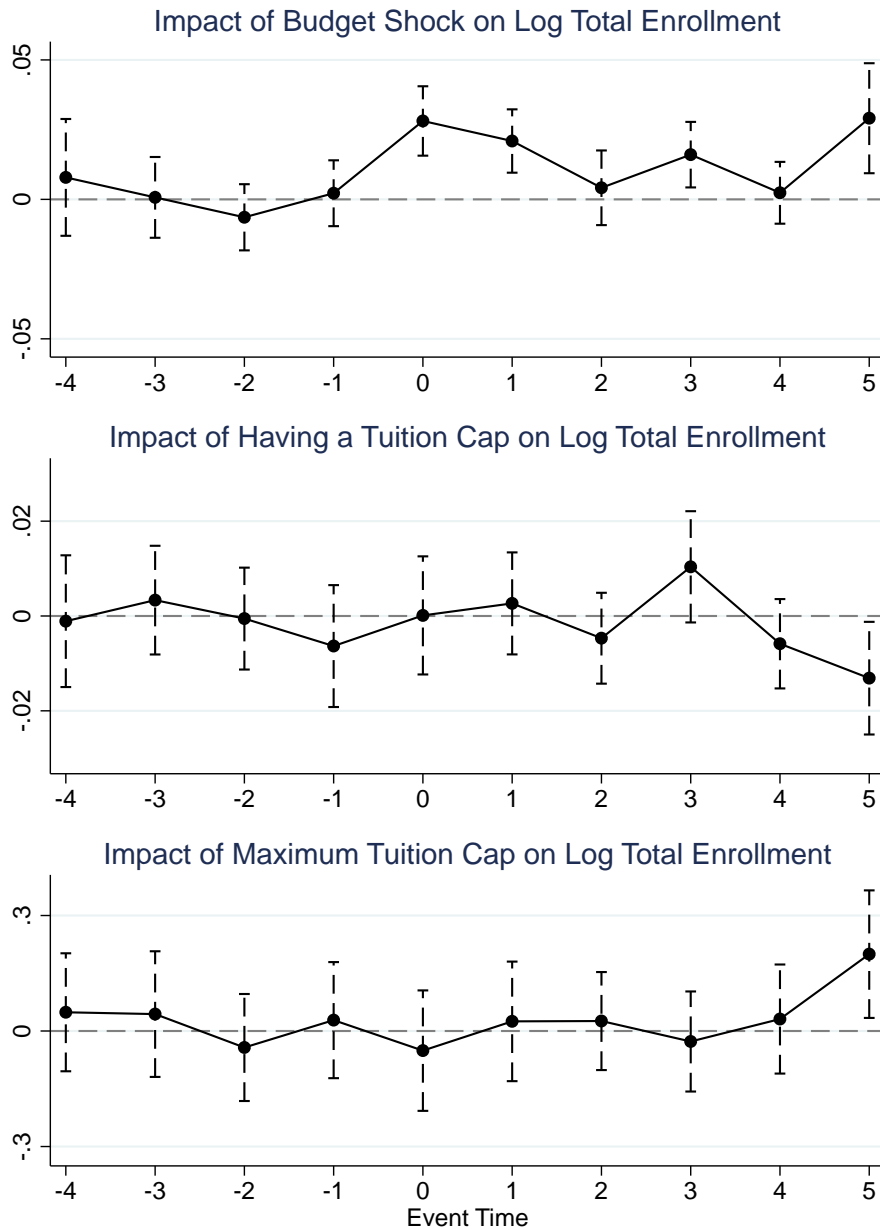
Notes: This figure presents estimates and 95 percent confidence intervals from regressions of log total enrollment on lags and leads of log total spending (top panel) and log tuition (bottom panel) for public institutions. Models also control for institution and year fixed effects. Standard errors are clustered at the institution level.

Figure 2



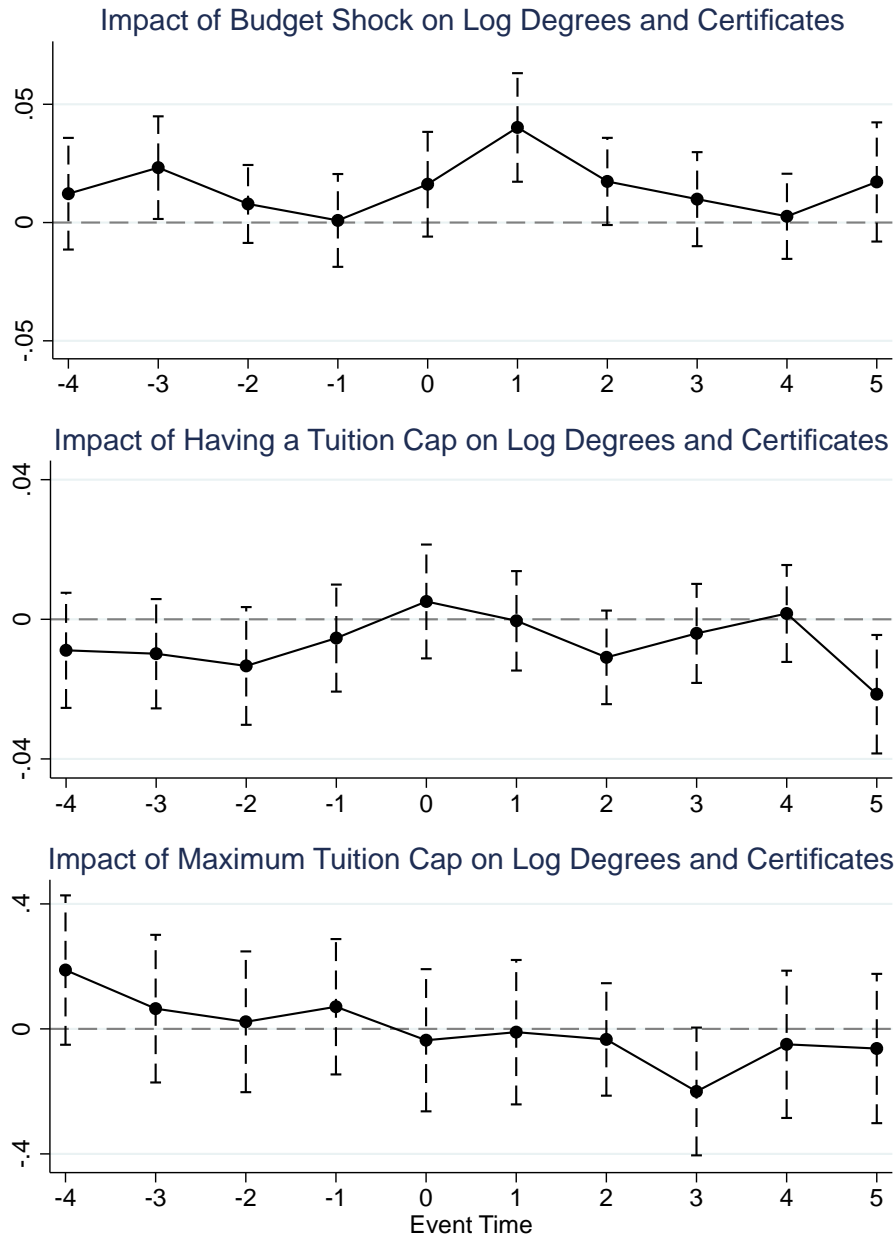
Notes: This figure presents estimates and 95 percent confidence intervals from regressions of log total spending (top panel), log tuition (middle panel), and log enrollment (bottom panel) on lags and leads of state appropriations for public institutions. Models also control for institution and year fixed effects. Standard errors are clustered at the institution level.

Figure 3



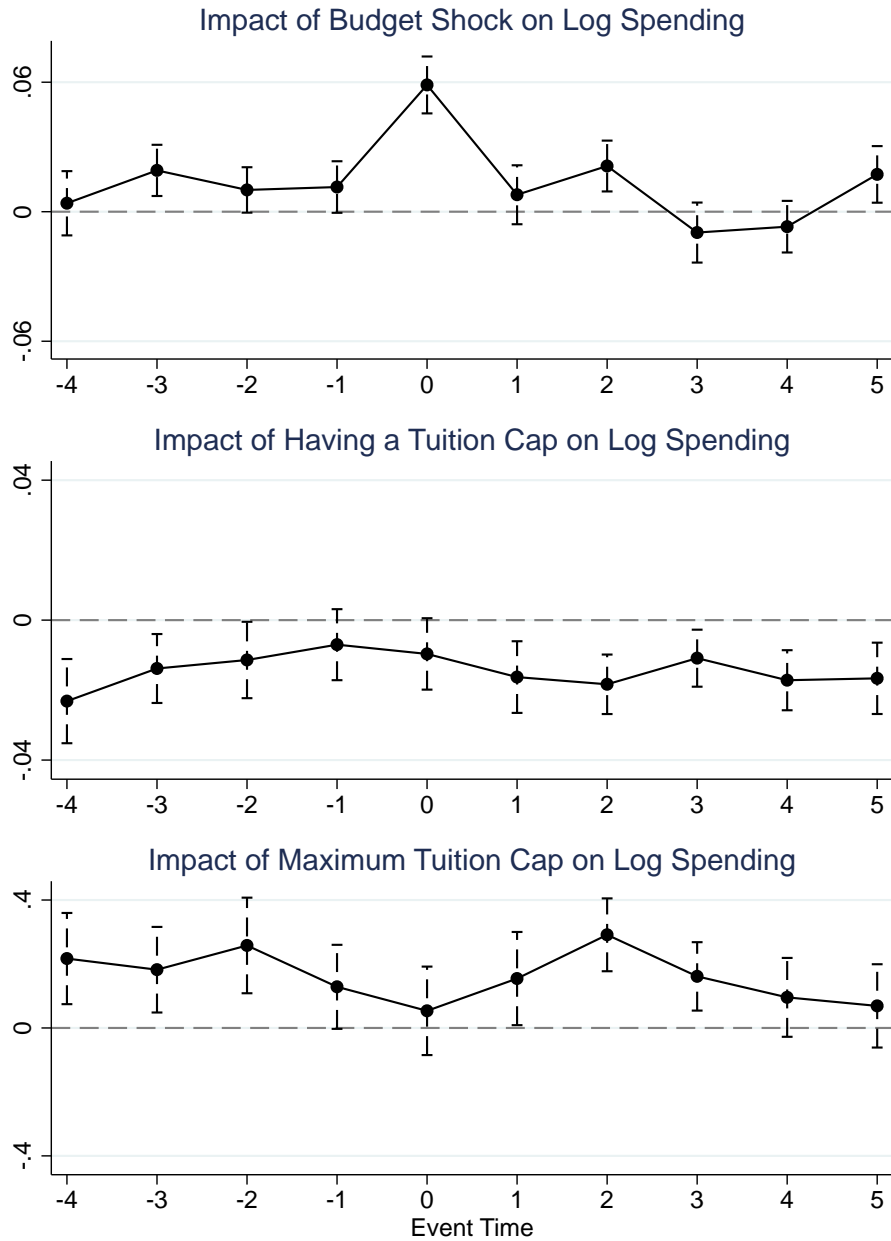
Notes: This figure presents estimates and 95 percent confidence intervals from a regression of log total enrollment for public institutions on lags and leads of the state budget shock instrument (the share of each institution's revenue from state appropriations in 1990 times yearly state appropriations per capita, in \$1000s), an indicator for whether a tuition cap is in place, and the maximum percentage increase allowed under the cap (set to zero when no cap is in place). The model also controls for lags and leads of county and state unemployment rates, time-varying county and institution characteristics, institution fixed effects, year fixed effects, and state-specific time trends. Standard errors are clustered at the institution level.

Figure 4



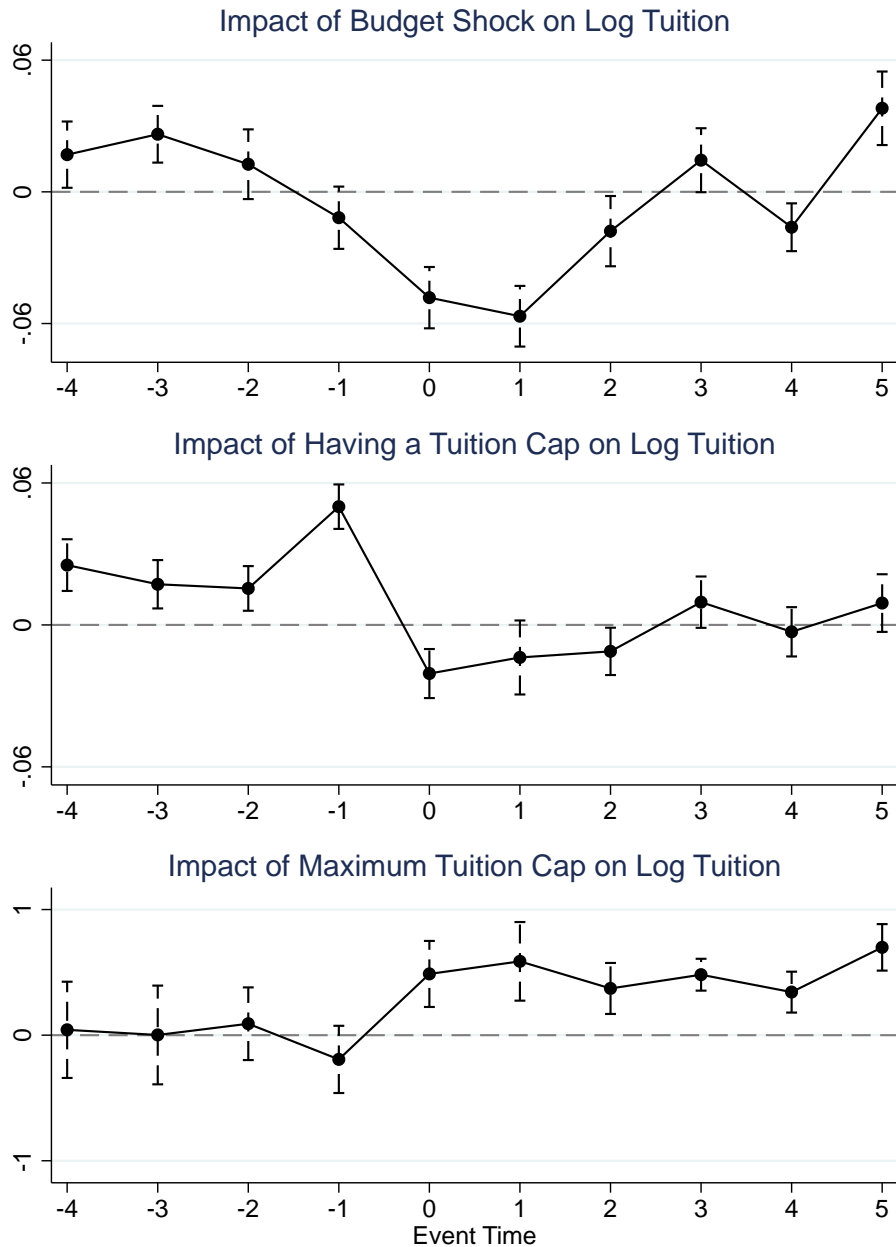
Notes: This figure presents estimates and 95 percent confidence intervals from a regression of log total degrees awarded on lags and leads of the state budget shock instrument (the share of each institution's revenue from state appropriations in 1990 times yearly state appropriations per capita, in \$1000s), an indicator for whether a tuition cap is in place, and the maximum percentage increase allowed under the cap (set to zero when no cap is in place). The model also controls for lags and leads of county and state unemployment rates, time-varying county and institution characteristics, institution fixed effects, year fixed effects, and state-specific time trends. Standard errors are clustered at the institution level.

Figure 5



Notes: This figure presents estimates and 95 percent confidence intervals from a regression of log total spending on lags and leads of the state budget shock instrument (the share of each institution's revenue from state appropriations in 1990 times yearly state appropriations per capita, in \$1000s), an indicator for whether a tuition cap is in place, and the maximum percentage increase allowed under the cap (set to zero when no cap is in place). The model also controls for lags and leads of county and state unemployment rates, time-varying county and institution characteristics, institution fixed effects, year fixed effects, and state-specific time trends. Standard errors are clustered at the institution level.

Figure 6



Notes: This figure presents estimates and 95 percent confidence intervals from a regression of log tuition on lags and leads of the state budget shock instrument (the share of each institution's revenue from state appropriations in 1990 times yearly state appropriations per capita, in \$1000s), an indicator for whether a tuition cap is in place, and the maximum percentage increase allowed under the cap (set to zero when no cap is in place). The model also controls for lags and leads of county and state unemployment rates, time-varying county and institution characteristics, institution fixed effects, year fixed effects, and state-specific time trends. Standard errors are clustered at the institution level.

**Table 1 - Descriptive statistics**

	1990			2013		
	Selective, four	Nonselective, four	Two year	Selective, four	Nonselective, four	Two year
	year	year		year	year	
	(1)	(2)	(3)	(4)	(5)	(6)
Tuition and fees	4,978	3,267	1,027	15,953	8,418	2,381
State appropriations	13,695	8,108	2,549	8,626	5,290	1,890
Local appropriations	45	105	1,013	0	108	1,455
Total grants	1,676	1,111	496	5,594	3,462	1,990
Instructional spending	11,142	6,472	2,606	15,738	7,491	3,007
Academic support	1,593	726	230	4,630	1,700	536
Student services	742	387	257	1,930	1,093	647
Administration	1,279	746	407	3,230	1,703	950
Scholarships	902	559	258	1,753	1,070	836
Total spending	31,946	16,147	5,672	45,584	20,172	7,441
Enrollment	21,278	9,306	3,626	25,865	11,752	5,451
Institution count	36	549	1,383	35	633	1,224

*Notes:* Table 1 presents average enrollment and financial statistics for U.S. public institutions. Data are from the Integrated Postsecondary Education Data System (IPEDS) for 1990 and 2013. Selective colleges are ranked as "Most Competitive" or "Highly Competitive" by the 2009 Barron's Profile of American Colleges. The column "Two Year" also contains a small number of public institutions that offer only less than two year credentials. The first five rows present categories of institutional revenue. The next three rows present categories of institutional spending, and the last two rows are total Fall enrollment and the number of institutions in each category respectively. All financial figures are in 2013 dollars. Total grants includes Federal sources such as the Pell grant, as well as state merit aid and private scholarships. The sample for columns (4)-(6) is restricted to institutions open in 1990.

**Table 2 - Two-stage least squares estimates of the effects of tuition and spending on enrollment**

	First stage		Second stage
	Log spending	Log tuition	Log enrollment
	(1)	(2)	(3)
Budget shock instrument	0.061*** (0.004)	-0.081*** (0.007)	-
Any tuition cap	0.001 (0.002)	-0.030*** (0.003)	-
Maximum increase	-0.045* (0.027)	0.304*** (0.056)	-
Log total spending	-	-	0.300** (0.134)
Log tuition	-	-	-0.017 (0.076)
AP partial <i>F</i> -statistic	26.6	58.2	-
Sample size		27659	

*Notes* : This table reports two-stage least squares estimates of the effects of tuition and spending on total fall enrollment. In the first stage we regress each institution's yearly change in log total spending and log tuition on the change in the budget shock instrument (the share of each institution's revenue from state appropriations in 1990 times yearly state appropriations per capita, in \$1000s), the change in an indicator for whether a tuition cap is in place in the current state and year, and the change in the maximum percentage increase allowed under the cap (set to zero when no cap is in place). The second stage regresses each institution's yearly change in log enrollment on the changes in tuition and spending from the first stage. Columns (1) and (2) report first stage results, and column (3) reports the second stage results. Both stages also control for time-varying county demographic and economic covariates, time-invariant institution characteristics, year effects, and state fixed effects. Standard errors, clustered at the institution level, are in parentheses.

\*significant at 10-percent; \*\*significant at 5-percent; \*\*\*significant at 1-percent



**Table 3 - Two-stage least squares estimates by year and institution type**

	Current year	T+1	T+2	T+3
	(1)	(2)	(3)	(4)
<i>Panel A: all institutions</i>				
Log total spending	0.304** (0.131)	0.796*** (0.181)	0.845*** (0.207)	0.830*** (0.207)
Log tuition	-0.016 (0.072)	0.066 (0.100)	0.031 (0.118)	-0.073 (0.123)
Sample Size	28197	26958	25687	24422
<i>Panel B: two-year institutions</i>				
Log total spending	0.283 (0.253)	1.020*** (0.315)	1.052*** (0.371)	0.951*** (0.345)
Log tuition	-0.083 (0.133)	0.097 (0.169)	0.087 (0.201)	-0.100 (0.204)
Sample Size	16800	16109	15389	14664
<i>Panel C: four-year institutions</i>				
Log total spending	0.239*** (0.081)	0.472*** (0.129)	0.662*** (0.164)	0.574*** (0.182)
Log tuition	0.005 (0.060)	-0.039 (0.085)	-0.046 (0.111)	-0.127 (0.116)
Sample size	11397	10849	10298	9758

*Notes* : This table reports two-stage least squares estimates of the effects of tuition and spending on log total fall enrollment by year and type of public institution. In the first stage we regress each institution's yearly change in log total spending and log tuition on the change in the budget shock instrument (the share of each institution's revenue from state appropriations in 1990 times yearly state appropriations per capita), the change in an indicator for whether a tuition cap is in place in the current state and year, and the change in the maximum percentage increase allowed under the cap (set to zero when no cap is in place). The second stage regresses each institution's yearly change in log enrollment on the changes in tuition and spending from the first stage. Panel A reports results for all institutions, while Panels B and C show results for two-year and four-year institutions respectively. Both stages also control for time-varying county demographic and economic covariates, time-invariant institution characteristics, year effects, and state fixed effects. Standard errors, clustered at the institution level, are in parentheses.

\*significant at 10-percent; \*\*significant at 5-percent; \*\*\*significant at 1-percent

**Table 4 - 2SLS estimates of the impacts of tuition and spending on log certificates and degrees**

	Current year	T+1	T+2	T+3
<i>Panel A: all institutions</i>	(1)	(2)	(3)	(4)
Log total spending	0.201 (0.259)	0.779** (0.317)	0.944*** (0.330)	0.645** (0.304)
Log tuition	0.115 (0.151)	0.226 (0.178)	0.184 (0.187)	-0.006 (0.174)
Sample size	28108	26834	25564	24304
<i>Panel B: two year institutions</i>				
Log total spending	0.451 (0.488)	1.455** (0.621)	1.459** (0.635)	0.637 (0.547)
Log tuition	0.264 (0.275)	0.445 (0.337)	0.244 (0.347)	-0.291 (0.319)
Sample size	16791	16074	15352	14628
<i>Panel C: four year institutions</i>				
Log total spending	0.259 (0.190)	0.325 (0.226)	0.459* (0.234)	0.451* (0.254)
Log tuition	0.123 (0.135)	0.189 (0.151)	0.252 (0.173)	0.306* (0.172)
Sample size	11317	10760	10212	9676

*Notes* : This table reports two-stage least squares estimates of the effects of tuition and spending on the log of total certificates and degrees awarded. In the first stage we regress each institution's yearly change in log total spending and log tuition on the change in the budget shock instrument (the share of each institution's revenue from state appropriations in 1990 times yearly state appropriations per capita), the change in an indicator for whether a tuition cap is in place in the current state and year, and the change in the maximum percentage increase allowed under the cap (set to zero when no cap is in place). The second stage regresses each institution's yearly change in certificates or degrees on the changes in tuition and spending from the first stage. Panel A reports results for all institutions, while Panels B and C show results for two-year and four-year institutions respectively. Both stages also control for time-varying county demographic and economic covariates, time-invariant institution characteristics such as sector, selectivity and highest degree offered, year fixed effects, and state fixed effects. Standard errors, clustered at the institution level, are in parentheses.

\*significant at 10-percent; \*\*significant at 5-percent; \*\*\*significant at 1-percent

**Table 5 - Spillover impacts of public budget shocks on outcomes in private institutions**

<i>Panel A: log enrollment</i>	Current year	T+1	T+2	T+3
	(1)	(2)	(3)	(4)
County average budget shock	-0.025*	-0.011	0.000	-0.027
	(0.014)	(0.018)	(0.022)	(0.024)
Any tuition cap	-0.002	-0.003	-0.013	-0.020**
	(0.006)	(0.008)	(0.008)	(0.009)
Sample size	55962	50018	44742	39855
<i>Panel B: log total awards</i>				
County average budget shock	-0.011	-0.049**	-0.041	-0.034
	(0.018)	(0.021)	(0.026)	(0.027)
Any tuition cap	0.002	-0.001	-0.015*	-0.016*
	(0.007)	(0.009)	(0.009)	(0.009)
Sample size	51066	45689	40865	36398
<i>Panel C: log tuition</i>				
County average budget shock	0.003	0.007	0.009	0.020
	(0.008)	(0.010)	(0.011)	(0.012)
Any tuition cap	0.002	-0.001	0.003	-0.001
	(0.003)	(0.003)	(0.004)	(0.004)
Sample Size	48794	43733	39353	35009

*Notes* : This table reports reduced form estimates of the impacts of the change in the average enrollment-weighted budget shock instrument (the share of each institution's revenue from state appropriations in 1990 times yearly state appropriations per capita) across public institutions in a county and the change in an indicator for whether any tuition cap is in place in the current state and year on the change in outcomes in private (not-for-profit and for-profit) institutions in the same county. Panels A, B, and C report results for log enrollment, log total awards, and log tuition. The regressions also control for time-varying county demographic and economic covariates, time-invariant institution characteristics such as sector, selectivity and highest degree offered, year fixed effects, and state fixed effects. Standard errors, clustered at the institution level, are in parentheses.

\*significant at 10-percent; \*\*significant at 5-percent; \*\*\*significant at 1-percent

**Table 6 - 2SLS estimates of the impacts of tuition and spending on spending and revenue categories**

	Financial aid			Spending categories		
	Scholarship aid (1)	Pell Grant aid (2)	Total aid (3)	Instruction (4)	Academic support (5)	Student services (6)
<i>Panel A: all institutions</i>						
Log total spending	0.287 (0.687)	-0.389* (0.227)	0.053 (0.347)	0.974*** (0.124)	1.716*** (0.378)	1.043*** (0.252)
Log tuition	-0.536 (0.460)	-0.195 (0.131)	-0.143 (0.200)	-0.037 (0.075)	0.364 (0.233)	0.019 (0.137)
Sample size	27634	27795	28135	28204	28126	28164
<i>Panel B: two-year institutions</i>						
Log total spending	-0.117 (0.964)	-0.466 (0.410)	0.159 (0.418)	0.958*** (0.195)	1.180* (0.689)	1.076*** (0.391)
Log tuition	-0.737 (0.593)	-0.153 (0.223)	-0.097 (0.190)	-0.027 (0.106)	0.163 (0.400)	0.111 (0.210)
Sample size	16337	16502	16746	16812	16741	16774
<i>Panel C: four-year institutions</i>						
Log total spending	1.320** (0.624)	-0.160 (0.184)	0.298 (0.333)	0.920*** (0.110)	1.685*** (0.286)	1.166*** (0.271)
Log tuition	-0.101 (0.525)	-0.296** (0.127)	-0.076 (0.265)	-0.102 (0.086)	0.153 (0.215)	0.036 (0.157)
Sample size	11297	11293	11389	11392	11385	11390

*Notes* : This table reports two-stage least squares estimates of the effects of tuition and spending on institutional scholarship aid, Pell grant aid, and categories of spending. In the first stage we regress each institution's yearly change in log total spending and log tuition on the change in the budget shock instrument (the share of each institution's revenue from state appropriations in 1990 times yearly state appropriations per capita), the change in an indicator for whether a tuition cap is in place in the current state and year, and the change in the maximum percentage increase allowed under the cap (set to zero when no cap is in place). The second stage regresses each institution's yearly change in each spending category on the changes in tuition and spending from the first stage. Panel A reports all public institutions, while Panels B and C report results for two-year and four-year institutions. Both stages also control for time-varying county demographic and economic covariates, time-invariant institution characteristics such as sector, selectivity and highest degree offered, year fixed effects, and state fixed effects. Standard errors, clustered at the institution level, are in parentheses.

\*significant at 10-percent; \*\*significant at 5-percent; \*\*\*significant at 1-percent

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