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Abstract

Most public colleges and universities rely heavily on state financial support. As state budgets have tightened in recent decades, appropriations for higher education have declined substantially. Despite concerns expressed by policymakers and scholars that the declines in state support have reduced the return to education investment for public sector students, little evidence exists that can identify the causal effect of these funds on long-run outcomes. We present the first such analysis in the literature using new data that leverages the merger of two rich datasets: consumer credit records from the New York Fed's Consumer Credit Panel (CCP), sourced from Equifax, and administrative college enrollment and attainment data from the National Student Clearinghouse. We overcome identification concerns related to the endogeneity of state appropriation variation using an instrument that interacts the baseline share of total revenue that comes from state appropriations at each public institution with yearly variation in state-level appropriations. Our analysis is conducted separately for two-year and four-year students, and we analyze individuals into their mid-30s. For four-year students, we find that state appropriation increases lead to substantially lower student debt originations. They also react to appropriation increases by shortening their time to degree, but we find little effect on other outcomes. In the two-year sector, state appropriation increases lead to more collegiate and post-collegiate educational attainment, more educational debt consistent with the increased educational attainment, but lower likelihood of delinquency and default. State support also leads to more car and home ownership with lower adverse debt outcomes, and these students experience substantial increases in their credit score and in the affluence of the neighborhood in which they live. Examining mechanisms, we find state appropriations are passed on to students in the form of lower tuition in the four-year sector with no institutional spending response. For community colleges, we find evidence of both price and quality mechanisms, the latter captured in higher educational resources in key spending categories. These results are consistent with the different pattern of effects we document in the four-year and two-year sectors. Our results underscore the importance of state support for higher education in driving student debt outcomes and the long-run returns to postsecondary investments that students experience.

Key words: postsecondary education, state appropriations, student loans, returns to education

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1 Introduction

The US higher education system is dominated by public institutions that rely heavily on state funding. In the 2017-2018 school year, state appropriations accounted for 19% of total expenditures among all public institutions; state appropriations covered 27% of expenditures in public two-year institutions and were 18% of expenditures in public four-year universities. In total, states spent $81.7 billion dollars in support of public higher education in 2017-2018, $72.9 billion of which was direct appropriations.[1] Understanding the importance of state financing of higher education has taken on increased importance in recent years due to significant reductions in such support, including recent budget cuts driven by the Covid-19 pandemic. In 1990-1991, state appropriations covered 39% of total expenditures. This percent dropped to 33% by 2000 and to 26% by 2005. The decline in state funding has occurred in absolute terms as well. Figure 1 presents trends in real appropriations per student: from a peak of $9,495 in 2000, state appropriations were reduced to $8,345 in 2008 and to $5,726 by 2012. These patterns are similar in the two- and four-year sectors: state appropriations per student have fallen by 40% between 2000 and 2012 in each sector. Figure 1 suggests that these declines occur because states scale back appropriations during recessions and then do not subsequently increase them by as much.

Less state support for public higher education institutions is a particular concern for less-selective institutions, as they tend to rely more on state funding. These institutions also serve a disproportionate percentage of students from low-income and disadvantaged backgrounds. Thus, over time, reductions in state appropriations have contributed to the increased stratification of resources in the postsecondary sector, wherein resources are increasingly concentrated in a small set of elite universities that serve the most academically-advanced students (Hoxby 2009; Bound, Lovenheim and Turner 2010).

Does reduced state support for higher education lower the return to postsecondary investments made by students? This question has received scant attention in the research literature to date, likely owing to the difficulty in isolating exogenous variation in state appropriations and the lack of data linking such variation to long-run outcomes. Institutions that rely more on

[1] These tabulations come from the Digest of Education Statistics, Tables 333.10 and 334.10. In addition to appropriations, states provide revenues to higher education institutions in the form of grants and contracts.

state funding tend to have lower-resources and are less selective, and variation over time in state appropriations is likely to be correlated with the financial health of the state, the business cycle, funding priorities of voters in the state, and the need to fund other state programs (Okunade 2004; Kane, Orszag, and Apostolov 2005; Delaney and Doyle 2011). Furthermore, variation in state appropriations can affect resource levels of the university and tuition levels (Deming and Walters 2017; Bound et al. 2019), which can alter the composition of students attending each institution. Estimating the effect of state appropriations on the returns to education thus requires both exogenous variation in revenues coming from the state as well as individual data on educational and labor market outcomes.

A large body of work exists that suggests there is a substantial return to college quality both in terms of labor market outcomes (Brewer, Eide and Ehrenberg; Black and Smith 2004, 2006; Hoekstra 2009; Long 2010; Andrews, Li and Lovenheim 2016; Zimmerman 2016) and educational attainment (Bound, Lovenheim and Turner 2010, 2012; Cohodes and Goodman 2014; Goodman, Hurwitz and Smith 2015; Chakrabarti and Roy 2017).[2] One prevalent measure of college quality is per-student expenditures, which vary systematically with state appropriation (Deming and Walters 2017; Bound et al. 2019). What has received little attention in the literature, however, is whether changes in state higher education funding can directly affect student outcomes in the manner suggested by the returns to college quality research. The answer to this question has much policy importance, since state appropriations is a policy tool that state legislators can directly change through the budgeting process. Estimating the causal effect of state appropriations also is of interest because it provides additional evidence on how postsecondary spending affects the returns to college investment.

This paper provides one of the first analyses of the causal effect of state appropriations on student outcomes using an empirical method that can plausibly overcome the endogeneity of state funding decisions. It is the first to use such variation to examine student debt and default as well as long-run outcomes of students. One of the innovations of this analysis is to use novel data from a new data merger that links consumer credit records with postsecondary enrollment and attainment histories. These data are constructed by a merge of the New York Fed Consumer

[2]Notable exceptions to the finding of positive returns to college quality are Dale and Krueger (2002, 2014) and Stange (2012). However, Dale and Krueger (2002, 2014) find positive effects for students from low-income backgrounds.

Credit Panel (CCP), sourced from Equifax, with the National Student Clearinghouse (NSC). The CCP data consist of a 5% random sample of US individuals with credit files and their household members. The panel follows individuals for whom we observe the history of credit card debt, student debt, and consumer durables debt (such as cars and homes). We also can observe whether (and when) individuals have debt in delinquency, how much of their debt is delinquent, and whether (and when) they have defaulted on any loan. Finally, the data contain a credit score and location of residence, which are useful summary measures of life outcomes.

The CCP has been merged with National Student Clearinghouse data that contain term-by-term enrollment information as well as degree attainment and major among those who complete a degree. For each individual in the linked dataset, we can observe whether and where they attended college, for how long, and debt accumulation both in college and after. We also observe information on post-collegiate enrollment in graduate programs. Together, these data provide a level of detail on students and their financial outcomes previously unavailable to researchers. The dataset we construct spans 1986 through the fourth quarter of 2018. We primarily focus on outcomes among two age groups: 25-30 year olds and 30-35 year olds. This allows us to trace out the timing of any effects on former students through their mid-30s. We also examine 22 and 25 year olds for student debt and completion outcomes to capture the time pattern of these effects.

In order to overcome the endogeneity of state appropriations, we use an instrumental variables approach that follows the insights of the shift-share instrument first proposed by Bartik (1991). We exploit the fact that state-level changes in appropriations will affect institutions differently depending on how reliant they are on state funding. Thus, we specify a base year proportion of total revenues that come from state appropriations, and we multiply this state appropriation share by the annual level of overall state appropriations in each state, scaled by the number of college-age residents. This instrument is valid as long as state decisions about how much money to allocate to higher education are uncorrelated with unobserved changes in the productivity of any specific college or university in the state. Given the large number of postsecondary institutions in most states, this assumption is plausible. We present an extensive set of robustness checks and an analysis of secular trends that provide confidence in the validity

of the approach.

Two recent analyses use similar variation to examine the effect of state appropriations on college enrollment and completion using institution-level data from IPEDS (Deming and Walters 2017; Bound et al. 2019). Both analyses show that state appropriations affect the extensive margin, which creates a challenge for analyzing long-run outcomes. To abstract from the extensive margin, we focus on how state appropriations affect students who are already enrolled in a given institution: we characterize students by their freshman year (cohort). The shift-share appropriations variation is then calculated as the average over 150% of the statutory degree time for the type of college in which a student first enrolls. Critically, we demonstrate that the instrument defined this way does not change the composition of students at a given postsecondary institution. Because private colleges and universities do not receive state appropriations, we focus exclusively on public institutions.[3]

Our results show that both four-year and two-year students are affected by state appropriations changes during their time of enrollment, but in different ways. In the four-year sector, a $1,000 per student state appropriations increase shortens time to degree by increasing the likelihood a student obtains at least a BA by age 25 by 1.5 percentage points (or 2.30% relative to the mean). This effect dissipates by age 30. There also is evidence that state appropriations increases lead four-year students to complete a BA but not a graduate degree. There are strong effects on student loans as well: $1,000 per student of state appropriations reduces the likelihood of originating a student loan by about 2 percentage points at all ages (about 3% relative to the mean) and reduces the total amount of originations by $640 by age 22 (6.25%) and $5,363 by age 35 (11.63%). These students also are about 2 percentage points less likely to default on their student loans. Across the other longer-run outcomes we examine, there is little evidence of an effect of state appropriations changes. We show that this is because four-year institutions largely respond to state appropriations cuts by increasing net tuition. The net tuition changes are sufficient to account for the changes in revenue, and thus expenditures and institutional resource allocations are unaffected.[4] As a result, long-run outcomes outside of student debt

[3]We use private institutions as a falsification test in Section 4.4, showing that state appropriation changes do not affect outcomes of students at similarly-selective private universities in the state.

[4]This finding contrasts somewhat with the results in Bound et al. (2019). They find that state appropriations cause a net price effect in public research universities but not in four-year public universities that are not research focused. Our results and conclusions are robust to excluding state flagship universities (see Section 4.4), which suggests the differences across studies is likely driven by differences in the state appropriations instruments being used.

change little.

In contrast to the four-year sector, students who first enroll in a community college experience better long-run outcomes due to state appropriations increases. For each $1,000 increase in state appropriations per student during the period of enrollment, students are 3.5 percentage points (15.43%) more likely to transfer to a four-year school, are 4.6 percentage points (20.12%) more likely to obtain at least a BA, and are 2 percentage points (40.53%) more likely to earn a graduate degree. The likelihood of originating a student loan declines with state appropriations, but the total amount originated increases among students in their 30s. The latter finding likely is driven by increased postsecondary investment among these students. Despite the increase in student loans, delinquencies and defaults on these loans decline substantially, which is consistent with students experiencing better labor market outcomes. We find suggestive evidence that credit card debt increases by age 35 but that delinquencies decline; a similar pattern emerges for both auto and home loans. By age 35, we show that $1,000 increase in state appropriations per student while in college increases credit scores by 13 points and increases the average income of the zip code of residence by $3,359 (7.37%). Taken together, these results indicate that state appropriations lead to more educational attainment and better long-run outcomes of community college students. Aligned with this result, we find that community colleges respond to increases in state appropriations by lowering net price, increasing instructional and academic support expenditures, and reducing student/faculty ratios. We posit that the two-year sector is less able to fully respond to changes in state support through a tuition mechanism because tuition is so low for community college students.

The main contribution of this paper is to provide the first estimates of the causal effect of state appropriations on short- and long-run financial and credit outcomes of college students. As discussed above, the large literature on the return to college quality is suggestive of such an effect, but most prior work has not been able to isolate the impact of changes in state appropriations, per se, or of institutional spending more broadly. Bound and Turner (2007) provide one of the earliest causal analyses of the effect of per-student institutional resources on academic attainment. They exploit the fact that state appropriations adjust slowly to changes in student demand, which motivates the use of college-age cohort size as an instrument

for per-student spending. The findings indicate that college-age population increases reduce resources and subsequent four-year degree production because of what the authors term "cohort crowding." Our approach differs from theirs in using variation in state funding that is directly under the control of policymakers and that comes from the supply side rather than from the demand side of the market. This question has received less attention in the two-year sector.

The two papers most related to ours are Deming and Walters (2017) and Bound et al. (2019). Deming and Walters (2017) use a similar shift-share instrument to estimate the effect of state appropriations on college enrollment and completion.[5] Bound et al. (2019) use state-year level variation in postsecondary appropriations (the "shift") as an instrument for state appropriations. Both analyses find evidence that state appropriations increase enrollment and completion in the four-year sector, though Bound et al. (2019) argue that the effects in the more selective public sector are muted because institutions are able to fully adjust on the net tuition margin. Our analysis is distinguished from these papers along three important dimensions. First, we abstract from the extensive margin to focus specifically on the effect of state funding changes among students already enrolled in a given institution. Because prior work used institution-level data, they cannot distinguish between completion effects stemming from changes in enrollment levels and composition versus changes in persistence. Our approach sidesteps the enrollment margin, which allows us to isolate the effects of state funding on already-enrolled students. We use individual-level panel data that permits us to examine graduation effects that derive from the persistence mechanism, and we also can estimate effects on student transferring behavior that is quite prevalent (Andrews, Li and Lovenheim 2014). Thus, our analysis complements earlier work by showing the importance of the persistence and transferring pathways in driving completion effects.

Second, we leverage the unique CCP-NSC linked data to estimate the first effects in the literature of how state appropriations shocks when enrolled in college affect student debt and default as well as long-run financial outcomes. Outstanding Federal student loan debt in 2019 was $1.3 trillion, and the default rate on these loans was between 10 and 12 percent (Trends in Student Aid 2019). Because state appropriations can alter net tuition, it is important to

[5] Goodman and Henriques (2018) also show that state appropriation declines are associated with a switch to for-profit institutions. This could lead to worse outcomes if these institutions are less productive, as suggested by prior work (e.g., Armona, Chakrabarti and Lovenheim 2018; Deming et al. 2016; Cellini and Turner 2016; Deming et al. 2012).

understand how these appropriations affect student debt and default outcomes. We provide the first such evidence on this question.

College completion is a critical education outcome, but examining longer-run effects of collegiate resources is important in order to understand the extent to which any completion effects persist into adulthood.[6] Furthermore, changes in state appropriations could affect human capital accumulation in ways that are not picked up by college completion. This is especially the case because college completion is a binary outcome that understates variation in human capital and because state appropriations may impact students who are not on the margin of dropping out of college. Our analysis is the first to be able to provide causal estimates of the effect of state appropriations on these longer-run outcomes, and the structure of the panel data also permits an analysis of whether any effects become smaller or larger with age.

Third, our paper analyzes both two- and four-year students. Much of the literature to date has focused on the four-year sector, but we show state appropriations have large effects on community college students that highlights the importance of separately focusing on this group.

The results from our analysis suggest that state appropriations have positive long-run effects on student outcomes that take somewhat different forms across the two-year and four-year sectors. In the four-year sector, state appropriation cuts largely lead to price effects that are reflected in student loans. Among two-year students, there are both price and institutional resource effects that impact student loan, educational attainment, and consumption/credit outcomes of students into their mid-30s. That the effects we find are driven by variation during college enrollment is particularly relevant, as we are not simply picking up changes in whether or where students enroll. These are important findings because they indicate state funding for postsecondary education has long-run effects on student outcomes, especially in the two-year sector. In both sectors, the cuts in state funding shown in Figure 1 are likely to have contributed to the increase in student debt over the past several decades (Looney and Yannelis 2015). Furthermore, because schools that serve students from lower-income backgrounds are

[6]Scott-Clayton and Zafar (2016) use CCP outcome data linked to administrative education data from West Virginia to estimate the effect of the West Virginia Promise Scholarship on long-run outcomes. They find that scholarship receipt leads to better long-run credit outcomes. Similarly, Bleemer et al. (2017) use CCP data to estimate how public university tuition increases affect education debt held by 24 year olds. They argue that tuition increases can explain upwards of 30% of debt increases held by 24 year olds between 2003 and 2011.

most affected by state appropriations cuts, reductions in state support have helped exacerbate inequality and stratification of outcomes in the postsecondary sector.

2 Data

The data we use in this analysis come primarily from three sources: the New York Fed Consumer Credit Panel (CCP), the National Student Clearinghouse (NSC), and the Integrated Post-Secondary Education Data System (IPEDS).

2.1 Measuring College Enrollment and Short-term Student Outcomes

A novel aspect of our analysis is to leverage a merger between two rich datasets: the New York Fed Consumer Credit Panel (CCP) and the National Student Clearinghouse (NSC). The CCP includes individual-level consumer credit records sourced from Equifax credit bureau through 2018, while NSC includes individual-level postsecondary education records through the 2014-2015 academic year. This unique dataset allows us to observe financial outcomes as well educational enrollment and attainment over time for a random sample of individuals. Since NSC coverage improved over the years, we consider cohorts starting from the 1975 birth year. To maximize the match between NSC and CCP, we exploit a stratified random sampling method based on the coverage of the NSC data, where we over-sample cohorts starting from the 1980 birth year.

When examining outcomes by age 30 and outcomes between ages 25 and 30 in the CCP data, we restrict our sample to those born prior to or in 1988, as the 1988 cohort is the last cohort that we observe through age 30. This enables us to use a balanced set of birth cohorts (1975-1988) in the sense that we observe each of these cohorts up to age 30. Similarly, when examining CCP outcomes by age 35, we restrict our sample to those born between 1975 and 1983. For student loans, we also examine outcomes by age 22 (1975-1996 birth cohorts) and by age 25 (1975-1993 birth cohorts). When analyzing educational outcomes, we focus on the age 25 and 30 samples as well as an unbalanced panel of those up to the 1996 birth cohort ("ever" sample).[7]

[7] Because the NSC data only go through 2014, the 30-year-old sample is comprised of the 1975-1984 birth cohorts, the 25-year-old sample is comprised of the 1975-1989 birth cohorts, and the "ever" sample is comprised of the 1975-1996 birth cohorts.

Online Appendix Table A-1 shows the cohorts used in our main analysis samples. Freshman age and cohort are defined as the age and year, respectively, in which each individual was first enrolled in college. Since we over-sample the 1980 and later birth cohorts to maximize data quality, the mean is higher than the median birth year included in each sample. Because we are interested in the effect of state appropriation shocks, our current analysis focuses only on college-attendees.

We only consider students whose first college of enrollment is a public college, as private colleges do not receive state appropriations. We link each student to a public institution based on the first college in which we see that student enrolled. The primary motivation for this approach is that transferring to a different college later in the student's educational career has the potential to be endogenous to state appropriation shocks that the student faced earlier. For example, a negative state appropriation shock occurring while a student is in school may drive up tuition, making that institution unaffordable and forcing him or her to transfer to a different school (or drop out altogether). These transfer and completion behaviors are outcomes of interest, which we examine directly, rather than exogenously determined characteristics of an individual. Hence, we focus here on the first school an individual attended.

From the NSC data, we obtain variables relating to educational outcomes that could be affected by state appropriations shocks, including whether and to what type of institution an individual transfers, whether and when an individual obtains a degree, and the major of the degree obtained. With these data we construct measures of whether a student transfers, to what type of institution transfer occurs, whether a student obtains a BA and/or a graduate degree, and the major category among completers.

2.2 Measuring State Appropriations

Since pre-entry state appropriation may well affect the college choice of students, we consider state appropriations that the student faced after he or she already enrolled in her first college, starting from her first year. We start by identifying for each individual the year she is first enrolled in college; Online Appendix Figure A-1 shows the distribution of students by their birth year. The data over-sample those who are born in 1980 and after, which is why the

cohort sizes increase in that year. The distribution of students by their freshman cohort and first institution level is shown in Online Appendix Figure A-2. We use this first-year cohort to identify the state appropriation shocks the student will face in her first college. Given the birth cohorts we use, most students enter college in the late 1990s and early 2000s, but there is a long right tail driven by non-traditional enrollment. We account for these sample features by including an indicator for being born in 1980 or afterwards and including birth year by entry cohort year fixed effects in our empirical models. Online Appendix Figure A-3 shows the distribution of institution-freshman cohort cell sizes for each analysis sample.

Since length of time actually spent in first college is a matter of choice and likely is correlated with unobserved attributes, we assume the student is exposed to state appropriations shock in her first college based on the level of the college (4-year or 2-year). Specifically, we assume students spend 150% of the statutory degree time enrolled in college. For a four-year (two-year) student, we determine the state appropriations faced by that institution for six (three) years starting from her freshman year, and we assume this is the total shock faced by the student in her first college. Institution level state appropriations per student averaged over the corresponding years constitutes our endogenous treatment variable. Figure 2 presents the distribution of institution level state appropriations per student at baseline for 4-year and 2-year institutions respectively. As may be expected, state appropriations per student is higher in 4-year institutions than in 2-year institutions, but there is a considerable amount of variation in state appropriations per student in each sector.

To construct our shift-share instruments, we use data on institution-year level state appropriations that we aggregate to the state-year level by college sector (four-year and two-year). We also use total enrollment and total revenue at the institution-year level. These data are obtained from IPEDS from the 1986-87 to the 2014-15 academic year. We focus on 2-year and 4-year public institutions, as these institutions rely most heavily on funding from state appropriations as compared with private non-profit and for-profit institutions.[8] Less than two year public institutions are excluded because of some IPEDS data inconsistencies during the period of our analysis; this group of institutions constitutes less than 1% of all public sector

[8]We also performed our analysis with IPEDS data from the Delta Cost Project and found similar results. Because the Delta Cost Project groups some institutions under a single parent institution, eliminating some variation in state appropriations faced by individual institutions, our primary analysis uses IPEDS data.

enrollment.

2.3 Long-Term Outcome Measures

A major strength of our dataset is that we have extensive data on longer-term financial outcomes from the CCP. For each individual, we identify a variety of financial outcomes that come directly from the CCP data, including student loan originations on the extensive and intensive margins, credit score, credit card balance, auto loan status and balance, and mortgage loan status and balance. For each loan type, we observe delinquencies and default. When examining delinquencies, we focus on the percent of the loan that is delinquent. Mortgage and auto loans, respectively, constitute our measures of homeownership and car ownership. Since all-cash home or car purchases are rare, especially among the relatively younger adults that constitute our analysis sample, these measures are reliable indicators of vehicle and home ownership (Chakrabarti, Gorton and van der Klaauw 2017; Bleemer et al. 2017; Chakrabarti and Pattison 2016). For loan balances and credit score, we calculate the average balance or score between ages 25 and 30 as well as between ages 30 and 35. We construct indicator variables for homeownership, car ownership, and student loan holdings that take a value of one if an individual ever bought a home or car or held student loans by a given age.

Student loan originations reflect the total originations by a given age, not the loan balance. Thus, we measure how much a student takes out in loans by a given age, rather than how quickly a student pays those loans back. The ability to pay back loans is reflected in the student loan delinquency and default outcomes.

2.4 Zipcode Income and College Selectivity

We match zipcode level income data for the period 2001-2014 from the US Treasury to our CCP-NSC matched data using individual level zipcode information from the CCP. For all students, we assume their initial zipcode of observation is their home location. We use US Census data to characterize the areas from which our students originate. We also examine the zipcode income in which people currently reside as a measure of neighborhood quality that could be affected by state appropriations. Finally, we calculate a Neighborhood Quality Index by first standardizing

the median zip code home value, adjusted gross income (AGI), and percent with a BA in the zipcode and then taking the average of these three standardized variables.

To measure college quality, we match Barron's selectivity rankings for 2001 four-year colleges to our CCP-NSC panel. Based on institutional characteristics such as acceptance rate, median entrance exam (SAT, ACT), GPA for the freshman class, and percentage of freshmen who ranked at the top of their high school graduating classes, Barrons ranks colleges into six categories (1-highest, 6-lowest). We group the lowest two categories into a single category (group 5). All community colleges are grouped in a separate category (group 6).

2.5 Descriptive Statistics

Table 1 shows descriptive statistics for our main outcomes and samples, separately by the sector of first enrollment. As expected, four-year students take out more loans and originate more student debt. Across sectors, student debt origination grows over time as a larger percentage of the cohort enrolls in college. Turning to delinquencies, we find that 6-8 percent of student loan balances are delinquent, and default is rather high at 12-17 percent. Credit scores, car and home ownership all increase with age and are higher in the four-year sector, reflecting in part the positive selection into four-year institutions. Home and auto loan delinquency is much lower than credit card delinquency, which is sensible because credit cards are not securitized. A large portion of the sample experiences some delinquency, but bankruptcy is relatively rare, especially among students in the four-year sector.

Institution-level descriptive statistics are shown in Table 2. Mean appropriations for students in the four-year sector is $5,839, with a standard deviation of almost $5,000. In the two-year sector, the mean per-student state appropriation is $2,322, with a standard deviation of $1,303. We scale results by $1,000/student, which Table 2 shows is a fifth of a standard deviation of per-student state appropriations in the four-year sector and is nearly a full standard deviation in the two-year sector. Descriptive statistics of NSC education outcome variables are shown in Online Appendix Table A-2.

3 Methodology

3.1 Construction of the Instrument

The goal of our analysis is to identify the causal effect of state appropriations changes while in college on long-run outcomes. The main identification concerns are that state appropriations are negatively correlated with college quality in the cross section, and within-institution variation in state funding over time is correlated with the business cycle, state funding priorities, and other funding obligations. Even a fixed effects panel regression at the institution-year level thus is likely to be biased, however it is unclear in which direction the bias would go. To overcome this problem, we use an instrument based on the shift-share approach pioneered by Bartik (1991) and used previously by Deming and Walters (2017). This instrument leverages the fact that state-wide changes in appropriations for higher education will have different effects on postsecondary institutions based on their underlying reliance on state funds.

Our preferred version of the instrument uses the 3-year lagged state appropriations share as the baseline share measure. This allows the base share to update each year. The rolling appropriations shock in the first year of college is calculated as:

$$\widetilde{SA}_{jsc} = \frac{SA_{js,c-3}}{REV_{js,c-3}} * SA_{sc}, \tag{1}$$

where c indexes one's freshman cohort year and $SA_{js,c-3}$ is per-student state appropriations received by institution j in state s in year $c-3$. The variable REV is total per-student revenue received by the institution in year $c-3$, and SA_{sc} is total state appropriations in the state in year c. We assess the robustness of our findings below to the use of a cumulative shock measure that fixes the baseline share at the 1986 level throughout. We favor the rolling shock measure because it provides more variation. Online Appendix Figure A-4 shows the relationship between the share in 1986 and in 2013. While there is some within-institution variation, the shares are highly correlated with one another. Unsurprisingly, the results are similar across the two different measures of state appropriation shocks.

Table 2 presents the means of base share, state appropriations, and enrollment across institutions in our analysis sample. Base shares are similar on average across the two-year and

four-year sectors, at 35%, and the selective four-years have lower base shares.[9] Figure 3 shows the distribution of rolling baseline shares across institutions. In both sectors, there is a wide distribution.[10] Our identification strategy leverages this variation directly.

Figure 4 shows the distribution of the second part of the instrument: overall state appropriations per college-age resident in the state in the baseline year. We take the number of people aged 18-44 as the measure of college-age residents. This is an expansive definition of the college-age population, due to the increasing prevalence of older, non-traditional students. Figure 4 presents the distribution of overall appropriations per college-age resident that have been demeaned with respect to the statewide average over our sample period.[11] The figure demonstrates that there are large changes in appropriations per capita within states over time. Interestingly, the changes in the four-year sector tend to be positive,[12] while those in the two-year sector tend to be negative.

One of the core identification concerns in this analysis is driven by the fact that baseline state appropriation share is negatively correlated with institutional quality. This pattern is illustrated in Figure 5, which presents the distribution of base shares by college selectivity using the Barron's rankings categories to measure selectivity. Category 1 is the most selective, and all community colleges are in group 6. There is a clear increase in base share as selectivity declines, although the distributions overlap across categories. In addition to college quality, baseline share is strongly negatively correlated with the average income of the student's initial zipcode, as shown in Figure 6. Institutions that rely more on state appropriations typically serve students from more disadvantaged backgrounds. The shift-share instrument we employ is designed to account for any bias stemming from the correlation of state appropriation share with student background characteristics. We present extensive evidence below that our instrument is uncorrelated with the composition of students at a given institution.

[9] Our shares match those from Deming and Walters (2017). They find that in 1990, state and local appropriations account for 44 percent of total spending for selective four-year institutions, 51 percent for less-selective four-year institutions, and 62 percent for two-year institutions. Our estimates for the revenue (rather than expenditure) share of state and local appropriations in 1986 are 41% for selective four-year schools, 49% for less-selective four-year institutions, and 65% for 2 year schools.

[10] Fifty-six percent of the baseline share variation in the four-year sector is across states, and 30% of the variation in the two-year sector is across states. While state policies and practices clearly drive much of the variation in baseline shares, there is considerable across-state variation as well.

[11] Online Appendix Figure A-5 shows the raw distributions that have not been demeaned. The distributions of the instrument by sector are shown in Online Appendix Figure A-6. Each panel reveals a large amount of variation, though as may be expected there is larger variation among four year universities.

[12] The positive changes in the four-year sector are not inconsistent with declining state support because overall expenditures are growing rapidly. Thus, even though states are putting more money towards higher education in some cases, on average the share of spending being paid for by state revenues is shrinking.

As discussed above, we want to abstract from the extensive margin in order to focus on the effect of state appropriation shocks among students already enrolled in college. In this way, we are identifying the effect of funding changes among already-enrolled students that are not coming from altering where students initially enroll. Institution j is defined as the public college or university in which a student initially enrolls. Similarly, each student's cohort (c) is defined as the academic year in which he is a freshman in college. We then average the shocks over six years of potential enrollment for four-year students and three years of potential enrollment for two-year students starting from their freshman year and scale by the number of college-age residents in the state.[13]

$$\widetilde{SA}^{4yr}_{jsc} = \frac{\frac{1}{6} * \sum_{\tau=c}^{c+5} \frac{SA_{js,\tau-3}}{REV_{js,\tau-3}} * SA_{js\tau}}{College_Age_Pop_{sc}} \tag{2}$$

$$\widetilde{SA}^{2yr}_{jsc} = \frac{\frac{1}{3} * \sum_{\tau=c}^{c+2} \frac{SA_{js,\tau-3}}{REV_{js,\tau-3}} * SA_{js\tau}}{College_Age_Pop_{sc}} \tag{3}$$

Equations (2) and (3) are the average state appropriations a student can expect based on prior institutional reliance on state appropriations and overall state funding for higher education in the period of expected enrollment. Critically, all of the state appropriations variation occurs *after* students have made initial college enrollment decisions.

3.2 Empirical Model

We use the instruments shown in equations (2) and (3) to overcome the selection problems associated with state appropriation variation. The reduced form model on which we focus is as follows:

$$Y_{ijsac} = \beta_0 + \beta_1 \widetilde{SA}_{ijc} + \beta_2 \frac{SA_{jc}}{REV_{jc}} + \beta_3 College_Age_Pop_{sc} + \beta_4 1(yob \geq 1980) + \gamma_{ac} + \phi_j + \epsilon_{ijsac}, \tag{4}$$

where Y is outcome of individual i who initially enrolled in institution j in state s and was a freshman of age a in freshman cohort c. Thus, c indexes the academic year in which a student

[13] All variation in state appropriations that we use comes after students make their initial enrollment decision. For example, for a student first enrolling in college in fall 2000, the enrollment decision is made based on factors that are known in fall 1999 or spring 2000, while appropriations in her freshman year are determined during the 2000-2001 academic year and thus cannot affect her enrollment decision. We show direct evidence that our instrument does not affect the composition of students in Section 3.2 (Table 3).

is first enrolled, and a indexes the age of the student in her freshman year. When Y is per-student state appropriations, equation (4) represents the first stage effect of the instrument on actual state appropriations. We include fixed effects for age at freshman year interacted with freshman cohort year (γ) in the model. Because our outcomes are stratified by age as well, the age-cohort fixed effects also account for calendar year effects.[14] We also include controls for college population in the state during one's first year and a dummy variable that takes a value of one for 1980 and later birth cohorts.

Equation (4) controls for institutional fixed effects and base share used to calculate the instrument as well. The baseline share variable is important to include because we employ a rolling baseline share that changes over time. Despite the stability of these shares (see Online Appendix Figure A-4), any within-institution changes could be correlated with unobserved attributes of students that relate to long-run outcomes. Directly controlling for the baseline share accounts for such changes. The institution fixed effects account for cross-sectional heterogeneity in institutional characteristics that are important in this design because of the negative correlation between state appropriations share and college selectivity.

We estimate equation (4) separately for those initially enrolling in two-year and four-year institutions. For two-year students, transferring to a four-year college is an important outcome that can be influenced by state appropriation changes. We examine this outcome below and classify students throughout based on the sector and institution in which students first enroll.

Because the instrument varies by first-year cohort and state, we cluster standard errors at the cohort-state level throughout the analysis. Adao, Kolesar, and Morales (2019) show that standard errors clustered at the geography of the "share" dimension of traditional shift-share instruments leads to standard errors that are biased downward. The intuition for this finding is that with a common "shift," there will be a mechanical correlation of the error terms for areas with similar shares. Unlike a traditional shift-share instrument that uses a common national shift, the shift variable in our setting varies by state. Institutions with similar state appropriation shares in different states will experience different state appropriation shifts, which mitigates concerns about errors being correlated across states among institutions that rely

[14] The reason for this equivalence is that calendar year in which outcomes are measured can be calculated knowing an individual's age at which outcomes are measured, his age in the first year of college, and the year in which the student entered college.

similarly on state funding. By clustering our standard errors at the state-cohort level, we are accounting for any within-state correlation of error terms across schools with similar shares because they experience similar state appropriation shifts. Hence, our method accounts for the error correlation structure that drives the bias in Adao, Kolesar, and Morales (2019) because we are able to cluster at the level of the shift variable.

The coefficient of interest in equation (4) is β_1, which shows the effect of state appropriations during one's potential time enrolled in college on outcomes at a given age. The identifying variation comes from two sources: 1) *cross-cohort* changes in state appropriations within each institution, essentially comparing outcomes of adjacent cohorts within a public university who experienced different levels of state appropriations during their expected enrollment years, and 2) variation across colleges in the extent of cross-cohort changes in appropriations as a function of historical reliance on state support.

Two recent papers have elucidated that the shift-share identification strategy relies either on the exogeneity of the baseline shares (Goldsmith-Pinkham, Sorkin, and Swift 2020) or on the exogeneity of the shifter (Borusyak, Hull, and Jaravel 2018). In our context, these requirements translate into an assumption that state-level changes in appropriations are uncorrelated with cross-cohort changes in potential outcomes of students at colleges that rely differentially on state funding. For example, if state appropriations are declining in states in which the students at more state-reliant institutions are entering college with lower achievement levels, this would bias our estimates. Put differently, bias stems from secular trends or shocks in unobserved student ability that are correlated both with changes in overall state appropriations and with the base share.

There are three classes of threats to identification that we examine in detail. First, the state appropriations instrument can be correlated with local economic activity. If recessions, for example, disproportionately hit areas with higher-share institutions in them and if recessions have independent effects on outcomes, it will bias our estimates. In Section 4.4.1, we examine the robustness of our estimates to this source of bias by including controls for share interacted with the county unemployment rate at the expected time of college exit, share interacted with the county home price index at the expected time of exit, share interacted with the county

unemployment rate in the year of college entry, and share interacted with fixed effects for state political party control. Our results change little with the addition of these controls.

Second, there could be secular trends across cohorts that are correlated with the instrument because of serial correlation in the instrument. We argue our estimates should not be highly sensitive to such secular variation. Despite the overall downward shift in state appropriations over time, most institutions experience both positive and negative shocks to state funding over our sample period (see Figure 4). Unidirectional secular trends should not present a bias in our estimates. To develop a better understanding of how secular trends in outcomes correlate with baseline shares, Online Appendix Figures A-7 through A-11 show pre-2005 trends in several of our main outcome variables as a function of the quartile of the baseline share in 2005. Across outcomes, ages, and sectors, there is no evidence of differential trends pre-2005 based on 2005 state appropriation shares. Online Appendix Figures A-12 through A-16 show identical patterns with respect to baseline shares in 2014. For both the early and later part of our sample, there is no evidence that outcomes are trending differently as a function of baseline share. In results available upon request, we conduct a similar exercise where we examine trends by quartiles of the instrument in 2005 and in 2014: we find that none of the outcome variables exhibit differential trends by quartile of the shift-share instrument in either year as well.

We additionally perform a series of robustness checks to assess this source of bias (see Section 4.4.2). We show our results and conclusions are robust to controlling for share interacted with college entry year fixed effects, share interacted with expected year of college exit fixed effects, state-by-freshman cohort fixed effects, state-by-freshman cohort-by selective college fixed effects, state-birth cohort fixed effects, and the shift variable (state appropriations per college-age resident). These results support our contention that secular trends do not bias our results, even in the presence of serial correlation of the instrument.

Third, there can be changes in the composition of students that are correlated with the instrument. To help guard against this concern, we examine state appropriation shocks among students already enrolled in a specific institution. While students can transfer, they are not making initial enrollment decisions based on the state appropriations shocks we assign to them since they already have made the enrollment decision. This distinguishes the parameter we

identify from those in Bound and Turner (2007), Deming and Walters (2017), and Bound et al. (2019). Those analyses allow for state appropriation shocks to influence enrollment decisions, which they show to be an empirically relevant margin of student response. The way in which we specify the instrument does not allow for such extensive margin adjustments; students can transfer in response to state appropriation changes, but this is a mechanism underlying the results rather than a source of bias.

Because the instrument may be serially correlated over time, it is not guaranteed that a focus on already-enrolled students will allow us to fully abstract from the extensive margin. In Table 3, we provide evidence that the composition of enrolled students is unaffected by the instrument. We use each student's initial (pre-collegiate) zipcode to construct measures of the types of neighborhoods from which students originate. The table presents both estimates from equation (4) and implied effect sizes in units of $\frac{\$1{,}000}{Student}$ that are calculated using the first-stage estimates in Table 4. Across outcomes, there is no evidence that changes in the instrument induce changes in the characteristics of students. The estimates are universally small and are not statistically significant at even the 10% level. Overall, Table 3 strongly supports our identification strategy by showing that the instrument we use does not alter selection into college: any serial correlation in the instrument is not inducing a change in the composition of students at a given institution. In Section 4.4.3, we show that controlling for these observable characteristics does not affect our results, which further supports our empirical approach.

An alternative explanation for the results in Table 3 is that these variables do not capture relevant margins of student selection. In Online Appendix Table A-3, we present estimates that use a one-year instrument that corresponds to the year immediately before entry; this is the value of the instrument the student would have faced at the time of her application for admission. The effect sizes are larger and more of the estimates are statistically significantly different from zero than is the case in Table 3. Taken together, the results in Tables 3 and A-3 indicate that variation in the instrument from the year *before* freshmen entry (i.e, prior to the enrollment decision) does correlate with student composition through changes in the extensive margin, while variation in the instrument based on the years *after* entry does not correlate with student composition of already enrolled students. The results in these two tables provide

evidence that focusing on state appropriation variation from after the initial enrollment decision is effective at addressing concerns about bias from changes to student composition driven by extensive margin selection.

We present two falsification tests in Section 4.4.4 that provides evidence on the relevance of all of these sources of bias. We show that state appropriations shocks do not affect similarly-selective private university outcomes and that state transportation spending does not produce the same effects as higher education spending. These tests would fail if we were picking up confounding variation from any of the sources discussed above; that these estimates differ substantially from those in our main model supports our methodological approach.

4 Results

4.1 First Stage Estimates

First stage estimates that show how the predicted state appropriations instrument relates to actual state appropriations for our two primary age groups of focus are reported in Table 4. Odd columns of the table include age-cohort fixed effects, baseline state appropriations share, and college-age population, while even columns add institution fixed effects. The first two columns of Table 4 show estimates for 25-30 year olds and columns (3) and (4) show estimates for 30-35 year olds. Panel A shows results for students whose first college is a public four-year institution, and Panel B shows the associated 2-year estimates. In Panel A, a $1 increase in state appropriations per college-age resident leads to between a $25.22 and $26.62 increase in appropriations per student at the institutional level when the 3-year lagged state appropriations share of revenue is 1 percentage point higher.[15] Holding state-level appropriations fixed, an increase of 10 percentage points in the share of revenues constituted by state appropriations thus leads to an increase of $252-$266. This effect is significant at the 1% level using standard errors that are clustered at the freshman cohort-state level. Comparisons with models that exclude institution fixed effects shows that the fixed effect make the estimates slightly larger

[15] These estimates are much larger than one because of the way we have scaled the instrument and the endogenous independent variable. The former is in terms of total state appropriations per resident aged 18-44, while the later is per student at the institution level. Since there are many more residents aged 18-44 than there are college students at any one school, the scale of the instrument is smaller than the scale of per-student state appropriations at the institution level. The Table 2 summary statistics show the differences in scale between the instrument and per-student state appropriations at the institution level.

and more precise. However, they do not have a large impact on the results.

Estimates in the two-year sector are somewhat smaller than those in the four-year sector, but they still are large, positive and significant at the 1% level. An increase in predicted state appropriations of $1 leads to a $19.83-$20.84 increase in per-student state appropriations at the institution level. Together, the results in Table 4 indicate that the state appropriations instrument is strongly and robustly related to actual institution-level state appropriations.

4.2 Reduced Form Estimates

We focus on reduced form effects of the shift-share state appropriations instrument on a range of adult outcomes. All estimates include institution fixed effects.[16] First, we show effects on educational attainment for 25 and 30 year olds as well as for sample members at the highest age of observation ("ever"). We then examine student loan outcomes at ages 22, 25, 30, and 35, after which we analyze long-run credit outcomes for 25-30 year olds and 30-35 year olds. In each table, we show estimates from equation (4), where the coefficient on the shift-share instrument is scaled in terms of $100 of state appropriations per college-age resident. We present effect sizes in square brackets, which is the reduced form effect divided by the first stage. Effect sizes are scaled to be in units of $1,000 per student. In curly brackets, we show effect sizes in percent terms relative to the means in Table 1 for CCP outcomes and in Online Appendix Table A-2 for NSC educational outcomes.

4.2.1 Educational Attainment

We first examine the effect of state appropriations changes while enrolled in college on educational attainment.[17] Panel A of Table 5 presents estimates of how state appropriations alter the likelihood of transferring to a four-year institution. This outcome only is relevant for those who start at a two-year college. The estimates in columns (3) and (4) show that $100 of appropriations per college-age resident increases the likelihood of transfer by 6-7 percentage points when the baseline share is 1 percentage point higher. These estimates are significant at the 1% level in column (4) and at the 10% level in column (3). The effect sizes show that transfer to a

[16] Reduced form results without institution fixed effects are available upon request. They are generally similar to those with institution fixed effects.

[17] First stage estimates for the NSC samples used to generate the estimates in Tables 5-7 are shown in Appendix Table A-5.

four-year institution increase by 3.1-3.5 percentage points, or 15-17% relative to the mean, for each $1,000 per student of state appropriations at the institution level.

In Panel B, we show effects of transferring to a more-selective institution. Two-year students may transfer to similarly-non-selective four-year schools, but the estimates are very similar to those in Panel A. Hence, students are not only transferring to four-year schools due to state appropriation increases but are transferring to more-selective institutions. Columns (1) and (2) show no evidence that four-year students are likely to "transfer up," however. In results not reported, we have examined whether state appropriation shocks alter the likelihood of lateral and downward transfers as well. These estimates are small and are not statistically significantly different from zero at conventional levels. State appropriations lead to increases in two-year to four-year transitions, but they do not induce other types of transfers. This is an interesting result in part because of high transfer rates among four-year public university students (Andrews, Li, and Lovenheim 2014).

Table 6 shows the effects of state appropriations on collegiate attainment. For four-year students in Panel A, $1,000 per student of state appropriations while enrolled in college increases the number of years of postsecondary education by a statistically significant 0.10 years by age 25. The effect is cut almost in half by age 30, and the age-30 and "ever" estimates are not statistically significantly different from zero. These results suggest that state appropriations alter the timing of educational investments. Panel B reinforces this conclusion, showing that $1,000 of state appropriations increases the likelihood of obtaining a BA or higher by 1.5 percentage points (2.3%) at age 25 but not at older ages. Panels C and D examine whether students obtained only a BA and whether they earned a graduate degree, respectively. For four-year students, state appropriations increases lead to a significant increase in the likelihood of obtaining a BA but not a graduate degree. Hence, state appropriations increase the speed at which students earn a Bachelor's degree, but there is some evidence they are less likely to continue on to graduate school.

The results in Panel A do not fully align with other estimates of the effect of state funding on BA completion (Bound et al. 2019; Deming and Walters 2017). As discussed above, these papers find a positive effect of state appropriation increases on BA attainment. However,

they are unable to disentangle the extensive margin effects from the effect on already-enrolled students, nor are they able to distinguish between time to degree changes and eventual degree completion effects. Our results suggest that much of the six-year completion effects in existing research are driven by changes to the extensive margin at four-year universities rather than the effect of these additional resources on students who are already enrolled. Furthermore, the BA attainment effect we document for four-year students reflects a shifting of degree receipt to younger ages rather than an overall increase in the likelihood of BA receipt.

Among two-year students, state appropriation increases lead to more educational attainment across all levels we examine. Unlike in the four-year sector, there is no time to degree effect, but especially in the mid-30s there is a clear increase in educational attainment. A $1,000 increase in appropriations per student leads to a 0.19 year (5.17%) increase in the number of years of collegiate attainment by the mid-late 30s, a 2.5 percentage point (18.09%) increase in the likelihood of obtaining a BA, and a 2.0 percentage point (40.53%) increase in the likelihood of earning a graduate degree. These are large effects that demonstrate the importance of state support for community colleges in supporting these students' collegiate outcomes.

The return to higher education investment is highly heterogeneous by course of study (Altonji, Blom, and Meghir 2012; Altonji, Arcidiacono, and Maurel 2016; Kirkeboen, Leuven, and Mogstad 2016; Andrews, Imberman, and Lovenheim 2017). Table 7 shows results for field of study. In the four-year sector, state appropriations have no effect on the distribution of majors with which students graduate. In the two-year sector, by contrast, there are increases across-the-board in majors that are between 2-3 percentage points per $1,000 of state appropriations per student in column (6).[18] While the liberal arts estimate in column (6) is not statistically significant, the magnitude aligns with those in Table 6 and with the other major estimates. Students are more likely to graduate when state support increases, and they do not shift their majors to less technical ones.

4.2.2 Student Loans

Table 8 presents the first estimates in the literature of how state appropriations affect student loan takeup and originations. We examine four ages that trace out the impacts over the

[18] College non-completers are included in these regressions, with their major coded as zero for each outcome. Hence, the overall increase in college majors is driven by the increase in completion, which occurs evenly across the major distribution.

relevant part of the lifecourse: 22, 25, 30, and 35.[19] In the four-year sector, we find consistent evidence that state appropriation increases lead to reductions in the likelihood of having taken out a student loan. At each age in Panel A, $1,000 of appropriations while enrolled reduce the likelihood of loan takeup by about 2 percentage points (3.0-3.5%). The estimates are significant at the 5% level for all but the age-35 sample. Since $1,000 is about 1/5 of a standard deviation, a one standard deviation increase in state appropriations per student would have a very large effect on the likelihood of originating a student loan.

The effect on loan takeup understates the impact of state funding on student debt. Panel B shows results using the total amount originated by each age as the dependent variable, including zeros. By age 22, $1,000 of state appropriations reduces the amount originated by a statistically significant $639.54 (or 6.25%). This effect increases in absolute and percentage terms, such that by age-35 there is an effect of $5362.87, or 11.63%. State appropriation increases generate significant reductions in whether and how much student debt is originated by four-year students. In Section 4.3., we show that this effect is driven by institutional tuition and financial aid responses.

Panels C and D of Table 8 present effects on the percent of student loans that are delinquent and on student loan defaults, respectively. Because students do not tend to enter repayment until their mid-20s, we only show these outcomes for the 25-30 and 30-35 year old samples. The estimates of how state funding influences student loan delinquency show little relationship; the estimates are close to zero and are not statistically significant at even the 10% level. In Panel D, there is evidence that state appropriations are productive in reducing the likelihood of loan default, especially among the 30-35 year old sample. While the estimate only is significant at the 10% level for 30-35 year olds, the 2 percentage point effect is large relative to the sample mean (14.16%). It is likely the case that the improved default outcome is driven in part by the fact that students are taking on less debt.

The pattern of results is more complicated in the two-year sector, as demonstrated in columns (5)-(8) of the table. Panel A shows a consistent reduction in the likelihood of taking out a student loan, which is largest among the 35-year-old sample. For this sample, $1,000 of appropriations per student reduces the likelihood of originating a loan by 6.1 percentage points

[19] First stage estimates for the 22- and 25-year old samples are shown in Online Appendix Table A-4.

(11.21%). At younger ages the effect is closer to 2 percentage points, and it is statistically significant for all but the age-30 sample. The amount of student loans originated initially declines substantially but then becomes positive after age 30 (Panel B). While not statistically significant at conventional levels, these results do indicate an increase in loan originations driven by state appropriation increases. Recall that two-year students obtain more postsecondary education when there are positive appropriation shocks, and these effects are largest into the mid-30s. The added debt originated is plausibly due to students increasing their period of enrollment. Critically, in Panel C there is a reduction in the percent of student loans that are delinquent for both age groups. For each $1,000 of state appropriations per student, there is a reduction of between 1.4 and 3.5 percent of existing loan balances that become delinquent. These are large effects relative to the baseline delinquency rate. In Panel D, there is between a 2 and 4.5 percentage point decline in default as well, which represent 14.77% and 26.70% reductions relative to the mean, respectively. Although student loan originations are increasing, adverse debt outcomes decline substantially. This pattern of results strongly suggests that the increased educational attainment induced by state appropriations has positive labor market effects for students, as they are more successful at paying off a larger volume of loans.

The results in Table 8 show that the incidence of state appropriations falls in large part on students, which is reflected in changes in student debt outcomes. As we show below, the core mechanism driving these effects is changes to net tuition. That student loans are highly sensitive to state appropriation changes suggests that declining state appropriations have contributed to the large rise in student debt in the US (Looney and Yannelis 2015).

4.2.3 Long-Run Outcomes

We now turn to an analysis of how state appropriations affect longer-run outcomes of students that we can observe on credit reports: debt from credit cards, auto loans, and home loans, credit scores, and location of residence. For each outcome, we examine loan takeup as well as delinquency in order to better understand whether the changes in debt are reflective of higher consumption from permanent income increases or whether they are driven by consumers over-leveraging themselves.

Table 9 presents results for credit card balance (Panel A) and percent of the credit card

balance that is delinquent (Panel B). The estimates in Panel A are all positive, but only the 25-30 estimate for four-year students is statistically significant. Taking the point estimates at face value, $1,000 of state appropriations increases credit card balances by $172-$274 in the four-year sector and by between $48 and $239 in the two-year sector. Despite evidence of higher credit card balances, delinquencies either remain unchanged or decline. For three of the samples, delinquencies do not change, while for the 30-35 year old two-year attendees there is a large and statistically significant decline in the proportion of debt that is delinquent of almost 25% per $1,000 of state appropriations. We interpret Table 9 as showing state appropriations increase consumption without generating adverse debt consequences.

The results in Table 10 tell a similar story. In Panels A and B we present results for whether an individual has an auto loan and the percent of the auto loan that is delinquent, respectively. Panels C and D show similar outcomes for home loans. For four-year students, these outcomes change negligibly, consistent with the broad findings that long-run outcomes are unaffected by state appropriations changes in this sector. For two-year students, there is more evidence of higher consumption and better debt outcomes. The likelihood an individual originates an auto loan by age 35 increases by 4 percentage points (5.17%) for each $1,000 of state higher education funding. The percent of auto loans that is delinquent among both 25-30 and 30-35 year olds declines by statistically significant amounts as well. Becoming delinquent on these loans is relatively rare, as shown in Table 1, so the effects in Table 10 are large relative to the mean. Two-year students also are more likely to take out a home loan when they experience state appropriations increases in college. The effect sizes are 2.4-3.0 percentage points, although only the 25-30 estimate is statistically significant at even the 10% level. We do not see much of an effect on delinquencies. Like with credit cards, Table 10 shows that state appropriations increases during college lead two-year students to incur more consumption-related debt into their mid-30s while experiencing (weakly) fewer adverse credit events.

Finally, in Table 11 we examine two summary measures of adult well-being: credit scores and the adjusted gross income (AGI) of the current zipcode of residence.[20] Our data do not contain labor market income, but credit score and neighborhood quality are both highly correlated

[20] In results available upon request, we also examine the effect of state appropriations on whether individuals have any accounts in collection or whether they declare bankruptcy. We find no evidence that these outcomes change for either age group or sector.

with income, so these are informative outcomes. Panel A shows that four-year students do not experience statistically significant increases in credit score, but the estimates are positive. In contrast, both age groups experience large positive increases in credit score when there are state appropriation increases in the two-year sector. The effect sizes are 5.7 and 13.1, respectively, for 25-30 year olds and 30-35 year olds. These are small in percent terms, due to the high mean of credit scores, but they are sizable relative to the standard deviation in scores of 88. Two-year students are in substantially better financial health into their 30s when they experience state appropriation increases in college.

Neighborhood quality also increases for two-year students (Panel B). An additional $1,000/student in appropriations increases the mean AGI of the neighborhood at age 35 by $3,359, which is a 7.37% increase. The estimate at age 30 is much smaller and is not statistically significant. Because two-year students are investing in education into their 30s, as shown in Tables 5-8, it makes sense that the effects on neighborhood quality take time to appear. Panel C shows similar results but uses the difference between the current zipcode AGI and the initial zipcode AGI as the dependent variable. This is a measure of economic mobility, and it is positive and economically large for the two-year sample at age 35. However, it is not statistically significantly different from zero. In Panel D, we show results using the Neighborhood Quality Index discussed in Section 2.4. The results indicate that two-year students live in neighborhoods that are 4-10 percent of a standard deviation better for each $1,000/student of state appropriations they experience. Taken together, the results in Table 11 show that state appropriations have positive effects on the long-run well-being of two-year students.

The effect of state appropriation increases on neighborhood quality among four-year students is less clear. The point estimate in Panel B for 30 year olds is positive but not significant at conventional levels, while the estimate among 35-year-olds is negative and statistically significant. A similar pattern emerges when we examine the change in neighborhood income in Panel C and the neighborhood quality index in Panel D. This is a somewhat surprising result, but it is consistent with the evidence in Table 6 showing that four-year students react to state appropriations increases by obtaining a BA more quickly but not attending graduate school. As a result, they live in more affluent neighborhoods earlier on in their lives, but then those

who experience smaller appropriations changes catch up by the time they are in their mid-30s.

4.3 Mechanisms

How do institutions respond to state appropriation changes and do those responses align with the effects on long-run outcomes we show above? In Table 12, we present institution-level reduced form estimates of how our shift-share instrument affects institutional resources. To conduct this exercise, we average outcomes over the same 150% degree time used in our individual-level regressions. The time period of analysis is constructed to overlap the years in which those in the NSC-CCP data are enrolled in college.

The first three columns show coefficients, effect sizes, and percent effect sizes in the four-year sector.[21] There is a clear negative effect of state appropriation increases on tuition: a $1,000 increase in appropriations per student leads to a reduction in in-state tuition of $483, in out-of-state tuition of $713, and of net tuition (which incorporates institutional financial aid) of $521.[22] Assuming 150% time enrollment, a net tuition increase of $521 beginning in the freshman year leads to $3,126 in additional tuition payments. The loan origination estimates in Table 8 are $1,717 at age 25 and $3,204 at age 30, which align closely with the change in tuition payments in Table 12. Indeed, student loan originations increase by about 9% due to a $1,000/student reduction in appropriations per student, which is almost exactly the percent increase in net tuition.

The remaining rows of Table 12 show that four-year institutions do not alter other resources in response to state appropriation cuts. We do see a positive correlation between state appropriations and capital appropriations, but the capital effect is quite small. Total appropriations change nearly one-for-one with state appropriations, suggesting our estimates are not biased by ignoring other forms of state, federal, or local support. Expenditures change little with state appropriations, with the exception of research expenditures and institutional support that are not likely to have large effects on student progress.[23] These results are consistent with the lack

[21] Sample means on which the percent effects are based are shown in Online Appendix Table A-7.

[22] This estimate is qualitatively similar to Webber (2017), who uses state-year level variation in state appropriations and shows that post-2000, over 30% of appropriations decreases are passed through to students in the form of higher net tuition.

[23] Dinerstein et al. (2015) show that increases in federal research revenue from the 2009 fiscal stimulus generated an increase in research expenditures, a decline in state appropriations, and an increase in net tuition at public universities. Our results broadly align with theirs in demonstrating a negative relationship between state appropriations and tuition as well as a negative relationship between state appropriations and research funds. State appropriations affect a broader set of institutions than does federal research funding, which underscores the importance of examining the former source of funding variation directly.

of long-run effects for four-year students: state appropriations affect net price but not institutional resource allocation. Students who experience declines in state support take on more debt, but they receive the same quality of education. Importantly, the increase in student debt does not appear to have long-run adverse consequences for these students.

In the two-year sector, there is both a price and a quality response. Net tuition revenue declines by $354 due to a $1,000 state appropriation increase, which translates to $1,062 more in tuition payments over the three years of enrollment. In Table 8, student loan originations by age 25 decline by $618 per thousand dollars per student of state funding. Unlike the four-year sector, there is evidence of changes in other appropriations and in institutional resources when states alter their support. Local appropriations decline by about $300 per student when state appropriations increase by $1,000 per student, so part of the incidence of state appropriations falls on local taxpayers through this substitution mechanism. One implication of this result is that our two-year estimates are biased towards zero, since $1,000 of state appropriations leads to only $699 of total appropriations. Table 12 also shows evidence that two-year schools alter their educational inputs when state appropriations change. For each $1,000 of state appropriations, academic support expenditures increase by $38 (7.0%) per student[24] and instructional expenditures increase by $98 (3.4%) per student, although the latter estimate is not statistically significant. There also is a 0.003 (5.8%) increase in the faculty/student ratio, which has been shown to be an important resource in driving student academic attainment (Bound, Lovenheim, and Turner 2010). The fact that long-run outcomes of two-year students are so responsive to state appropriation changes is consistent with the evidence in Table 12 on how institutions alter educational resources when state funding changes.

One question that arises from these results is why there is a quality and price effect in the two-year sector while there is only a price response in the four-year sector. We argue this likely is driven by the fact that tuition is already very low in community colleges. As a result, there is less scope for price adjustments in this sector.

[24] Webber and Ehrenberg (2010) show evidence that academic support expenditures are important determinants of student success in the postsecondary sector.

4.4 Robustness Checks

There are three types of threats to our identification strategy: variation in local economic conditions that correlate with the instrument and with outcomes, serial correlation in the instrument generating bias from secular trends in outcomes across schools with different shares, and the instrument correlating with the composition of students *already enrolled* in a given institution. In this section, we assess the robustness of our results to these different sources of potential confounding variation. In the interest of brevity, we show estimates for student loan outcomes only. Other outcomes are available upon request. We show estimates for the four-year sector in Table 13 and for the two-year sector in Table 14.

4.4.1 Local Economic Variation

The first set of potential confounders that we explore is local economic variation. State appropriations variation (the shift) is pro-cyclical: bias can arise from changes in economic conditions that correlate with state appropriation variation *differentially across areas with different institutional shares*. If, for example, a recession leads to lower state appropriations and the recession is deeper in areas with higher-share schools in them, our estimates of the effect of state appropriations on student outcomes may be biased. We run a series of robustness checks to assess the sensitivity of our results to such variation.

In row (1) of Tables 13 and 14, we include in equation (4) an interaction of the baseline share with county unemployment at the expected time of college exit (c+5 for four-years and c+2 for two-years). We also control for the uninteracted county unemployment rate. This specification check is motivated in part by the finding that graduating in a recession can harm long-run labor market outcomes (Kahn 2010; Oreopoulos, Von Wachter, and Heisz 2012). Across outcomes, age groups and sectors, the estimates change little when we include these controls. We next include controls for baseline share interacted with the home price index of the county at the time of expected exit in row (2). This helps to account in particular for variation in state appropriations coming from the great recession. The estimates are extremely similar to baseline. Our results similarly are robust to controlling for baseline share interacted with county unemployment in the year of college entry and in sophomore year, which are shown in rows (3) and (4) of

Tables 13 and 14.[25] In results not shown, our results are similar when we control for baseline share interacted with county unemployment or the home price index in *any* year of potential enrollment. Together, the results in rows (1)-(4) show that our estimates are not sensitive to variation in local economic activity at the point of entry, during college, or at the expected time of exit. Row (5) shows results that include share interacted with fixed effects for whether all levels of state government are run by Democrats or Republicans (with mixed government being the excluded category). The results again change little with the inclusion of these controls, suggesting our results are not confounded by changes in the state political environment.

4.4.2 Serial Correlation in the Instrument

We next examine whether secular trends in outcomes across cohorts in each institution bias our estimates to the extent that they are correlated with serial correlation in the instrument. Recall that Online Appendix Figures A-7 through A-16 show no evidence of pretrends in outcome variables by quartiles of the 2005 and 2014 baseline shares. We also see no evidence of such trends by quartiles of the instrument. The next set of robustness checks further examine the sensitivity of our results to this potential source of bias.

In row (6) of Tables 13 and 14, we show estimates that control for college entry year fixed effects interacted with the rolling share. This model accounts for secular variation in the composition of the entering cohorts that is correlated with the state appropriations share. The results are very similar to baseline. In row (7), we conduct a similar exercise but interact baseline share with the expected year of college exit fixed effects (based on 150% time enrollment) to account for any exit-year shocks that may differentially affect students coming from colleges with different shares. The results change little when we include these controls. The next robustness check (row 8) includes state-by-freshman-cohort fixed effects. This variation is collinear with the state-level shifts in appropriations, and it accounts for any state-by-year level shocks that are correlated with state higher education funding and potential student outcomes. Some of the estimates are smaller and some are larger than those shown in the main results, but overall they are qualitatively similar. The estimates in row (8) are less precise, however, and so while the overall story does not change with these controls our statistical power does. In Row (9)

[25] In all of these regressions, we also control for uninteracted unemployment and home price measures.

of Table 13, we include an interaction between the state-freshman cohort fixed effects and an indicator for whether a college is in the top 3 selectivity categories of the Barron's rankings. Allowing for state-cohort shocks by college selectivity yields similar results.[26]

In row (10) of Table 13 and row (9) of Table 14, we include state-by-birth cohort fixed effects. The extensive margin debt effects are somewhat attenuated but are qualitatively similar, while the loan amount effects more closely match the baseline results. The next row in each table include controls for state appropriations per college-age resident to which each student is exposed during the assumed 150% enrollment time. For the four-year sector, the estimates become larger (in absolute value) when we add this control. In the two-year sector, the point estimates are attenuated, but they also are imprecise and the 95% confidence intervals typically include the baseline estimates. The results in rows (6)-(11) in Table 13 and in rows (6)-(10) in Table 14 show that our estimates are robust to the existence of secular trends across cohorts that are correlated with baseline share and with state appropriations in the state in which each institution is located.

4.4.3 Composition of Students and Other Robustness Checks

We also examine directly how the instrument is related to selection of different students into postsecondary schools. In row (12) of Table 13 and (11) of Table 14, we control for all of the initial Census Block observables shown in Table 3. Consistent with a lack of an effect on these observables, controlling for them does not change the estimates.

One may be concerned that state flagship institutions rely much less on state funding and tend to have endowments as well as research funding that can shield them from declines in state support. They also tend to serve a more advantaged population. As Bound et al. (2019) show, these institutions react differently to state appropriation changes than do less research-oriented publics. Our estimates change negligibly when we exclude these institutions (row 13 of Table 13).

In row (14) of Table 13 and row (12) of Table 14, we use the fixed baseline share instrument that fixes baseline state appropriations shares at their 1986 levels.[27] These estimates show the

[26] We also include a lagged value of the instrument as a control to test for bias from serial correlation. We lack the power for such an analysis: coefficients are qualitatively similar but are imprecise. The estimates from Table 3 as well as those from Tables 13 and 14 provide extensive evidence that the estimates are not biased from serial correlation in the instrument.

[27] First stage estimates using the fixed baseline instrument are shown in Online Appendix Table A-6.

effect of cumulative shocks since 1986. The results and conclusions are similar to those that use the rolling baseline, although the estimates are less precise.

Throughout the analysis, we have assumed students are enrolled for 150% of statutory degree time based on the initial sector. When we instead assume 100% degree time enrollment, the results and conclusions are unchanged (row 15 in Table 13 and row 13 in Table 14).

4.4.4 Falsification Tests

Finally, we conduct two falsification tests that provide further support for the validity of our identification assumptions. In row (16) of Table 13 and row (14) of Table 14, we examine how state appropriations variation affects private colleges and universities (which do not receive state appropriations). To conduct this test, we need to assign private colleges a state appropriations share. We do so by assigning each college the average share of public colleges in the state and year with a similar Barron's selectivity ranking. If there are no public universities of similar selectivity (for example, no public college in Massachusetts is as selective as Harvard, MIT, Amherst, or Williams), we assign them shares of universities that are the closest in terms of selectivity. This falsification test examines whether similarly-selective private universities are "affected" by state appropriation changes, which would occur if there is secular variation at the local level that impacts colleges of similar selectivity regardless of public/private status. The private school estimates are universally small, are not statistically significant in any specification, and generally are of the opposite sign of the treatment effects presented above. These results strongly support our identification strategy by showing that similar private universities in the same state and year are unaffected by state appropriation shocks: the shift-share instrument is not simply picking up changes in unobservables that differentially affect institutions of varying selectivity.

The second falsification test uses transportation funding per college-age resident rather than state appropriations per college-age resident as the shift variable in the instrument. This specification examines whether we are picking up secular variation correlated with the health of state budgets rather than the state appropriation funding itself. Row (17) of Table 13 and row (15) of Table 14 shows the results of this specification. Like the private school test, the estimates tend to be small in magnitude, are not statistically significant from zero in most

columns, and they are often of the opposite sign from the baseline results. These estimates indicate that our findings are not driven by *any* changes in state budgets but are specific to higher education funding.[28]

5 Conclusion

This paper presents the first analysis in the literature on how changes in state appropriations while students are enrolled in college affect short- and long-run educational and credit outcomes. We contribute to existing research along two dimensions. The first is the use of an instrument for state appropriations that exploits variation in how state-level funding variation differentially impacts institutions depending on their historic reliance on state revenues. This shift-share approach has been used recently by Deming and Walters (2017), but we extend it to examine long-run outcomes, to focus on variation occurring *after* students have made initial enrollment decisions, and to examine variation during the course of college enrollment. The second contribution is to use a new dataset that combines administrative postsecondary enrollment records from the National Student Clearinghouse (NSC) with credit data from New York Fed Consumer Credit Panel (CCP). The data provide a level of detail regarding postsecondary enrollment behavior, student debt and default, and long-run financial outcomes that are not available in other datasets.

Our findings indicate that state appropriation increases when students are enrolled in college have important impacts on student outcomes that vary by the sector of first enrollment. In the four-year sector, state appropriation increases largely generates a price response: net tuition decreases and student loan originations decline by a similar amount. Four-year students shorten their time to obtain a BA, but they also are less likely to enroll in graduate school. We do not find consistent or economically significant effects on other long-run outcomes, which is aligned with the lack of an education resource response by colleges and universities. The incidence of state appropriation changes falls on students in terms of changes in prices, but educational quality and the gross returns to the educational investment do not change.

[28] We cannot conduct a similar falsification test using K-12 funding because K-12 funding is highly correlated with higher education funding. That we examine variation from when individuals are already enrolled in college suggests that we are not picking up effects of K-12 funding.

For students who first entered the postsecondary system at a community college, there is both a price and a quality response. Increases in state appropriations generate large increases in educational attainment, including substantially higher rates of BA and graduate attainment. As a result, student debt actually increases, but the likelihood of student debt delinquency or default decline. Two-year students also are more likely to take out auto and home loans into their mid-30s when they experience state appropriation increases during college, and adverse debt outcomes decline. State funding increases lead to higher credit scores and to students living in higher-income neighborhoods in their mid-30s as well. Overall, state appropriation increases lead these students to be consuming more, have better credit with lower rates of adverse debt outcomes, and to be living in more affluent areas by the age of 35. This effect is consist with our analysis of institutional resources: community colleges change net price but also core educational resources when states alter their funding. These resource changes likely drive the changes in long-run returns to investing in community colleges that we document.

The results from our analysis have several policy implications. First, they suggest that state appropriations have a large effect on the return to postsecondary investments made by young adults that last into later adulthood, especially in the two-year sector. State appropriations are under the direct control of policymakers, and so our results are relevant for those making state budgeting decisions in supporting expenditures on higher education. Furthermore, our estimates relate more generally to the question of whether increased spending affects postsecondary and longer-run outcomes. The results we present suggest this is indeed the case, and although we examine revenues from one source – state governments – there is no reason to believe that revenues from different sources would have different impacts on students. Lastly, our estimates relate to ongoing policy concerns regarding student debt. Student debt levels have risen substantially over the past several decades, which has caused large concerns about the "affordability" of higher education. Our results suggest that cuts to state appropriations are an important contributor to this trend, which also implies that increases in state funding can be used to reduce student debt burdens associated with financing higher education.

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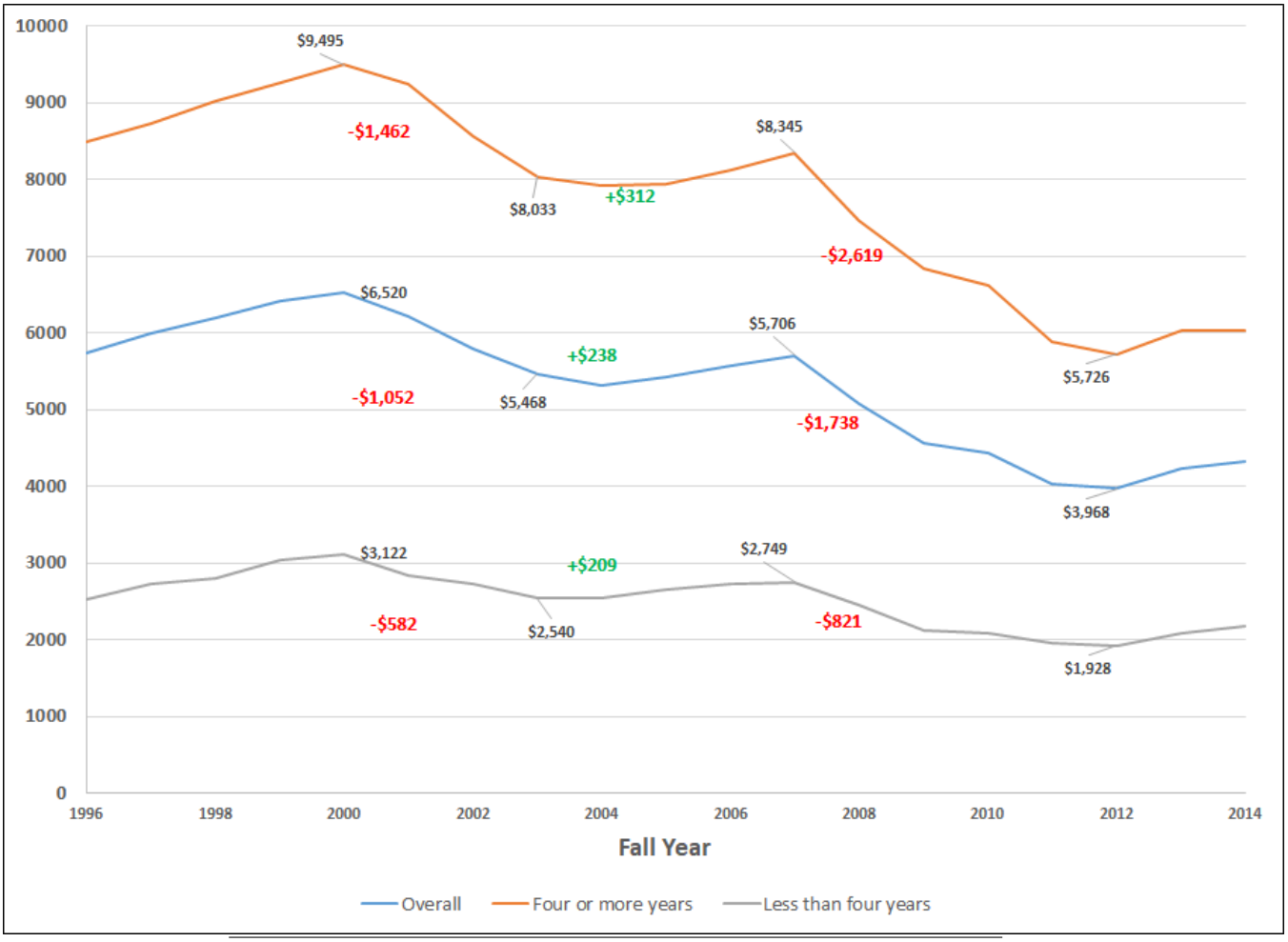
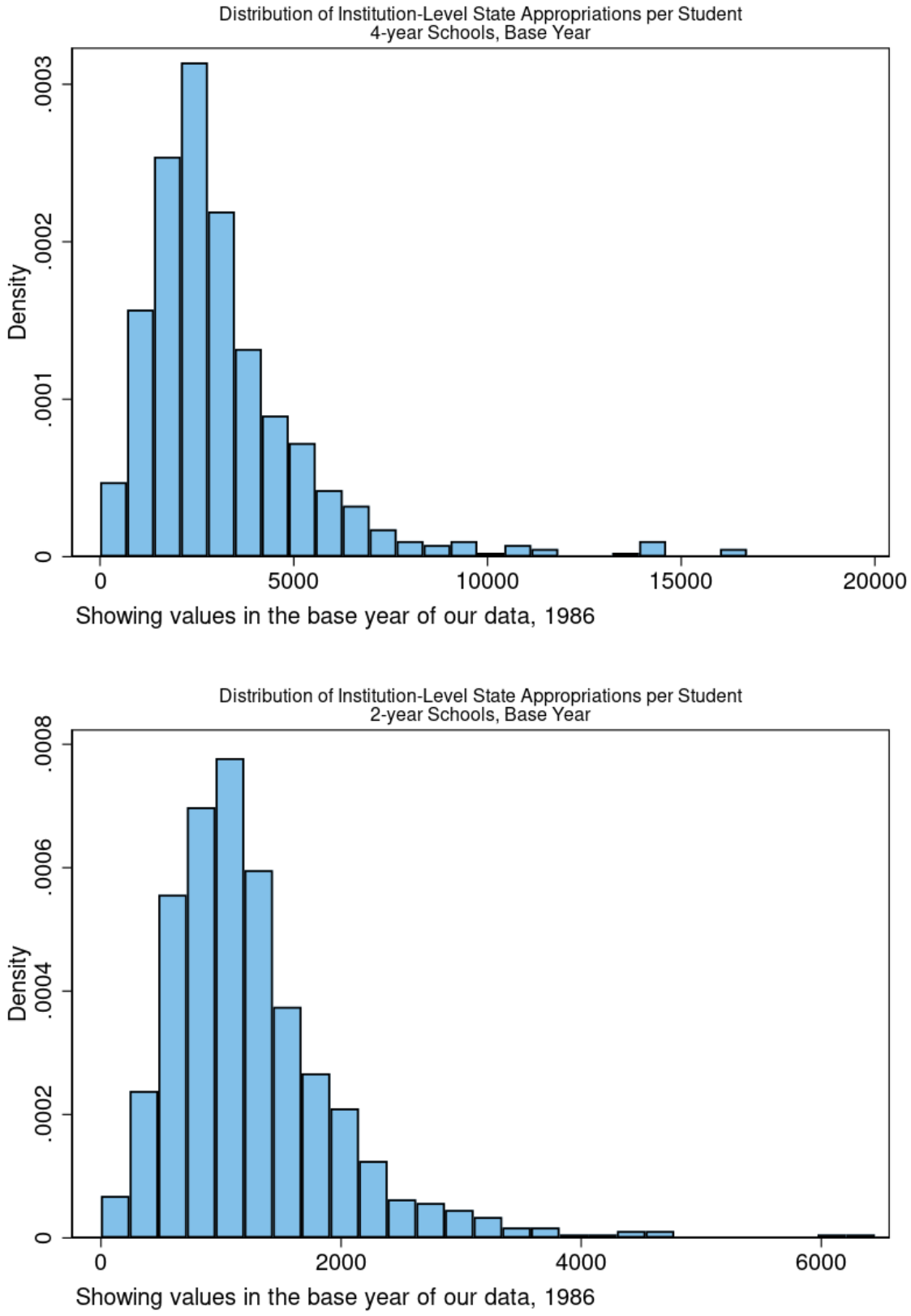
Figure 1: Trends in State Appropriations over Time



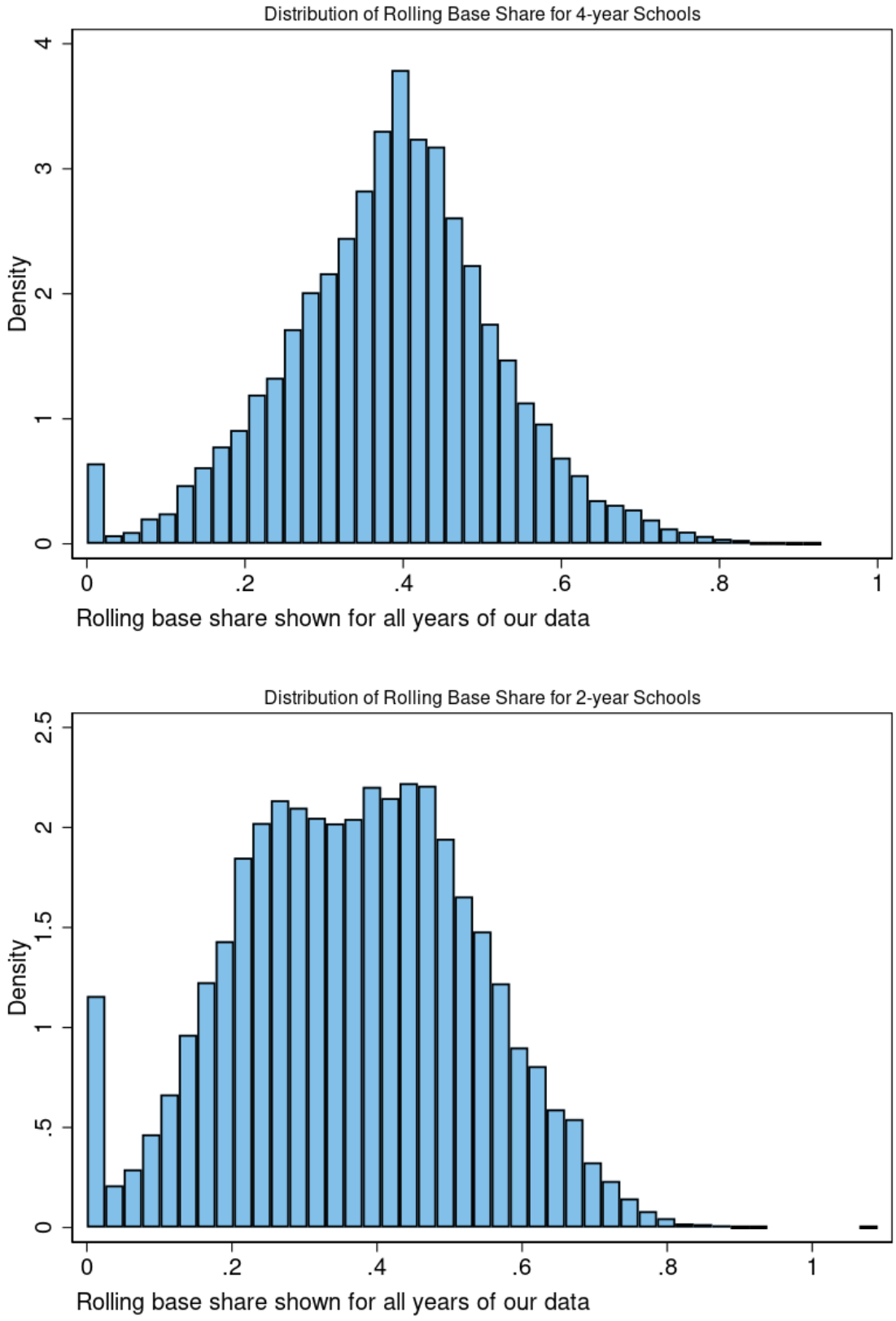
	% Change in SA/Student			
Institution Type	2000-2003	2004-2007	2007-2012	2012-2014
4-Year	-15.39%	5.29%	-31.39%	5.39%
2-Year	-18.64%	8.40%	-29.85%	13.12%

Figure 2: Distribution of Institution-Level State Appropriations per Student (Base Year)



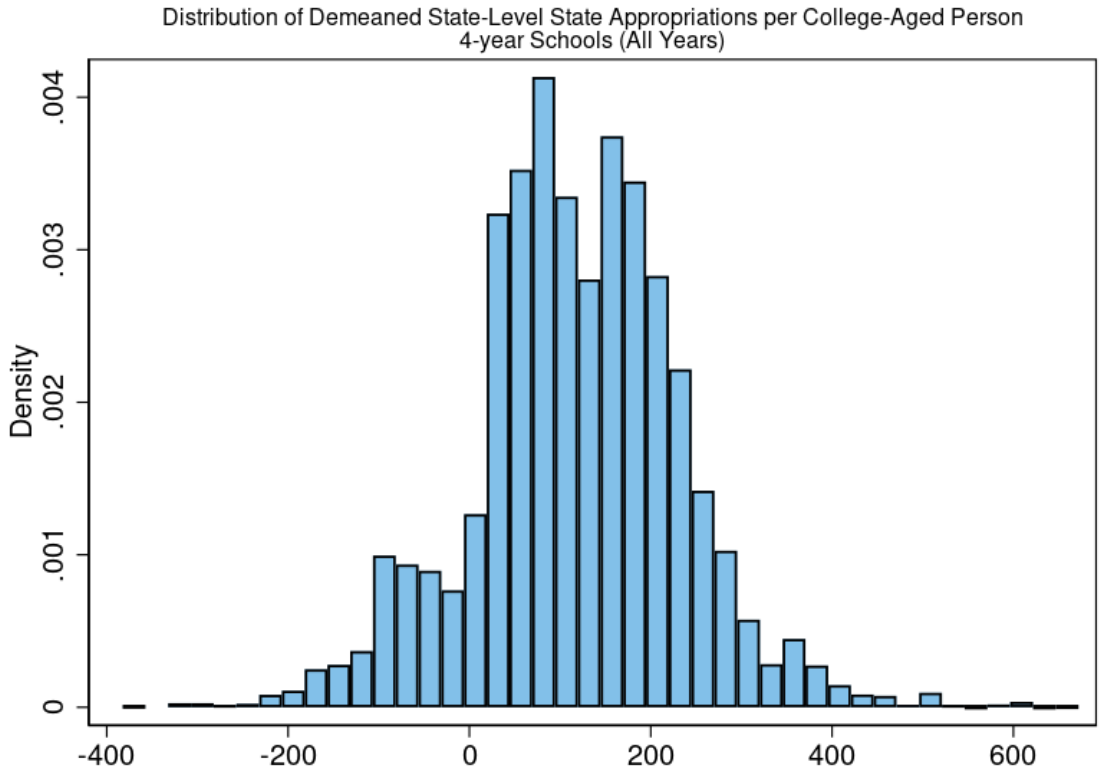
Each panel shows the distribution of institution-level state appropriations per student from IPEDS. The top panel shows the four-year distribution, and the bottom panel shows two-year schools.

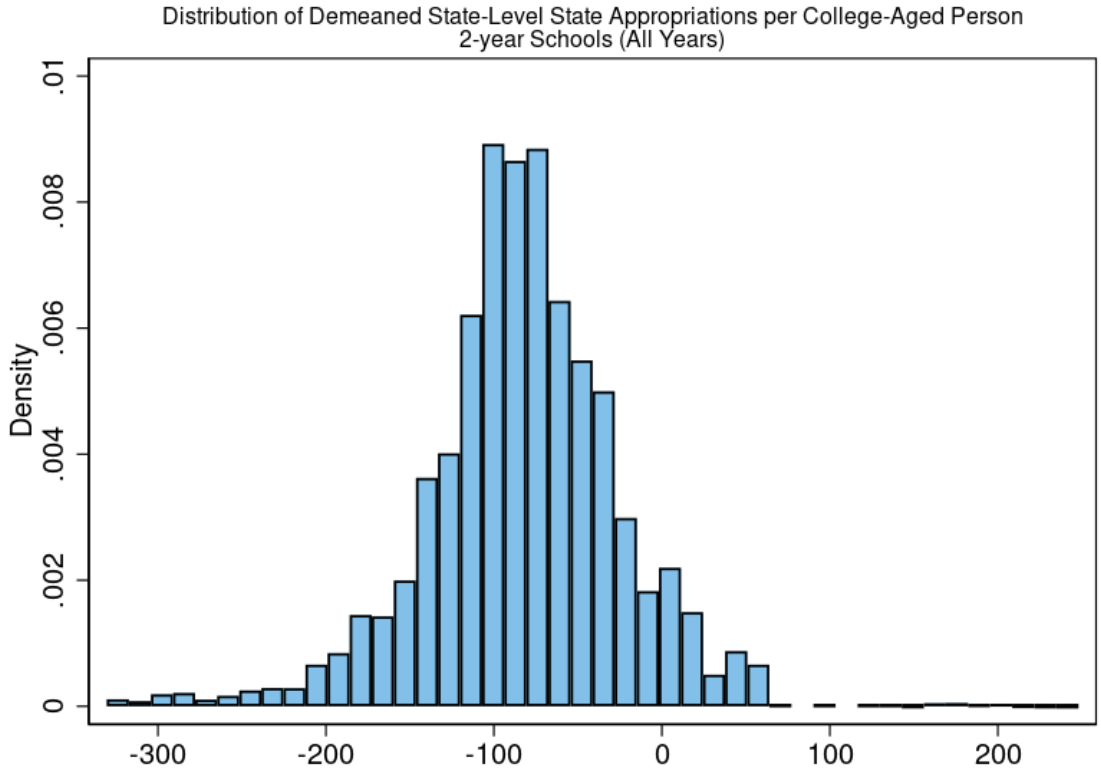
Figure 3: Distribution of Baseline Share (Base Year)



Each panel shows the distribution of rolling baseline share of revenues constituted by state appropriations at the institution level from IPEDS. The top panel shows the four-year distribution, and the bottom panel shows two-year schools.

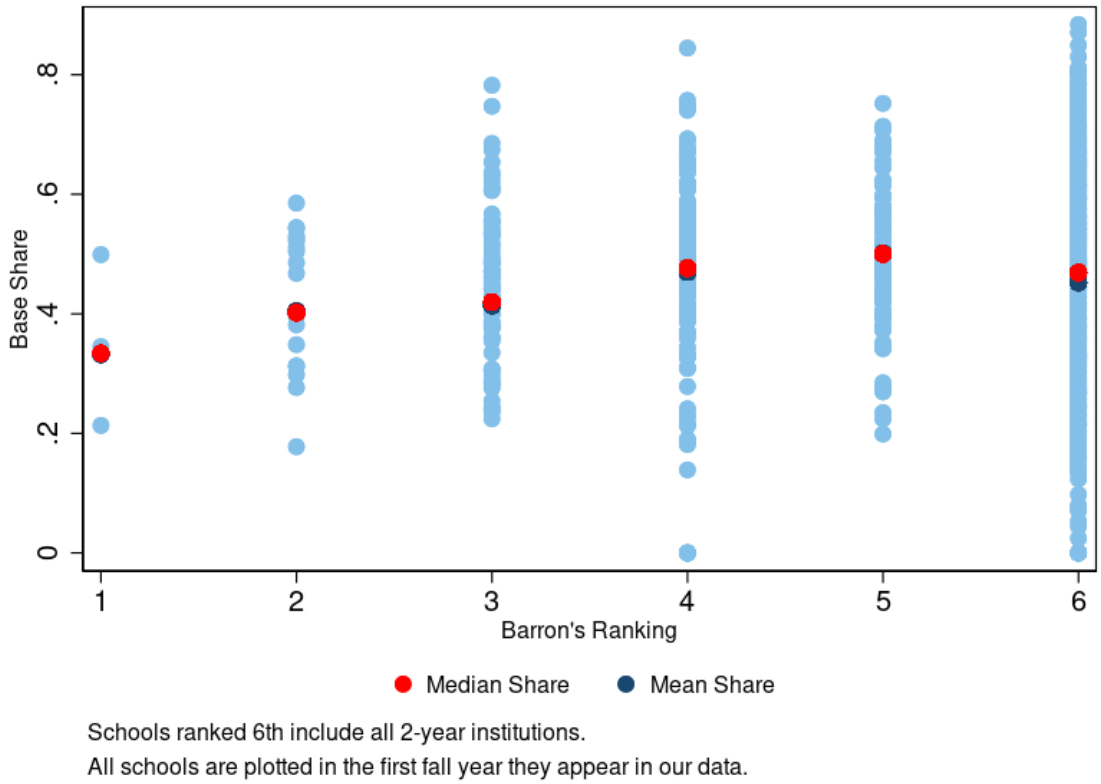
Figure 4: Demeaned State-Level Appropriations per College-Age Resident (All Years)





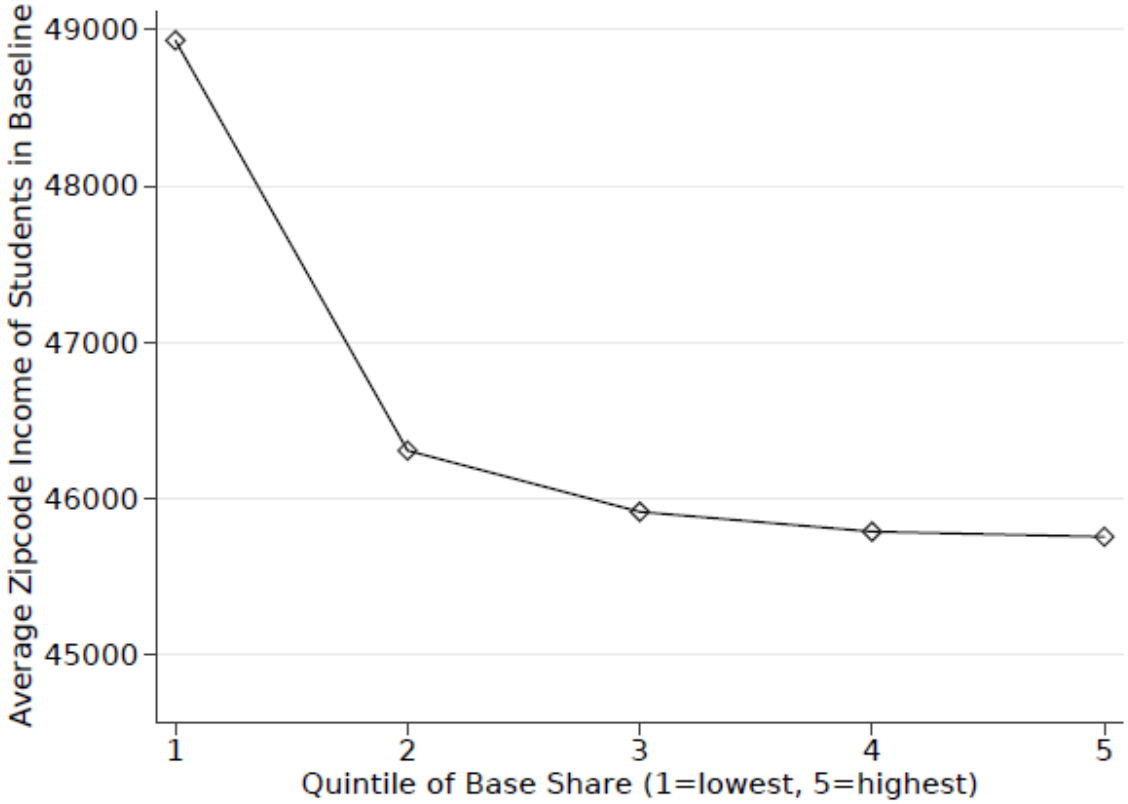
Each panel shows the distribution of state-level appropriations per college-age resident, which have been demeaned with respect to the statewide mean over the analysis period. The top panel shows the four-year distribution, and the bottom panel shows two-year schools.

Figure 5: Distribution of Base Share by Institution Selectivity



The figure shows the distribution of 3-year rolling baseline state appropriation shares by institutional selectivity from the Barron's rankings. We combine the bottom two four-year selectivity categories in Barron's into category 5, and all two year schools are in category six.

Figure 6: Average Baseline Income by Base Share Quintile



The figure shows the average AGI of households in initial zipcodes by the quintile of baseline share of the first postsecondary institution in which students enroll.

Table 1: Summary Statistics of CCP Variables

	4-year Schools		2-year Schools	
Variable	**mean**	**std. dev.**	**mean**	**std. dev.**
Student Loans:				
Originate Student Loan by 22	0.57	0.49	0.36	0.48
Originate Student Loan by 25	0.63	0.48	0.45	0.50
Originate Student Loan by 30	0.67	0.47	0.52	0.50
Originate Student Loan by 35	0.68	0.47	0.54	0.50
Origination Amount - Age 22	10,240.17	15,639.51	4,423.14	10,295.00
Origination Amount - Age 25	20,001.92	33,193.25	9,082.88	20,867.41
Origination Amount - Age 30	35,704.41	63,448.62	17,283.00	38,098.60
Origination Amount - Age 35	46,093.20	86,203.95	23,346.82	51,205.39
% Student Loan Balance Delinquent (25-30)	6.16	18.81	7.59	21.08
% Student Loan Balance Delinquent (30-35)	6.13	19.51	8.52	22.81
Default on Student Loan by 30	0.12	0.32	0.13	0.33
Default on Student Loan by 35	0.14	0.34	0.17	0.38
Age 25-30 Outcomes:				
Mean Credit Card Balance	2,732.37	4,107.33	2,012.79	3,848.24
Percent Credit Card Balance Delinquent	10.01	25.21	15.80	30.69
Own Car	0.69	0.46	0.69	0.46
Percent Auto Loan Delinquent	1.89	10.98	4.18	16.42
Own Home	0.37	0.48	0.30	0.46
Percent Home Loan Delinquent	0.69	6.06	1.24	8.48
Declare Bankruptcy	0.03	0.16	0.05	0.22
Any Account in Collection	0.41	0.49	0.61	0.49
Credit Score	671.55	88.38	631.79	88.14
Mean AGI of Current Zip Code	50,766.55	39,606.77	45,258.82	29,053.29
AGI of Current - Initial Zip Code	3,648.85	41,731.46	1,302.79	29,520.27
Neighborhood Quality Index (SD)	0.57	1.00	0.32	0.89
Age 30-35 Outcomes:				
Mean Credit Card Balance	4,045.84	7,690.83	2,821.96	5,413.45
Percent Credit Card Balance Delinquent	8.64	23.18	14.41	28.95
Own Car	0.80	0.40	0.78	0.41
Percent Auto Loan Delinquent	1.98	11.21	4.74	17.64
Own Home	0.57	0.50	0.43	0.50
Percent Home Loan Delinquent	1.39	9.35	2.13	11.87
Declare Bankruptcy	0.05	0.22	0.11	0.31
Any Account in Collection	0.47	0.50	0.69	0.46
Credit Score	692.62	94.67	642.99	95.15
Mean AGI of Current Zip Code	51,285.77	28,270.02	45,602.24	30,734.26
AGI of Current - Initial Zip Code	5,605.13	31,768.53	2,316.04	32,797.02
Neighborhood Quality Index (SD)	0.62	1.00	0.33	0.89

Source: Merged Consumer Credit Panel, National Student Clearinghouse, and IPEDS data as described in the text. All tabulations are done at the individual-level.

Table 2: Institution-Level Summary Statistics

	4-year Schools		2-year Schools	
Variable	**mean**	**std. dev.**	**mean**	**std. dev.**
Institution-Level Approp. per Student	5,838.65	4,858.59	2,321.98	1,302.50
State-Level Approp. per College-Age Pop.	384.72	174.39	108.33	74.56
Base Share	0.35	0.12	0.34	0.15
$\widetilde{SA}$	131.57	61.82	41.57	32.50
Base Share, Selective	0.30	0.11		
Base Share, Nonselective	0.36	0.11		
Institutional Enrollment	13,886.03	10,891.14	7,836.51	7,161.30
Number of Institutions	619		936	

Source: IPEDS data as described in the text. Summary statistics are at the institutional level. Selective institutions are those that have a rating of "Selective" or higher in the Baron's rankings.

Table 3: Selection into College as a Function of the Instrument

	4-Year		2-Year	
Dependent Variable	Estimate	Effect Size ($\frac{\$1000}{Student}$)	Estimate	Effect Size ($\frac{\$1000}{Student}$)
HH Income (Census Block)	-1618.89	-608.15	44.79	21.49
	(1090.61)		(1254.32)	
HH Income (Zip - Tax Data)	-737.31	-276.98	44.79	-152.97
	(786.11)		(862.25)	
Above Median HH Income (Zip)	-0.01	-0.004	0.01	0.005
	(0.01)		(0.02)	
Percent White	-0.00	0.000	0.01	0.005
	(0.01)		(0.01)	
Percent Black	-0.00	0.000	-0.01	-0.005
	(0.01)		(0.01)	
Percent Married	0.00	0.000	-0.00	0.000
	(0.01)		(0.01)	
Percent HH with Kids<18	0.00	0.000	-0.00	0.000
	(0.02)		(0.01)	
Percent BA+	0.00	0.000	-0.02**	-0.010
	(0.01)		(0.01)	
Percent Some College	-0.00	0.000	-0.00	0.000
	(0.00)		(0.00)	
Percent No College	-0.00	0.000	0.02*	0.010
	(0.01)		(0.01)	
Median Gross Rent	-12.73	-4.78	5.91	2.84
	(15.67)		(19.07)	
Median Home Value	85.75	32.21	-8893.05	-4269.35
	(5880.12)		(7358.31)	
LFP Rate	-0.00	0.000	0.00	0.000
	(0.00)		(0.01)	
Employment Rate	0.00	0.000	-0.00	0.000
	(0.00)		(0.00)	
Total Population	1.78	0.67	5.36	2.57
	(42.63)		(82.68)	
Total Population ≥25	-23.15	-8.70	13.24	6.36
	(26.72)		(42.04)	

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text and the 3-year rolling baseline shift-share instrument. All regressions include cohort-by-age and institution fixed effects as well as controls for baseline state appropriations share, state college-age population, and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. Each cell in the "Estimate" columns is a separate regression. The instrument is in units of $100 per college-age resident. The "Effect Size" shows the reduced form estimate divided by the first stage in Table 4. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 4: First Stage Results: Birth Cohorts 1975-1988 and 1975-1983

Dep. Var.: State Appropriations per Student				
	Panel A: 4-Year			
Birth years	1975-1988		1975-1983	
Age	25-30		30-35	
	(1)	(2)	(3)	(4)
$\widetilde{SA}$	23.27***	26.62***	18.54***	25.22***
	(2.08)	(0.99)	(2.45)	(1.18)
Institution FE	N	Y	N	Y
Observations	19199	19180	9120	9096
	Panel B: 2-Year			
Birth years	1975-1988		1975-1983	
	(1)	(2)	(3)	(4)
$\widetilde{SA}$	17.61***	20.84***	13.76***	19.83***
	(1.05)	(1.03)	(1.44)	(1.20)
Institution FE	N	Y	N	Y
Observations	27568	27536	12432	12369

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text. All regressions include cohort-by-age and institution fixed effects as well as controls for baseline state appropriations share, state college-age population, and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. State appropriations is expressed in $ per student and the state appropriations instrument is expressed in $ per college-age resident in the state. The 1975-1988 birth year cohorts constitute the sample for age 25-30 outcomes and the 1975-1983 birth year cohorts constitute the sample for age 30-35 outcomes. The state appropriations instrument ($\widetilde{SA}$) uses the 3-year lagged rolling baseline state appropriations share as shown in equation (1). Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 5: The Effect of State Appropriations on Transfer Behavior

Panel A: Transfer to a 4-year Institution		
College Level:	2-Year	
Age:	30	Ever
Independent Var.	(3)	(4)
$\widetilde{SA}$	0.06*	0.07***
	(0.04)	(0.02)
Effect Size ($\frac{\$1000}{student}$)	[0.031]	[0.035]
% Effect Size	{16.981}	{15.433}
Observations	14431	36368

Panel B: Transfer to a More Selective Institution				
College Level:	4-Year		2-Year	
Age:	30	Ever	30	Ever
Independent Var.	(1)	(2)	(3)	(4)
$\widetilde{SA}$	0.01	0.01	0.08***	0.06***
	(0.02)	(0.01)	(0.02)	(0.02)
Effect Size ($\frac{\$1000}{student}$)	[0.004]	[0.004]	[0.041]	[0.030]
% Effect Size	{9.758}	{6.428}	{37.049}	{20.284}
Observations	110830	26878	114431	36368

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text and the 3-year rolling baseline shift-share instrument. All regressions include cohort-by-age and institution fixed effects as well as controls for baseline state appropriations share, state college-age population, and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. The state appropriations instrument is expressed in $100 per college-age resident in the state. The "Effect Size" shows the reduced form estimate divided by the first stage in Online Appendix Table A-5 and the "% Effect Size" shows the effect size divided by the sample mean. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 6: The Effect of State Appropriations on Collegiate Attainment

Panel A: Years of Postsecondary Education						
College Level:		4-Year			2-Year	
Age:	25	30	Ever	25	30	Ever
Independent Var.	(1)	(2)	(3)	(4)	(5)	(6)
$\widetilde{SA}$	0.27***	0.21	0.06	0.04	-0.03	0.38**
	(0.09)	(0.19)	(0.11)	(0.15)	(0.26)	(0.16)
Effect Size ($\frac{\$1000}{student}$)	[0.102]	[0.082]	[0.023]	[0.020]	[-0.015]	[0.193]
% Effect Size	{2.48}	{1.61}	{0.48}	{0.72}	{-0.45}	{5.17}
Observations	21306	11083	26878	28638	14431	36368
Panel B: Earn BA or Graduate Degree						
College Level:		4-Year			2-Year	
Age:	25	30	Ever	25	30	Ever
Independent Var.	(1)	(2)	(3)	(4)	(5)	(6)
$\widetilde{SA}$	0.04*	0.02	-0.01	0.08***	0.05	0.09***
	(0.02)	(0.04)	(0.02)	(0.02)	(0.03)	(0.02)
Effect Size ($\frac{\$1000}{student}$)	[0.015]	[0.008]	[-0.004]	[0.039]	[0.025]	[0.046]
% Effect Size	{2.30}	{0.96}	{-0.48}	{27.13}	{15.34}	{20.12}
Observations	21067	10964	26571	28134	14187	35718
Panel C: Highest Degree=BA						
College Level:		4-Year			2-Year	
Age:	25	30	Ever	25	30	Ever
Independent Var.	(1)	(2)	(3)	(4)	(5)	(6)
$\widetilde{SA}$	0.05**	0.05	0.02	0.05**	0.02	0.05**
	(0.02)	(0.04)	(0.02)	(0.02)	(0.03)	(0.02)
Effect Size ($\frac{\$1000}{student}$)	[0.019]	[0.019]	[0.008]	[0.024]	[0.010]	[0.025]
% Effect Size	{4.03}	{4.52}	{2.08}	{18.76}	{7.27}	{18.09}
Observations	21067	10964	26571	28134	14187	35718
Panel D: Earn Graduate Degree						
College Level:		4-Year			2-Year	
Age:	25	30	Ever	25	30	Ever
Independent Var.	(1)	(2)	(3)	(4)	(5)	(6)
$\widetilde{SA}$	-0.01	-0.03	-0.02*	0.03***	0.03	0.04***
	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)
Effect Size ($\frac{\$1000}{student}$)	[-0.004]	[-0.012]	[-0.008]	[0.015]	[0.015]	[0.020]
% Effect Size	{-4.21}	{-5.30}	{-5.50}	{48.78}	{25.43}	{40.53}
Observations	21067	10964	26571	28134	14187	35718

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text and the 3-year rolling baseline shift-share instrument. All regressions include cohort-by-age and institution fixed effects as well as controls for baseline state appropriations share, state college-age population, and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. The state appropriations instrument is expressed in $100 per college-age resident in the state. The "Effect Size" shows the reduced form estimate divided by the first stage in Online Appendix Table A-5 and the "% Effect Size" shows the effect size divided by the sample mean. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 7: The Effect of State Appropriations on Field of Study Among Graduates

Panel A: Receive a STEM Degree						
College Level:		4-Year			2-Year	
Age:	25	30	Ever	25	30	Ever
Independent Var.	(1)	(2)	(3)	(4)	(5)	(6)
$\widetilde{SA}$	0.01	-0.01	-0.02	0.02	-0.01	0.04**
	(0.01)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)
Effect Size ($\frac{\$1000}{student}$)	[0.004]	[-0.004]	[-0.008]	[0.010]	[-0.005]	[0.020]
% Effect Size	{1.895}	{-1.621}	{-4.053}	{10.840}	{-5.086}	{20.262}
Observations	21067	10964	26571	28134	14187	35718
Panel B: Receive a Business Degree						
College Level:		4-Year			2-Year	
Age:	25	30	Ever	25	30	Ever
Independent Var.	(1)	(2)	(3)	(4)	(5)	(6)
$\widetilde{SA}$	0.01	-0.00	-0.00	0.05***	0.05***	0.05***
	(0.01)	(0.03)	(0.01)	(0.02)	(0.02)	(0.02)
Effect Size ($\frac{\$1000}{student}$)	[0.004]	[-0.000]	[-0.000]	[0.024]	[0.025]	[0.025]
% Effect Size	{2.368}	{-0.000}	{-0.000}	{40.650}	{31.790}	{36.185}
Observations	21067	10964	26571	28134	14187	35718
Panel C: Receive a Liberal Arts Degree						
College Level:		4-Year			2-Year	
Age:	25	30	Ever	25	30	Ever
Independent Var.	(1)	(2)	(3)	(4)	(5)	(6)
$\widetilde{SA}$	-0.00	-0.05*	-0.02	0.04*	0.03	0.03
	(0.02)	(0.03)	(0.01)	(0.02)	(0.03)	(0.02)
Effect Size ($\frac{\$1000}{student}$)	[-0.000]	[-0.019]	[-0.008]	[0.020]	[0.015]	[0.015]
% Effect Size	{-0.000}	{-8.456}	{-4.530}	{19.512}	{13.872}	{13.816}
Observations	21067	10964	26571	28134	14187	35718
Panel D: Receive a Vocational Degree						
College Level:		4-Year			2-Year	
Age:	25	30	Ever	25	30	Ever
Independent Var.	(1)	(2)	(3)	(4)	(5)	(6)
$\widetilde{SA}$	0.01	0.05	0.00	0.03*	-0.01	0.04**
	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)
Effect Size ($\frac{\$1000}{student}$)	[0.004]	[0.019]	[0.000]	[0.015]	[-0.005]	[0.020]
% Effect Size	{2.229}	{9.724}	{0.000}	{14.634}	{-3.633}	{16.886}
Observations	21067	10964	26571	28134	14187	35718

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text and the 3-year rolling baseline shift-share instrument. All regressions include cohort-by-age and institution fixed effects as well as controls for baseline state appropriations share, state college-age population, and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. The state appropriations instrument is expressed in $100 per college-age resident in the state. The "Effect Size" shows the reduced form estimate divided by the first stage in Online Appendix Table A-5 and the "% Effect Size" shows the effect size divided by the sample mean. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 8: The Effect of State Appropriations on Student Loans

	Panel A: Ever Originate Student Loans							
College Level:	4-Year				2-Year			
By Age:	22	25	30	35	22	25	30	35
Independent Var.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\widetilde{SA}$	-0.05**	-0.05**	-0.06***	-0.06	-0.04**	-0.05**	-0.03	-0.12**
	(0.01)	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)	(0.03)	(0.06)
Effect Size ($\frac{\$1000}{student}$)	[-0.019]	[-0.019]	[-0.023]	[-0.024]	[-0.020]	[-0.024]	[-0.014]	[-0.061]
% Effect Size	{-3.358}	{-3.007}	{-3.364}	{-3.499}	{-5.623}	{-5.420}	{-2.768}	{-11.206}
Observations	31972	30603	19643	9250	50607	45175	28524	12888
	Panel B: Origination Amount (Dollars)							
College Level:	4-Year				2-Year			
By Age:	22	25	30	35	22	25	30	35
Independent Var.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\widetilde{SA}$	-1670.48***	-4530.57***	-8529.72***	-13525.15**	-810.89*	-1267.53	2958.93	4503.89
	(431.15)	(1012.32)	(2514.25)	(5755.98)	(451.08)	(980.40)	(2203.72)	(4267.34)
Effect Size ($\frac{\$1000}{student}$)	[-639.54]	[-1716.78]	[-3204.25]	[-5362.87]	[-410.37]	[-618.31]	[1419.83]	[2271.25]
% Effect Size	{-6.25}	{-8.58}	{-8.97}	{-11.63}	{-9.28}	{6.81}	{8.22}	{9.73}
Observations	31972	30603	19643	9250	50607	45175	28524	12888
	Panel C: Percent of Student Loans that are Delinquent							
College Level:	4-Year				2-Year			
Age:	25-30	30-35			25-30	30-35		
Independent Var.	(3)	(4)			(7)	(8)		
$\widetilde{SA}$	0.36	-0.29			-2.93**	-6.89**		
	(0.89)	(1.79)			(1.43)	(2.75)		
Effect Size ($\frac{\$1000}{student}$)	[0.135]	[-0.115]			[-1.406]	[-3.475]		
% Effect Size	{2.195}	{-1.876}			{-18.524}	{-40.781}		
Observations	19643	9250			28524	12888		
	Panel D: Default on Student Loan							
College Level:	4-Year				2-Year			
Age:	25-30	30-35			25-30	30-35		
Independent Var.	(3)	(4)			(7)	(8)		
$\widetilde{SA}$	-0.01	-0.05*			-0.04*	-0.09**		
	(0.01)	(0.03)			(0.02)	(0.04)		
Effect Size ($\frac{\$1000}{student}$)	[-0.004]	[-0.020]			[-0.019]	[-0.045]		
% Effect Size	{-3.130}	{-14.161}			{-14.765}	{-26.698}		
Observations	19643	9250			28524	12888		

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text and the 3-year rolling baseline shift-share instrument. All regressions include cohort-by-age and institution fixed effects as well as controls for baseline state appropriations share, state college-age population, and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. The state appropriations instrument is expressed in $100 per college-age resident in the state. The "Effect Size" shows the reduced form estimate divided by the first stage in Table 4 and Online Appendix Table A-4, and the "% Effect Size" shows the effect size divided by the sample mean. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 9: The Effect of State Appropriations on Credit Card Debt

Panel A: Credit Card Balance (Including Zeros)				
College Level:	4-Year		2-Year	
Age:	25-30	30-35	25-30	30-35
Independent Var.	(1)	(2)	(3)	(4)
$\widetilde{SA}$	729.73**	433.09	100.54	474.00
	(85.94)	(591.20)	(225.88)	(531.67)
Effect Size ($\frac{\$1000}{student}$)	[274.13]	[171.72]	[48.24]	[239.03]
% Effect Size	{10.03}	{4.24}	{2.40}	{8.47}
Observations	19643	9250	28524	12888

Panel B: Percent Credit Card Balance Delinquent (0-100)				
College Level:	4-Year		2-Year	
Age:	25-30	30-35	25-30	30-35
Independent Var.	(1)	(2)	(3)	(4)
$\widetilde{SA}$	-0.44	-1.64	0.18	-7.07**
	(1.15)	(1.92)	(2.34)	(3.31)
Effect Size ($\frac{\$1000}{student}$)	[-0.165]	[-0.650]	[0.086]	[-3.565]
% Effect Size	{-1.651}	{-7.526}	{0.547}	{-24.742}
Observations	19643	9250	28524	12888

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text and the 3-year rolling baseline shift-share instrument. All regressions include cohort-by-age and institution fixed effects as well as controls for baseline state appropriations share, state college-age population, and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. The state appropriations instrument is expressed in $100 per college-age resident in the state. The "Effect Size" shows the reduced form estimate divided by the first stage in Table 4 and the "% Effect Size" shows the effect size divided by the sample mean. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 10: The Effect of State Appropriations on Auto and Home Loans

Panel A: Have an Auto Loan				
College Level:	4-Year		2-Year	
By Age:	30	35	30	35
Independent Var.	(1)	(2)	(3)	(4)
$\widetilde{SA}$	0.02	0.01	-0.00	0.08*
	(0.02)	(0.04)	(0.03)	(0.05)
Effect Size ($\frac{\$1000}{student}$)	[0.008]	[0.004]	[-0.000]	[0.040]
% Effect Size	{1.089}	{0.496}	{-0.000}	{5.172}
Observations	19643	9250	28524	12888
Panel B: Percent Auto Loan Delinquent (0-100)				
College Level:	4-Year		2-Year	
Age:	25-30	30-35	25-30	30-35
Independent Var.	(1)	(2)	(3)	(4)
$\widetilde{SA}$	-0.04	-0.32	-2.80**	-4.78*
	(0.48)	(0.96)	(1.24)	(2.44)
Effect Size ($\frac{\$1000}{student}$)	[-0.015]	[-0.127]	[-1.344]	[-2.410]
% Effect Size	{-0.795}	{-6.408}	{-32.143}	{-50.854}
Observations	19643	9250	28524	12888
Panel C: Have a Home Loan				
College Level:	4-Year		2-Year	
By Age:	30	35	30	35
Independent Var.	(1)	(2)	(3)	(4)
$\widetilde{SA}$	0.02	-0.01	0.05*	0.06
	(0.02)	(0.04)	(0.03)	(0.05)
Effect Size ($\frac{\$1000}{student}$)	[0.008]	[-0.004]	[0.024]	[0.030]
% Effect Size	{2.31}	{-0.696}	{7.997}	{7.037}
Observations	19643	9250	28524	12888
Panel D: Percent Home Loan Delinquent (0-100)				
College Level:	4-Year		2-Year	
Age:	25-30	30-35	25-30	30-35
Independent Var.	(1)	(2)	(3)	(4)
$\widetilde{SA}$	-0.18	-2.03**	-0.05	-0.26
	(0.26)	(0.95)	(0.52)	(1.31)
Effect Size ($\frac{\$1000}{student}$)	[-0.068]	[-0.805]	[-0.024]	[-0.131]
% Effect Size	{-9.800}	{-57.908}	{-1.974}	{-6.156}
Observations	19643	9250	28524	12888

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text and the 3-year rolling baseline shift-share instrument. All regressions include cohort-by-age and institution fixed effects as well as controls for baseline state appropriations share, state college-age population, and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. The state appropriations instrument is expressed in $100 per college-age resident in the state. The "Effect Size" shows the reduced form estimate divided by the first stage in Table 4 and the "% Effect Size" shows the effect size divided by the sample mean. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 11: The Effect of State Appropriations on Credit Score and Neighborhood Quality

Panel A: Credit Score				
College Level:	4-Year		2-Year	
Age:	25-30	30-35	25-30	30-35
Independent Var.	(1)	(2)	(3)	(4)
$\widetilde{SA}$	2.25	7.11	11.91**	26.00**
	(4.08)	(7.86)	(5.71)	(11.29)
Effect Size ($\frac{\$1000}{student}$)	[0.845]	[2.819]	[5.715]	[13.111]
% Effect Size	{0.126}	{0.407}	{0.905}	{2.039}
Observations	19419	9181	27737	12626
Panel B: AGI of Current Zip Code				
College Level:	4-Year		2-Year	
By Age:	30	35	30	35
Independent Var.	(1)	(2)	(3)	(4)
$\widetilde{SA}$	2093.63	-4198.36**	194.08	6661.27**
	(1618.25)	(2087.28)	(2021.35)	(2705.18)
Effect Size ($\frac{\$1000}{student}$)	[786.49]	[-1664.69]	[93.13]	[3359.19]
% Effect Size	{1.55}	{-3.25}	{0.21}	{7.37}
Observations	19193	9058	27624	12574
Panel C: Difference Between AGI of Current and Initial Zip Code				
College Level:	4-Year		2-Year	
By Age:	30	35	30	35
Independent Var.	(1)	(2)	(3)	(4)
$\widetilde{SA}$	3199.30*	-3319.26	-1936.84	3115.67
	(1830.60)	(2120.52)	(2006.63)	(2645.81)
Effect Size ($\frac{\$1000}{student}$)	[1201.84]	[-1316.12]	[-929.39]	[1571.19]
% Effect Size	{32.94}	{-23.48}	{-71.34}	{67.84}
Observations	19119	9016	27488	12507
Panel D: Neighborhood Quality Index (SD)				
College Level:	4-Year		2-Year	
By Age:	30	35	30	35
Independent Var.	(1)	(2)	(3)	(4)
$\widetilde{SA}$	0.04	-0.14*	0.08*	0.19**
	(0.04)	(0.08)	(0.05)	(0.08)
Effect Size ($\frac{\$1000}{student}$)	[0.02]	[-0.06]	[0.04]	[0.10]
% Effect Size	{2.64}	{-8.95}	{12.00}	{31.94}
Observations	19301	9136	27750	12647

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text and the 3-year rolling baseline shift-share instrument. All regressions include cohort-by-age and institution fixed effects as well as controls for baseline state appropriations share, state college-age population, and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. The Neighborhood Quality Index is created by standardizing the median zip code home value, AGI, and percent with a BA and then taking the average of these three standardized variables. The state appropriations instrument is expressed in $100 per college-age resident in the state. The "Effect Size" shows the reduced form estimate divided by the first stage in Table 4, and the "% Effect Size" shows the effect size divided by the sample mean. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 12: Mechanisms: Effects on Per-Student Revenues and Expenditures

Dependent Variable	Estimate	4-Year Effect Size ($\frac{\$1000}{Student}$)	Percent Effect	Estimate	2-Year Effect Size ($\frac{\$1000}{Student}$)	Percent Effect
In-State Tuition/Fees	-1285.92***	-483.07	-8.90	-1006.87***	-483.38	-19.68
	(133.63)			(101.72)		
Out-of-State Tuition/Fees	-1898.80***	-713.30	-5.09	-665.36***	-319.42	-5.60
	(294.72)			(138.98)		
Net Tuition Revenue	-1386.57***	-520.88	-9.70	-1324.74***	-353.92	-25.98
	(141.40)			(211.62)		
Capital Grants & Gifts	-337.84**	-126.91	-32.60	-29.72	-14.27	-21.25
	(135.60)			(32.36)		
Capital Appropriations	462.36***	173.69	29.31	191.23***	91.81	33.28
	(104.87)			(63.73)		
Local Appropriations	-25.40	-9.54	-23.98	-627.01***	-301.01	-26.98
	(17.48)			(100.35)		
Federal Appropriations	-13.75	-5.17	-8.68	-23.28***	-11.18	-94.00
	(10.42)			(5.44)		
Total Appropriations	2522.85***	947.73	14.80	1456.08***	699.03	21.44
	(103.66)			(134.91)		
Instructional Expenditures	-60.55	-22.75	-0.31	203.37	97.63	3.41
	(143.61)			(153.20)		
Research Expenditures	-384.53***	-144.45	-4.41	0.60	0.29	10.99
	(110.78)			(1.67)		
Academic Support Expend.	-42.76	-16.06	-0.82	78.95***	37.90	6.98
	(62.98)			(29.42)		
Student Services	-43.38	-16.30	-1.40	0.13	0.06	0.01
	(38.03)			(43.03)		
Institutional Support	207.44***	77.93	4.16	-35.86	-17.22	-1.80
	(70.63)			(53.22)		
Faculty/Student Ratio (FTE)	0.0019**	0.0007	1.19	0.0060***	0.0029	5.76
	(0.0009)			(0.0014)		
Fraction International	-0.0007**	-0.0003	-0.016	0.002***	0.001	0.157
	(0.0002)			(0.0005)		
Fraction Out of State	-0.002***	-0.001	-0.005	0.010***	0.005	0.096
	(0.001)			(0.002)		

Authors' estimation of equation (4) using IPEDS data as described in the text and the 3-year rolling baseline shift-share instrument. The sample comprises all years in which those in the CCP-NSC data were enrolled in college. All regressions include institution fixed effects, baseline year fixed effects, and controls for baseline state appropriations share and state college-age population. The state appropriations instrument is expressed in $100 per college-age resident in the state. The "Effect Size" shows the reduced form estimate divided by the first stage in Table 4, and the "Percent Effect Size" shows the effect size divided by the sample mean. Standard errors clustered at the state-by-base year level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 13: Robustness Checks - Four-Year Students

	Dep. Var.:	Originate a Student Loan				Origination Amount (Dollars)			
	Age:	22	25	30	35	22	25	30	35
	Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1)	Share*County Unemployment	-0.04**	-0.05**	-0.06***	-0.06	-1595.23***	-4452.76***	-8151.98***	-14570.37**
	at Expected Time of College Exit	(0.01)	(0.02)	(0.02)	(0.04)	(434.91)	(1023.94)	(2525.25)	(5655.39)
	Effect Size ($\frac{\$1000}{student}$)	[-0.015]	[-0.019]	[-0.023]	[-0.024]	[-610.73]	[-1687.29]	[-3062.35]	[-5777.31]
	% Effect Size	{-2.687}	{-3.007}	{-3.364}	{-3.499}	{-5.96}	{-8.44}	{-8.58}	{-12.53}
(2)	Share*County Home Price Index	-0.05***	-0.05***	-0.07***	-0.08*	-1664.43***	-4645.28***	-8994.04***	-15852.99***
	at Expected Time of College Exit	(0.02)	(0.02)	(0.02)	(0.05)	(444.87)	(1047.01)	(2649.33)	(5938.07)
	Effect Size ($\frac{\$1000}{student}$)	[-0.019]	[-0.019]	[-0.026]	[-0.032]	[-637.22]	[-1760.24]	[-3378.68]	[-6285.88]
	% Effect Size	{-3.358}	{-3.007}	{-3.925}	{-4.665}	{-6.22}	{-8.80}	{-9.46}	{-13.64}
(3)	Share*County Unemployment	-0.04**	-0.05**	-0.07***	-0.09*	-1308.20***	-3991.29***	-9346.45***	-17098.75***
	in Year of College Entry	(0.02)	(0.02)	(0.02)	(0.05)	(435.49)	(1037.73)	(2650.20)	(5892.71)
	Effect Size ($\frac{\$1000}{student}$)	[-0.015]	[-0.019]	[-0.026]	[-0.036]	[-500.84]	[-1512.43]	[-3511.06]	[-6779.84]
	% Effect Size	{-2.687}	{-3.007}	{-3.925}	{-5.248}	{-4.89}	{-7.56}	{-9.83}	{-14.71}
(4)	Share*County Unemployment	-0.04**	-0.05**	-0.07***	-0.09*	-1309.04***	-4352.44***	-9925.34***	-18847.76***
	in Sophomore Year	(0.02)	(0.02)	(0.02)	(0.05)	(446.66)	(1054.03)	(2616.05)	(5930.51)
	Effect Size ($\frac{\$1000}{student}$)	[-0.015]	[-0.019]	[-0.026]	[-0.036]	[-501.16]	[-1649.28]	[-3728.53]	[-7473.34]
	% Effect Size	{-2.687}	{-3.007}	{-3.925}	{-5.248}	{-4.89}	{-8.25}	{-10.44}	{-16.21}
(5)	Share*State Political	-0.06***	-0.06***	-0.07***	-0.05	-2039.00***	-5492.24***	-9385.99***	-14567.94**
	Party Composition FE	(0.02)	(0.02)	(0.02)	(0.04)	(435.10)	(1056.92)	(2556.25)	(6345.15)
	Effect Size ($\frac{\$1000}{student}$)	[-0.023]	[-0.023]	[-0.026]	[-0.020]	[-780.63]	[-2081.18]	[-3525.92]	[-5776.34]
	% Effect Size	{-4.030}	{-3.609}	{-3.925}	{-2.916}	{-7.62}	{-10.40}	{-9.88}	{-12.53}
(6)	Share*College Entry Year FE	-0.04***	-0.05***	-0.07***	-0.09*	-1604.62***	-4771.96**	-8021.27***	-17481.83**
		(0.01)	(0.02)	(0.02)	(0.04)	(439.23)	(1043.85)	(2542.96)	(6181.25)
	Effect Size ($\frac{\$1000}{student}$)	[-0.015]	[-0.019]	[-0.026]	[-0.036]	[-614.33]	[-1808.25]	[-3013.25]	[-6931.73]
	% Effect Size	{-2.687}	{-3.007}	{-3.925}	{-5.248}	{-6.00}	{-9.04}	{-8.44}	{-15.04}
(7)	Share*Expected College	-0.04***	-0.05***	-0.06***	-0.06	-1469.57***	-4489.41***	-7968.69***	-14027.38**
	Exit Year FE	(0.01)	(0.02)	(0.02)	(0.04)	(440.40)	(1036.13)	(2515.57)	(6116.08)
	Effect Size ($\frac{\$1000}{student}$)	[-0.015]	[-0.019]	[-0.023]	[-0.024]	[-562.62]	[-1701.18]	[-2993.50]	[-5562.01]
	% Effect Size	{-2.687}	{-3.007}	{-3.364}	{-3.499}	{-5.49}	{-8.51}	{-8.38}	{-12.07}
(8)	State-Freshman Cohort FE	-0.03	-0.11***	-0.02	-0.10	-1107.22	-8848.11**	-11464.94	-15205.56
		(0.04)	(0.05)	(0.07)	(0.12)	(1174.65)	(3584.38)	(8920.46)	(18414.71)
	Effect Size ($\frac{\$1000}{student}$)	[-0.011]	[-0.042]	[-0.008]	[-0.040]	[-423.90]	[-3352.83]	[-4306.89]	[-6029.17]
	% Effect Size	{-2.015}	{-6.616}	{-1.121}	{-5.831}	{-4.14}	{-16.76}	{-12.06}	{-13.08}
(9)	State-Freshman Cohort -	-0.04***	-0.05***	-0.07***	-0.08*	-1625.26***	-4790.53***	-8184.64***	-18503.36***
	Selective College FE	(0.02)	(0.02)	(0.02)	(0.04)	(438.93)	(1030.99)	(2592.30)	(6301.88)
	Effect Size ($\frac{\$1000}{student}$)	[-0.015]	[-0.019]	[-0.026]	[-0.032]	[-622.23]	[-1815.28]	[-3074.62]	[-7336.78]
	% Effect Size	{-2.687}	{-3.007}	{-3.925}	{-4.665}	{-6.08}	{-9.08}	{-8.61}	{-15.92}
(10)	State-Birth Cohort FE	-0.01	-0.05**	-0.06*	-0.02	-334.45	-2084.83	-7675.35**	-17369.31***
		(0.02)	(0.03)	(0.03)	(0.05)	(603.49)	(1480.42)	(3481.09)	(6625.31)
	Effect Size ($\frac{\$1000}{student}$)	[-0.004]	[-0.019]	[-0.023]	[-0.008]	[-128.04]	[-790.01]	[-2883.30]	[-6887.12]
	% Effect Size	{-0.672}	{-3.007}	{-3.364}	{-1.166}	{-1.25}	{-3.95}	{-8.08}	{-14.94}
(11)	Control for SA Level	-0.08***	-0.09***	-0.13***	-0.21***	-3113.54***	-7502.08***	-13125.02***	-24669.26**
	Shift during College	(0.03)	(0.03)	(0.04)	(0.08)	(713.92)	(1693.03)	(4278.60)	(10639.47)
	Effect Size ($\frac{\$1000}{student}$)	[-0.031]	[-0.034]	[-0.049]	[-0.083]	[-1192.01]	[-2842.77]	[-4930.51]	[-9781.63]
	% Effect Size	{-5.373}	{-5.413}	{-7.289}	{-12.245}	{-11.64}	{-14.21}	{-13.81}	{-21.22}
(12)	Control for Initial Census	-0.05***	-0.05**	-0.04*	-0.04	-1556.40***	-4308.51***	-7691.57***	-14645.13**
	Block Observables	(0.02)	(0.02)	(0.02)	(0.05)	(445.65)	(1045.68)	(2445.74)	(6463.59)
	Effect Size ($\frac{\$1000}{student}$)	[-0.019]	[-0.019]	[-0.015]	[-0.016]	[-595.87]	[-1632.63]	[-2889.40]	[-5806.95]
	% Effect Size	{-3.358}	{-3.007}	{-2.243}	{-2.332}	{-5.82}	{-8.16}	{-8.09}	{-12.60}

(13)	Exclude Flagship	-0.04*	-0.04**	-0.06**	-0.05	-1456.61***	-4074.57***	-9443.33***	-13563.36***
	Universities	(0.02)	(0.02)	(0.03)	(0.05)	(455.93)	(1045.25)	(2787.62)	(6717.16)
	Effect Size ($\frac{\$1000}{student}$)	[-0.015]	[-0.015]	[-0.023]	[-0.020]	[-557.66]	[-1543.98]	[-3547.46]	[-5378.02]
	% Effect Size	{-2.687}	{-2.406}	{-3.364}	{-2.916}	{-5.45}	{-7.72}	{-9.94}	{-11.67}
(14)	Fixed Baseline Instrument	-0.03**	-0.03**	-0.03	0.00	-898.58**	-2768.35***	-6426.48***	-6670.78
		(0.01)	(0.01)	(0.02)	(0.04)	(377.52)	(896.38)	(2525.25)	(4874.04)
	Effect Size ($\frac{\$1000}{student}$)	[-0.011]	[-0.011]	[-0.011]	[0.000]	[-344.02]	[-1049.01]	[-2414.15]	[-2645.04]
	% Effect Size	{-2.015}	{-1.804}	{-1.682}	{0.000}	{-3.36}	{-5.24}	{-6.76}	{-5.74}
(15)	100% Degree Time	-0.04***	-0.04***	-0.06***	-0.02	-1514.06***	-3705.12***	-7342.56***	-13201.44***
	in College	(0.01)	(0.02)	(0.02)	(0.04)	(409.97)	(979.29)	(2482.03)	(5427.01)
	Effect Size ($\frac{\$1000}{student}$)	[-0.015]	[-0.015]	[-0.023]	[-0.008]	[-579.66]	[-1403.99]	[-2758.29]	[-5234.51]
	% Effect Size	{-2.687}	{-2.406}	{-3.364}	{-1.166}	{-5.66}	{-7.02}	{-7.73}	{-11.36}
(16)	Private Institution	-0.00	-0.01	-0.00	-0.01	140.74	158.16	-133.13	1876.94
	Falsification	(0.00)	(0.00)	(0.01)	(0.01)	(210.82)	(355.58)	(678.53)	(1205.47)
(17)	Transportation Funding	0.00	0.00	0.04*	0.10*	-837.98	-1358.78	2600.98	22033.52*
	Falsification	(0.02)	(0.02)	(0.01)	(0.05)	(518.84)	(1319.98)	(1903.57)	(11840.33)

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text and the 3-year rolling baseline shift-share instrument. All regressions include cohort-by-age and institution fixed effects as well as controls for baseline state appropriations share, state college-age population, and a dummy for 1980 and later birth cohorts. The first row controls for base share interacted with county unemployment in the expected year of college exit assuming enrollment of 150% degree time. The second robustness check includes a control for baseline share interacted with the home price index in the county at the time of expected college exit assuming enrollment of 150% degree time. In row (14) we use a fixed baseline state appropriations share from 1986 to construct the instrument. The estimates that control for initial Census Block observables include the controls listed in Table 3. In row (16), we use only private institutions (including for-profits) and assign them the average baseline share of institutions in the state and year of the same Barron's selectivity rank. In row (17), we use transportation rather than higher education funding to construct the instrument. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 14: Robustness Checks - Two-Year Students

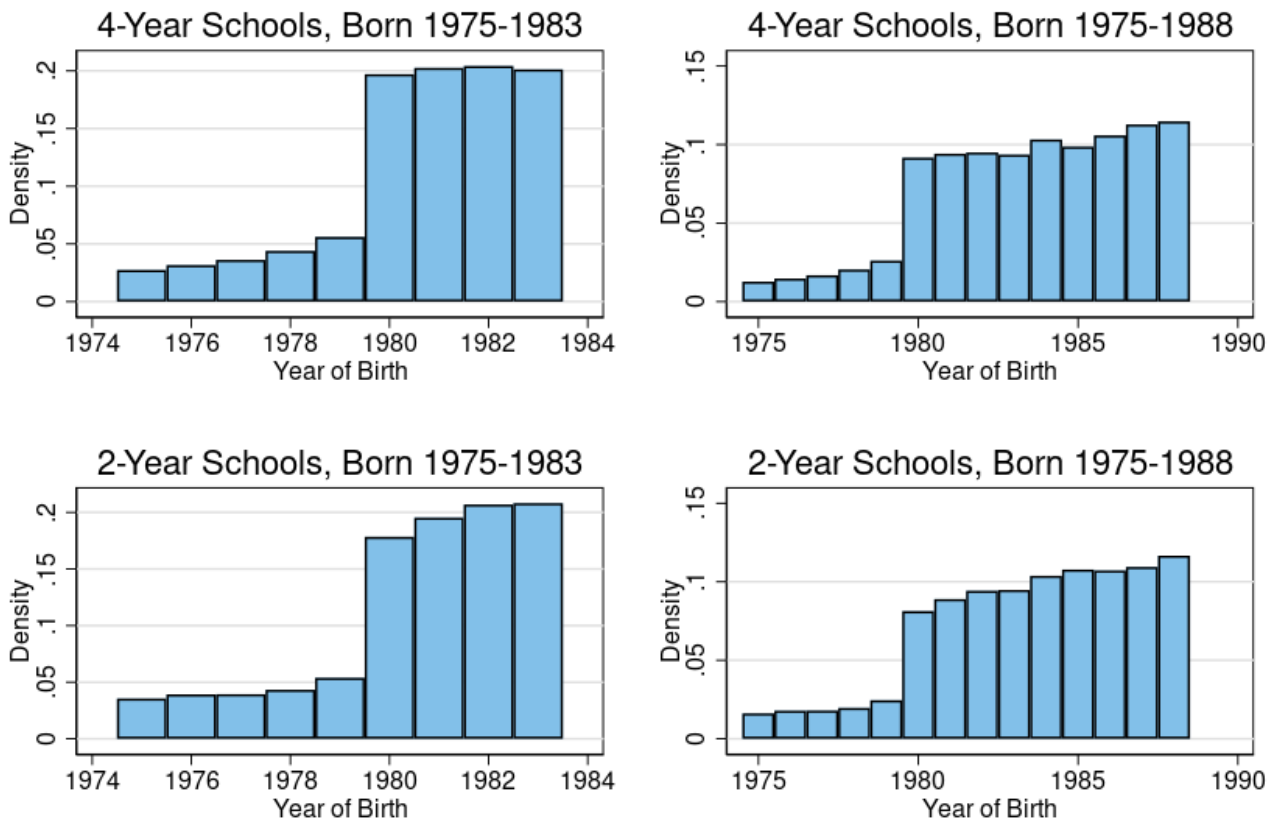
	Dep. Var.:	Originate a Student Loan				Origination Amount (Dollars)			
	Age:	22	25	30	35	22	25	30	35
	Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1)	Share*County Unemployment	-0.04**	-0.05**	-0.03	-0.13*	-851.42*	-1001.25	3150.70***	4490.12***
	at Expected Time of College Exit	(0.02)	(0.03)	(0.03)	(0.06)	(459.74)	(1032.98)	(346.11)	(753.32)
	Effect Size ($\frac{\$1000}{student}$)	[-0.020]	[-0.024]	[-0.014]	[-0.066]	[-430.88]	[-488.41]	[1511.85]	[2264.31]
	% Effect Size	{-5.623}	{-5.420}	{-2.768}	{-12.140}	{-9.74}	{-5.38}	{8.75}	{9.70}
(2)	Share*County Home Price Index	-0.06**	-0.06**	-0.04	-0.17**	-1247.03**	-1520.40	3741.11	5252.56
	at Expected Time of College Exit	(0.03)	(0.03)	(0.04)	(0.07)	(524.38)	(1150.63)	(2654.25)	(4650.80)
	Effect Size ($\frac{\$1000}{student}$)	[-0.030]	[-0.029]	[-0.019]	[-0.086]	[-631.09]	[-741.66]	[1795.16]	[2648.79]
	% Effect Size	{-8.435}	{-6.504}	{-3.691}	{-15.876}	{-14.27}	{-8.17}	{10.39}	{11.35}
(3)	Share*County Unemployment	-0.04**	-0.05**	-0.02	-0.12**	-787.46*	-1220.72	3167.45	4330.21
	in Year of College Entry	(0.02)	(0.03)	(0.03)	(0.06)	(435.49)	(456.06)	(2238.55)	(4290.19)
	Effect Size ($\frac{\$1000}{student}$)	[-0.020]	[-0.024]	[-0.010]	[-0.061]	[-398.51]	[-595.47]	[1519.89]	[2183.67]
	% Effect Size	{-5.623}	{-5.420}	{-1.846}	{-11.206}	{-9.01}	{-6.56}	{8.79}	{9.35}
(4)	Share*County Unemployment	-0.05**	-0.06**	-0.03	-0.13**	-822.29*	-1469.22	2733.39	4186.54
	in Sophomore Year	(0.02)	(0.03)	(0.03)	(0.06)	(459.14)	(1025.52)	(2244.22)	(4319.42)
	Effect Size ($\frac{\$1000}{student}$)	[-0.025]	[-0.029]	[-0.014]	[-0.066]	[-416.14]	[-716.69]	[1311.61]	[2111.22]
	% Effect Size	{-7.029}	{-6.504}	{-2.768}	{-12.140}	{-9.41}	{-7.89}	{7.59}	{9.04}
(5)	Share-State Political	-0.05**	-0.04	-0.01	-0.08	-767.68	-963.67	4037.66	5891.65
	Party Composition FE	(0.02)	(0.03)	(0.04)	(0.06)	(476.61)	(1051.02)	(2513.15)	(5122.85)
	Effect Size ($\frac{\$1000}{student}$)	[-0.025]	[-0.020]	[-0.005]	[-0.040]	[-388.50]	[-470.08]	[1937.46]	[2971.08]
	% Effect Size	{-7.029}	{-4.336}	{-0.923}	{-7.471}	{-8.87}	{-5.18}	{11.21}	{12.73}
(6)	Share-College Entry Year FE	-0.05**	-0.05**	-0.04	-0.15**	-767.62*	-998.37	3421.16	3778.13
		(0.02)	(0.02)	(0.03)	(0.06)	(458.43)	(1037.17)	(2249.83)	(4493.11)
	Effect Size ($\frac{\$1000}{student}$)	[-0.025]	[-0.019]	[-0.015]	[-0.059]	[-388.47]	[-487.01]	[1641.63]	[1905.26]
	% Effect Size	{-7.029}	{-4.210}	{-2.890}	{-11.014}	{-8.78}	{-5.36}	{9.50}	{8.16}
(7)	Share-Expected College	-0.03	-0.04*	-0.02	-0.12**	-185.00	-829.29	3229.58	4206.14
	Exit Year FE	(0.02)	(0.02)	(0.03)	(0.06)	(470.78)	(997.54)	(2217.73)	(4295.53)
	Effect Size ($\frac{\$1000}{student}$)	[-0.015]	[-0.020]	[-0.010]	[-0.061]	[-93.62]	[-404.53]	[1549.70]	[2121.10]
	% Effect Size	{-4.217}	{-4.336}	{-1.846}	{-11.206}	{-2.12}	{-4.45}	{8.97}	{9.09}
(8)	State-Freshman Cohort FE	0.00	-0.03	-0.10	-0.06	-1084.24	-2104.67	-9.63	7057.23
		(0.04)	(0.05)	(0.07)	(0.11)	(936.90)	(2362.50)	(5182.07)	(9697.78)
	Effect Size ($\frac{\$1000}{student}$)	[0.000]	[-0.011]	[-0.038]	[-0.024]	[-548.70]	[-1026.67]	[-4.62]	[3558.87]
	% Effect Size	{0.000}	{-2.526}	{-7.224}	{-4.406}	{-12.41}	{-11.30}	{-0.03}	{15.24}
(9)	State-Birth Cohort FE	0.06**	0.00	-0.02	-0.13**	906.84*	2038.13*	5528.94**	4036.48
		(0.03)	(0.03)	(0.04)	(0.06)	(549.98)	(1191.51)	(2502.32)	(4368.05)
	Effect Size ($\frac{\$1000}{student}$)	[0.030]	[0.000]	[-0.010]	[-0.066]	[458.93]	[994.21]	[2653.04]	[2035.54]
	% Effect Size	{8.435}	{0.000}	{-1.846}	{-12.140}	{10.38}	{10.95}	{15.35}	{8.72}
(10)	Control for SA Level	-0.01	-0.00	0.09	0.04	101.17	172.11	5267.57	11518.49
	Shift during College	(0.04)	(0.04)	(0.06)	(0.11)	(747.86)	(1637.08)	(3952.28)	(7652.70)
	Effect Size ($\frac{\$1000}{student}$)	[-0.005]	[-0.000]	[0.043]	[0.020]	[51.20]	[83.96]	[2527.62]	[5808.62]
	% Effect Size	{-1.406}	{-0.000}	{8.305}	{3.735}	{1.16}	{0.92}	{14.62}	{24.88}
(11)	Control for Initial Census	-0.05**	-0.02	-0.02	-0.11*	-745.14*	-1225.55	2764.51	2544.06
	Block Observables	(0.02)	(0.03)	(0.03)	(0.06)	(438.02)	(964.16)	(2154.08)	(4350.23)
	Effect Size ($\frac{\$1000}{student}$)	[-0.025]	[-0.010]	[-0.010]	[-0.055]	[-377.10]	[-597.83]	[1326.54]	[1282.93]
	% Effect Size	{-7.029}	{-2.168}	{-1.846}	{-10.273}	{-8.53}	{-6.58}	{7.68}	{5.50}
(12)	Fixed Baseline Instrument	-0.02	-0.03	-0.03	-0.11*	-886.92**	-712.09	3358.99***	4125.80***
		(0.02)	(0.02)	(0.03)	(0.05)	(346.11)	(753.32)	(346.11)	(753.32)
	Effect Size ($\frac{\$1000}{student}$)	[-0.010]	[-0.015]	[-0.014]	[-0.055]	[-448.85]	[-347.36]	[1611.80]	[2080.58]
	% Effect Size	{-2.812}	{-3.252}	{-2.768}	{-10.273}	{-10.15}	{-3.82}	{9.33}	{8.91}

(13)	100% Degree Time in College	-0.05**	-0.06**	-0.02	-0.13**	-738.10*	-1047.49	2671.80	4387.00
		(0.02)	(0.02)	(0.03)	(0.05)	(433.71)	(973.59)	(2230.33)	(4095.31)
	Effect Size ($\frac{\$1000}{student}$)	[-0.025]	[-0.029]	[-0.010]	[-0.066]	[-373.53]	[-510.97]	[1282.05]	[2212.30]
	% Effect Size	{-7.029}	{-6.504}	{-1.846}	{-12.140}	{-8.44}	{-5.63}	{7.42}	{9.48}
(14)	Private Institution	0.09	0.13	0.02	0.03	60.54	2788.49	8346.72	-14544.63
	Falsification	(0.10)	(0.09)	(0.11)	(0.24)	(2182.84)	(3976.12)	(8481.31)	(21479.56)
(15)	Transportation Funding	-0.04**	-0.01	-0.02	-0.01	-561.34	-851.28	-2034.45	1671.10
	Falsification	(0.02)	(0.02)	(0.03)	(0.05)	(363.08)	(896.30)	(2191.40)	(3770.84)

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text and the 3-year rolling baseline shift-share instrument. All regressions include cohort-by-age and institution fixed effects as well as controls for baseline state appropriations share, state college-age population, and a dummy for 1980 and later birth cohorts. The first row controls for base share interacted with county unemployment in the expected year of college exit assuming enrollment of 150% degree time. The second robustness check includes a control for baseline share interacted with the home price index in the county at the time of expected college exit assuming enrollment of 150% degree time. In row (12) we use a fixed baseline state appropriations share from 1986 to construct the instrument. The estimates that control for initial Census Block observables include the controls listed in Table 3. In row (14), we use only private institutions (including for-profits) and assign them the average baseline share of institutions in the state and year of the same Barron's selectivity rank. In row (15), we use transportation rather than higher education funding to construct the instrument. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

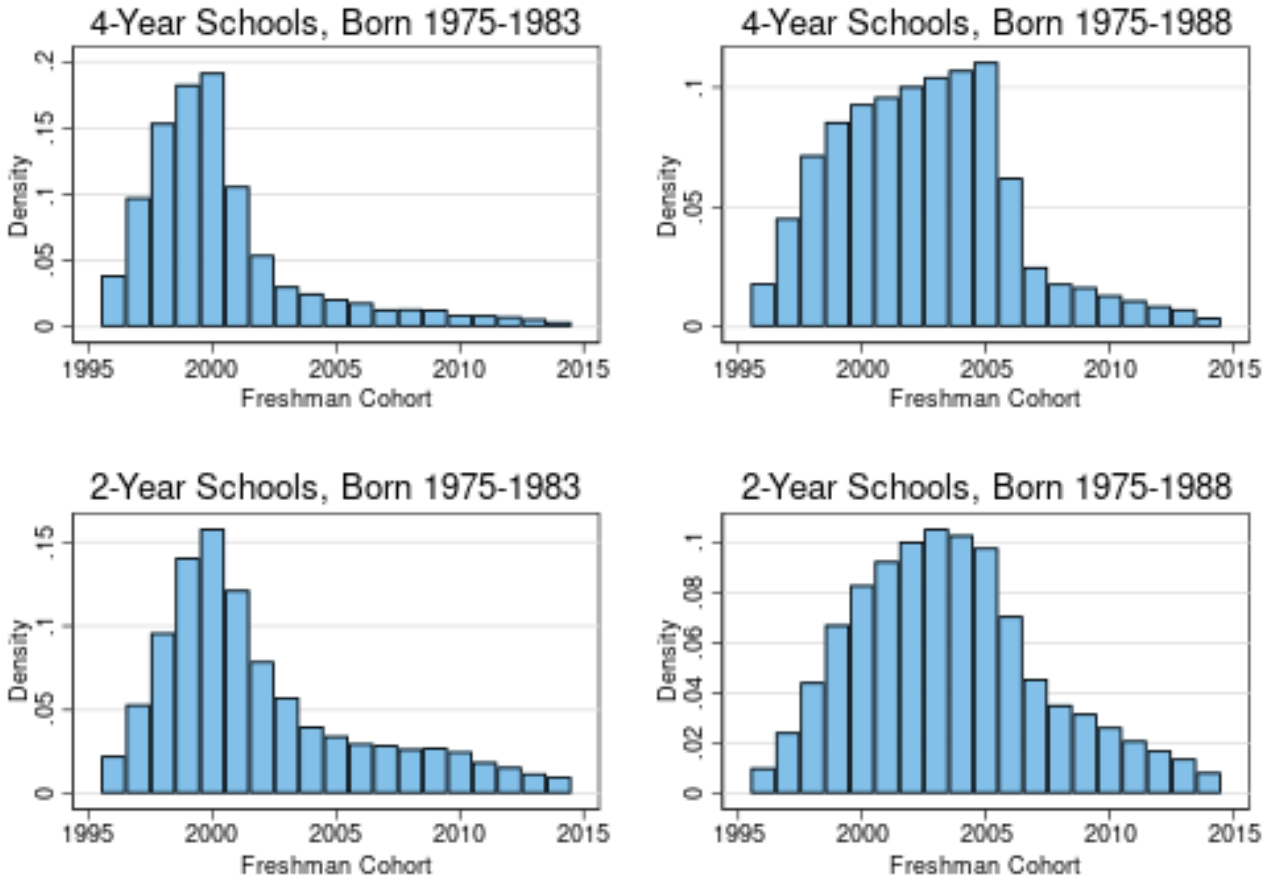
Online Appendix: Not for Publication

Figure A-1: The Distribution of Cohorts by Institution Level and Birth Year



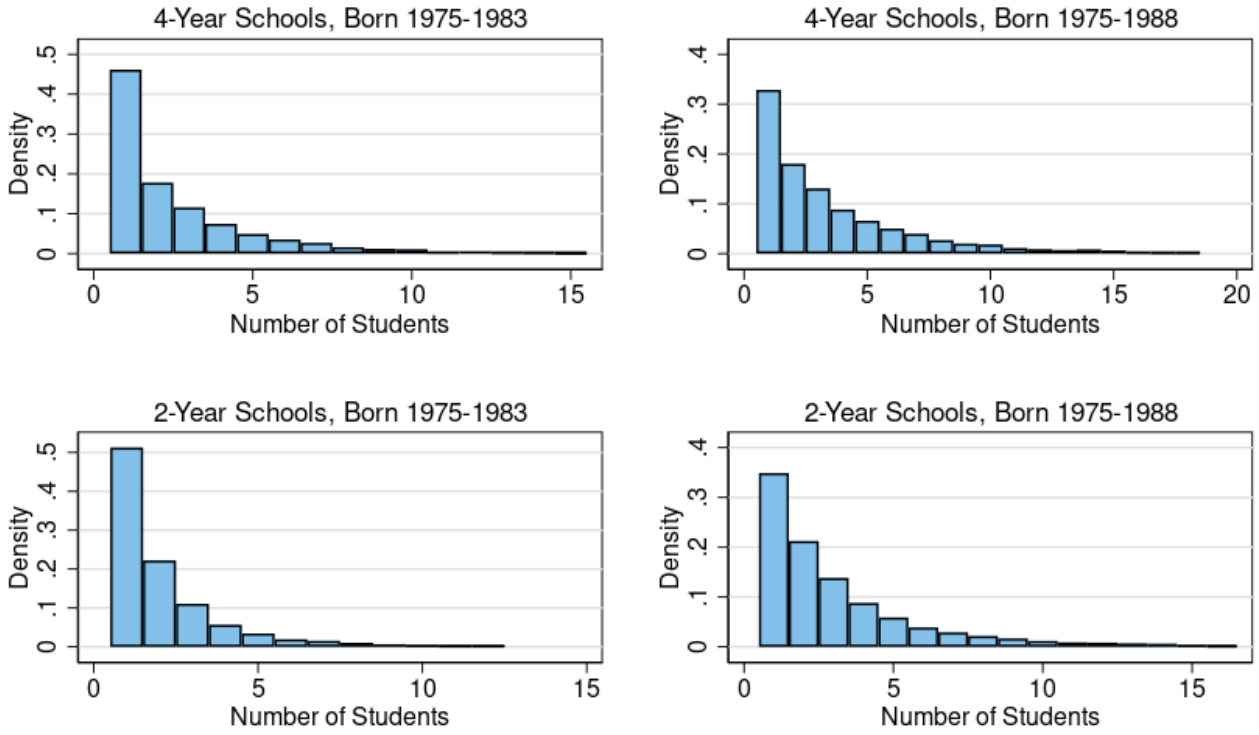
Source: Linked CCP-NSC data as described in the text.

Figure A-2: The Distribution of Cohorts by Institution Level and Freshman Year



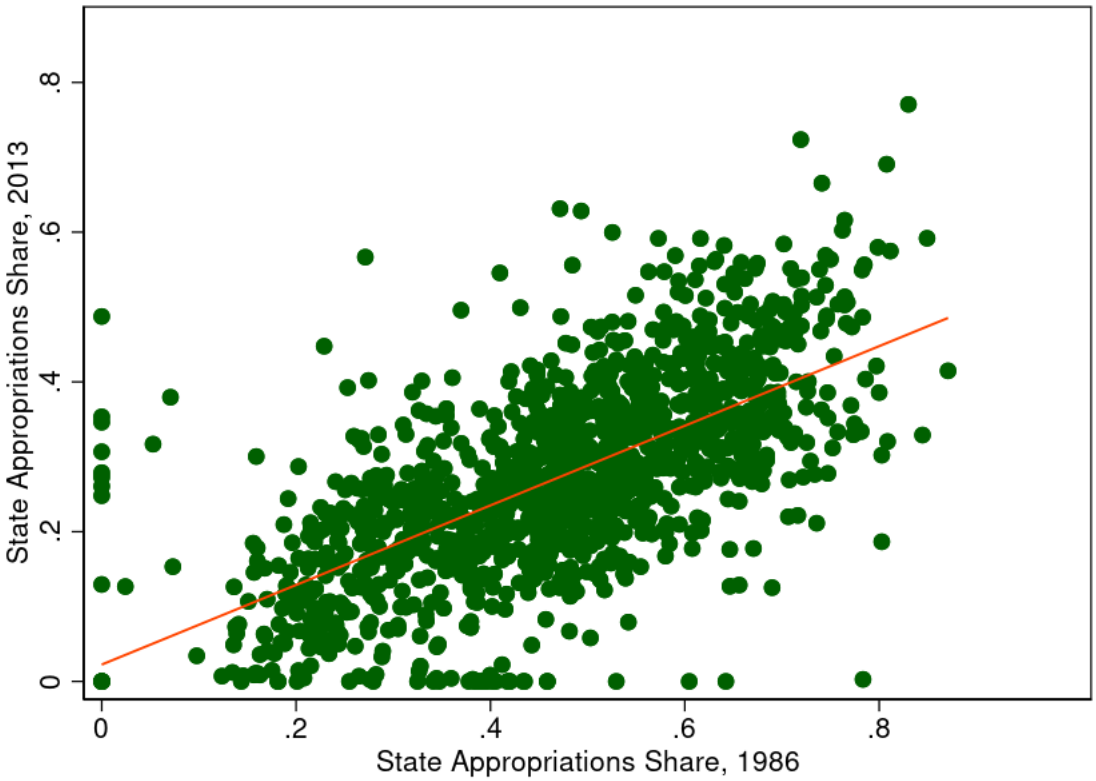
Source: Linked CCP-NSC data as described in the text. Years refer to the fall of the academic year.

Figure A-3: The Distribution of Institution-Cohorts Cell Sizes by Institution Level and Sample



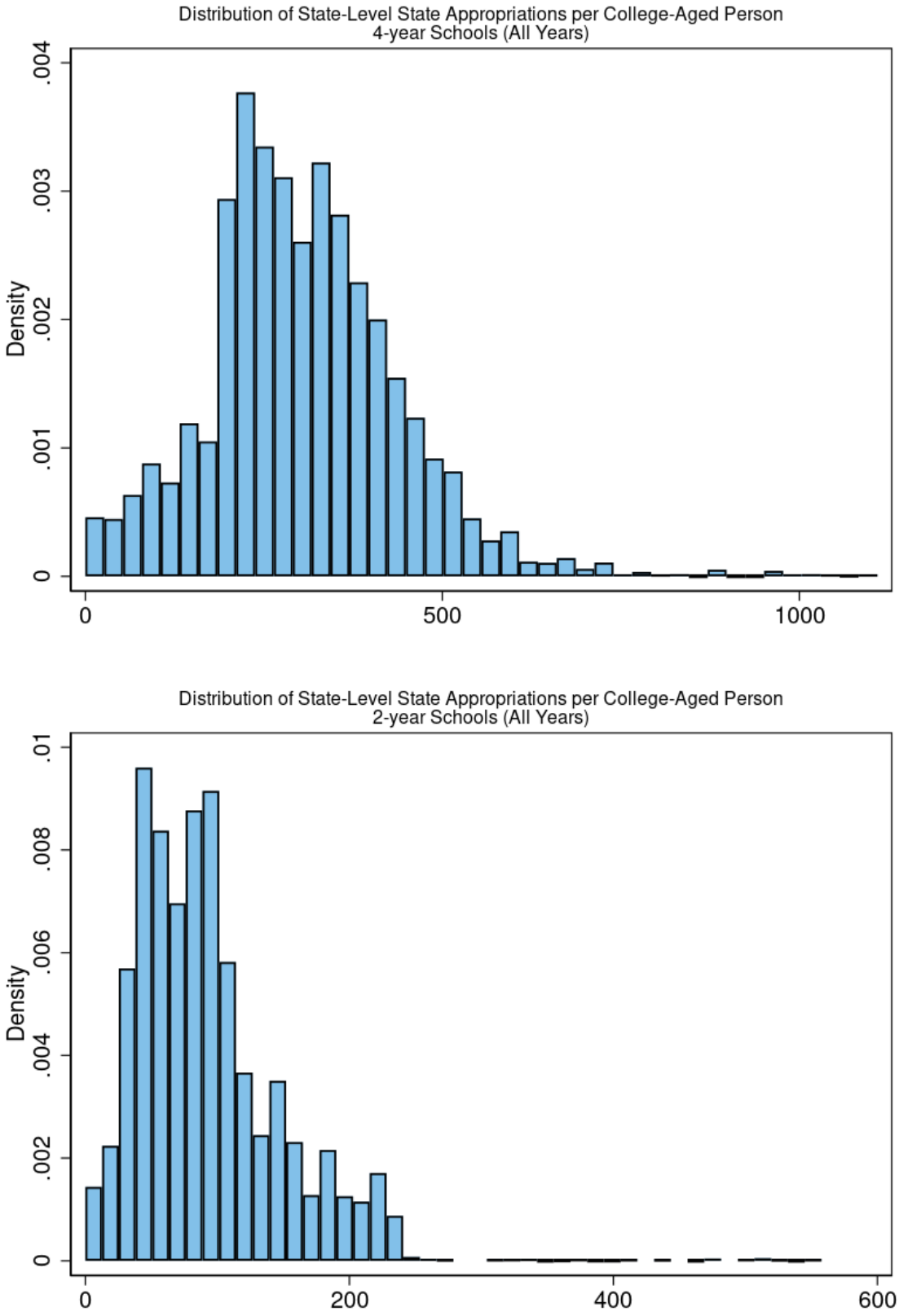
Source: Linked CCP-NSC data as described in the text.

Figure A-4: Within-Institution Change in Share, 1986 & 2013



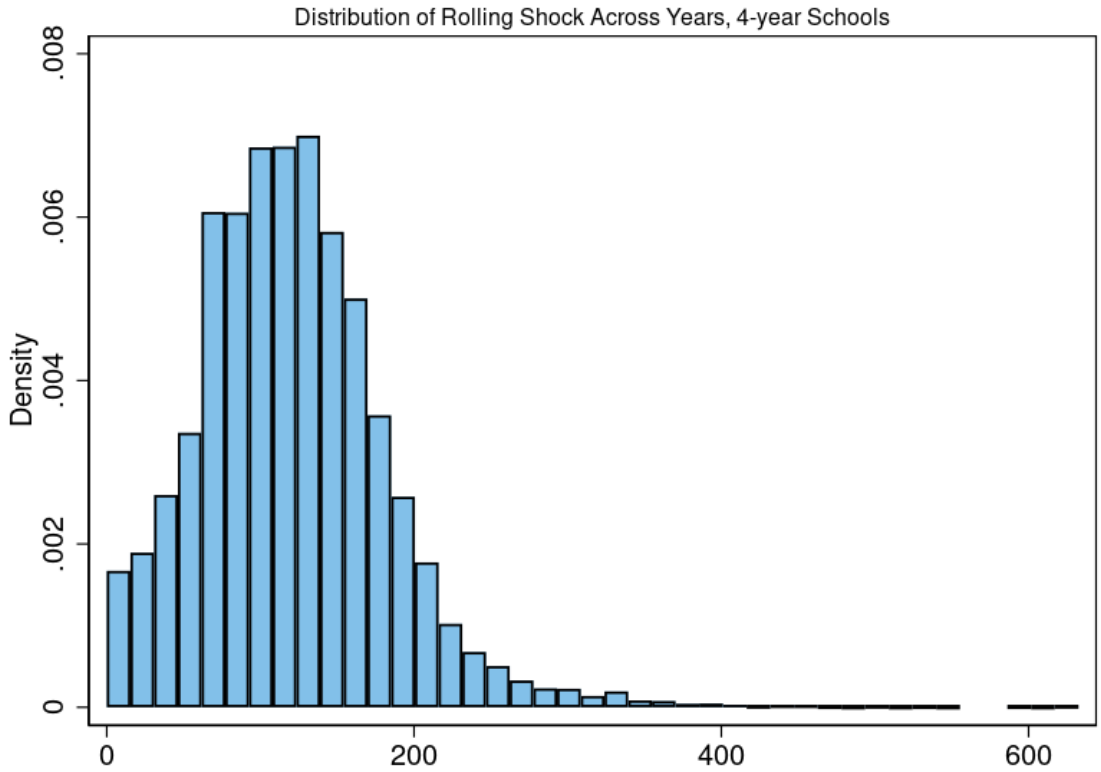
Source: IPEDS, 1986 and 2013. Each point is a separate postsecondary institution.

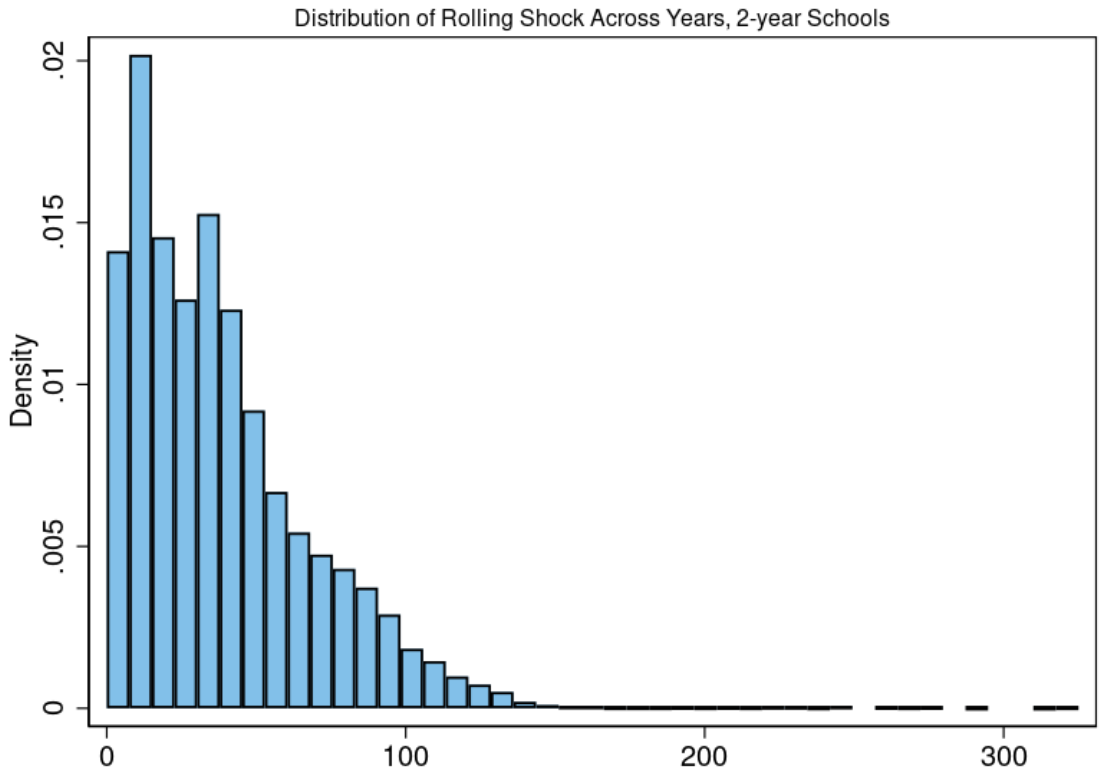
Figure A-5: State-Level Appropriations per College-Age Resident (All Years)



Each panel shows the distribution state-level appropriations per college-age resident. The top panel shows the four-year distribution, and the bottom panel shows two-year schools.

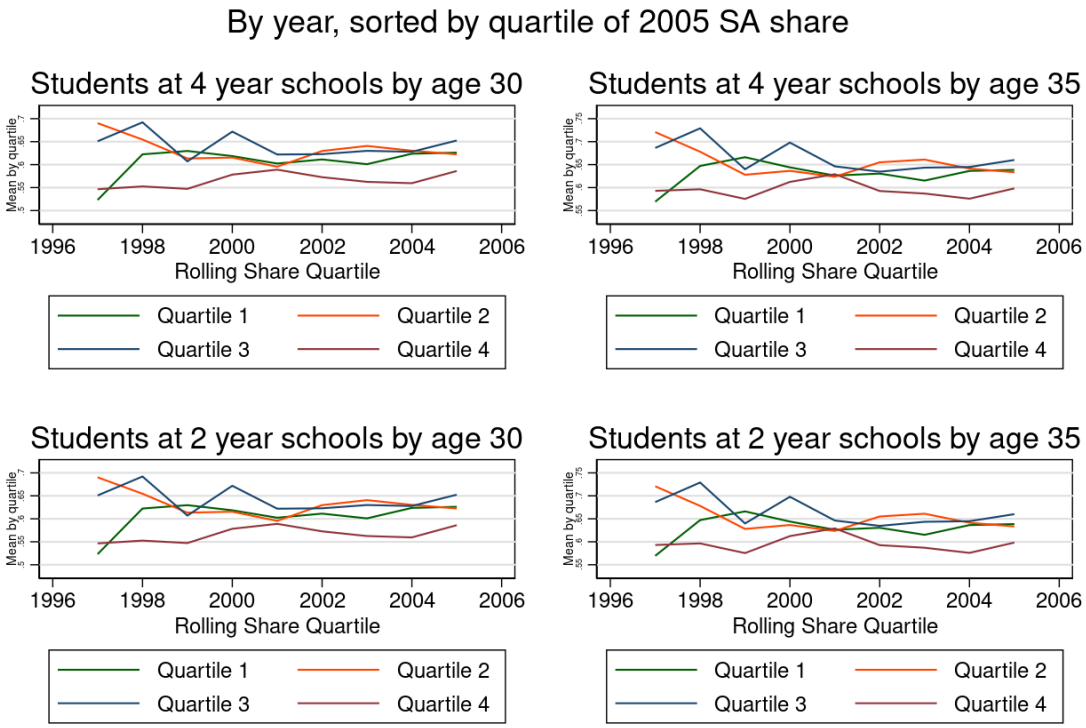
Figure A-6: Distribution of the Shift-Share State Appropriations Instrument, by Year





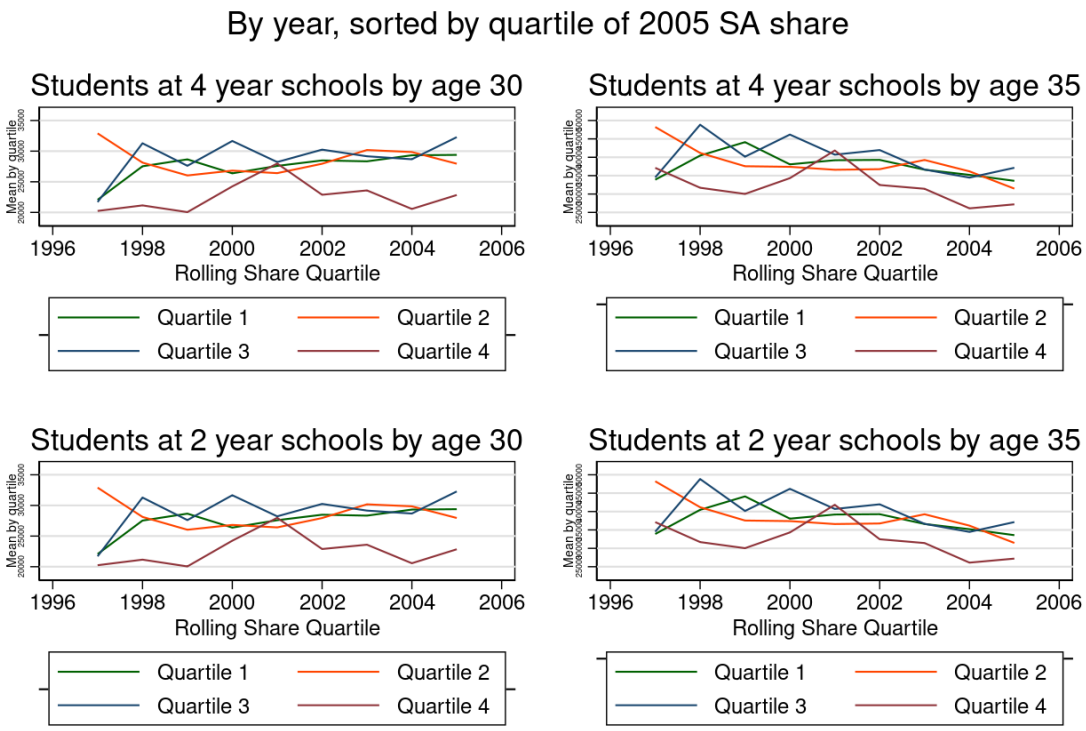
Each panel shows the distribution of the 3-year rolling baseline shift-share instrument. The top panel shows the four-year distribution, and the bottom panel shows two-year schools.

Figure A-7: Pre-Trends Relative to 2005 – Ever Originate Student Loan



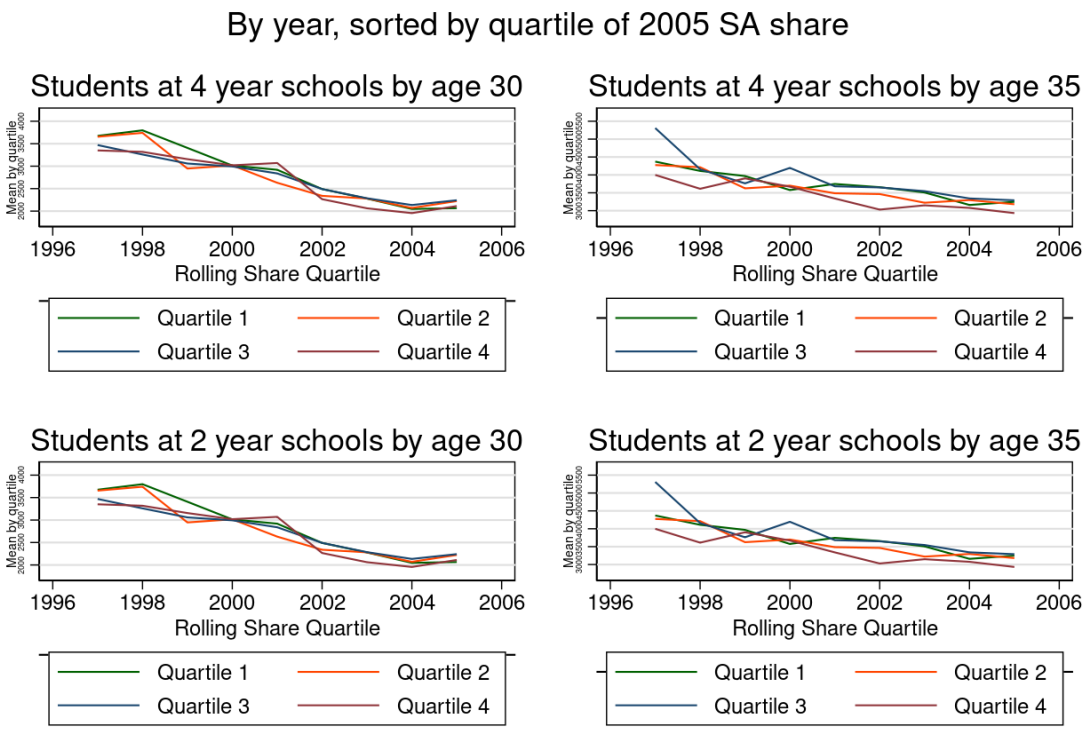
The figure shows pre-2005 trends in the variable by quartile of the 2005 baseline share.

Figure A-8: Pre-Trends Relative to 2005 – Student Loan Amount



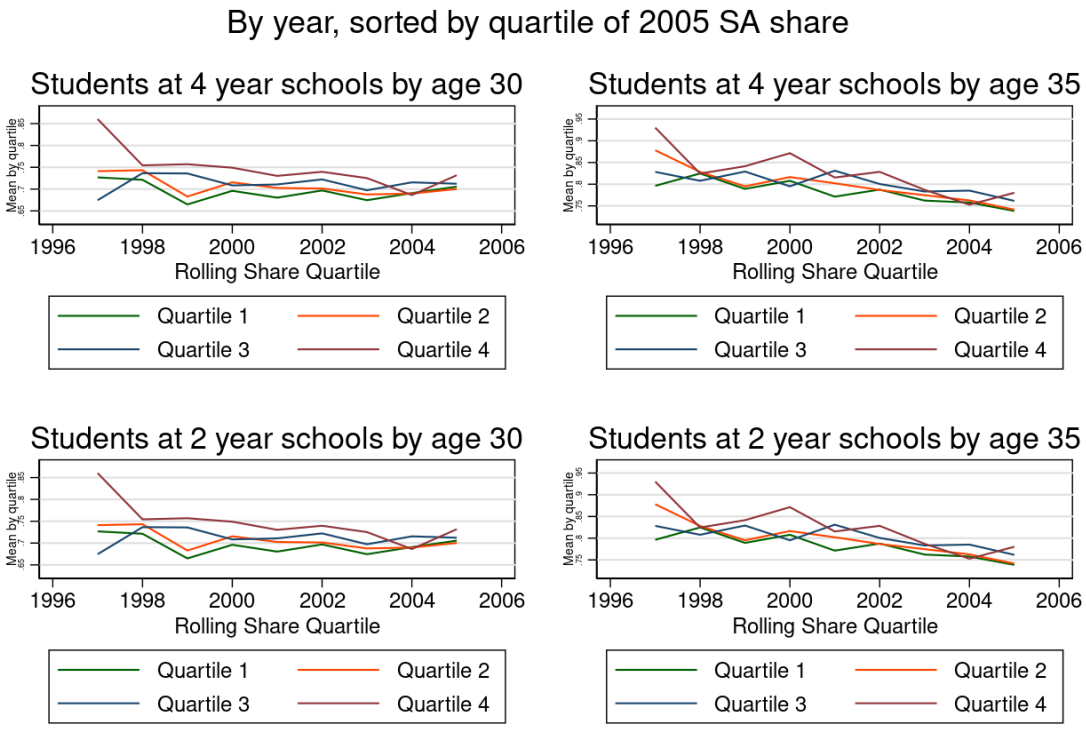
The figure shows pre-2005 trends in the variable by quartile of the 2005 baseline share.

Figure A-9: Pre-Trends Relative to 2005 – Credit Card Balance



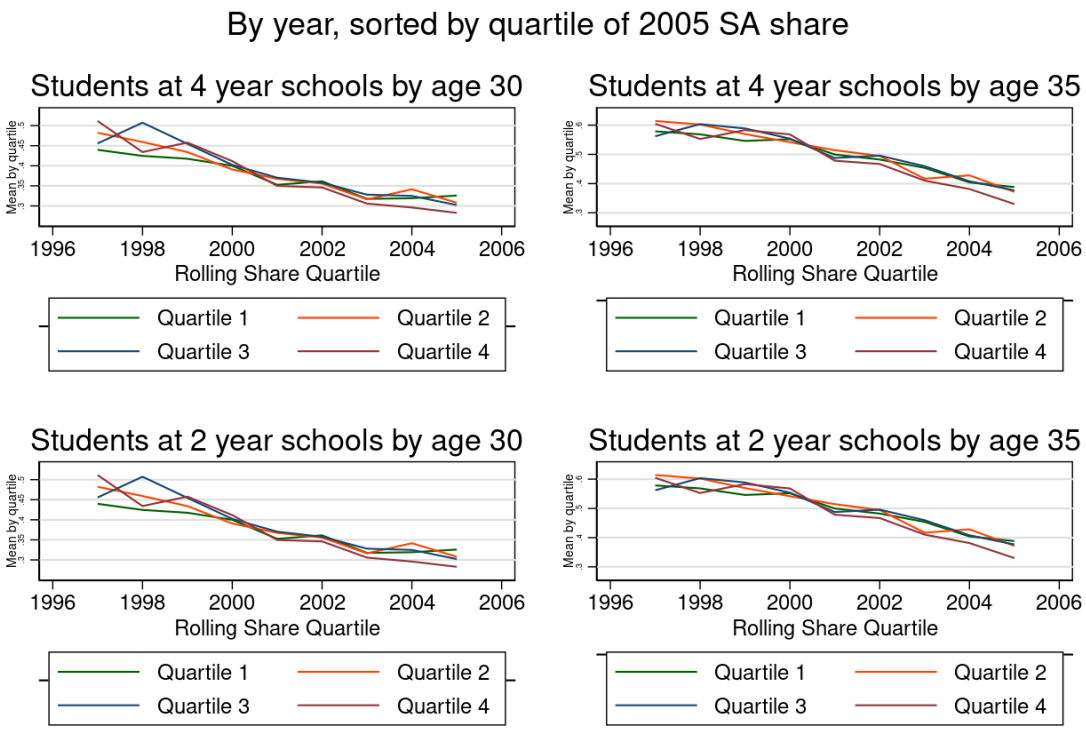
The figure shows pre-2005 trends in the variable by quartile of the 2005 baseline share.

Figure A-10: Pre-Trends Relative to 2005 – Own Car



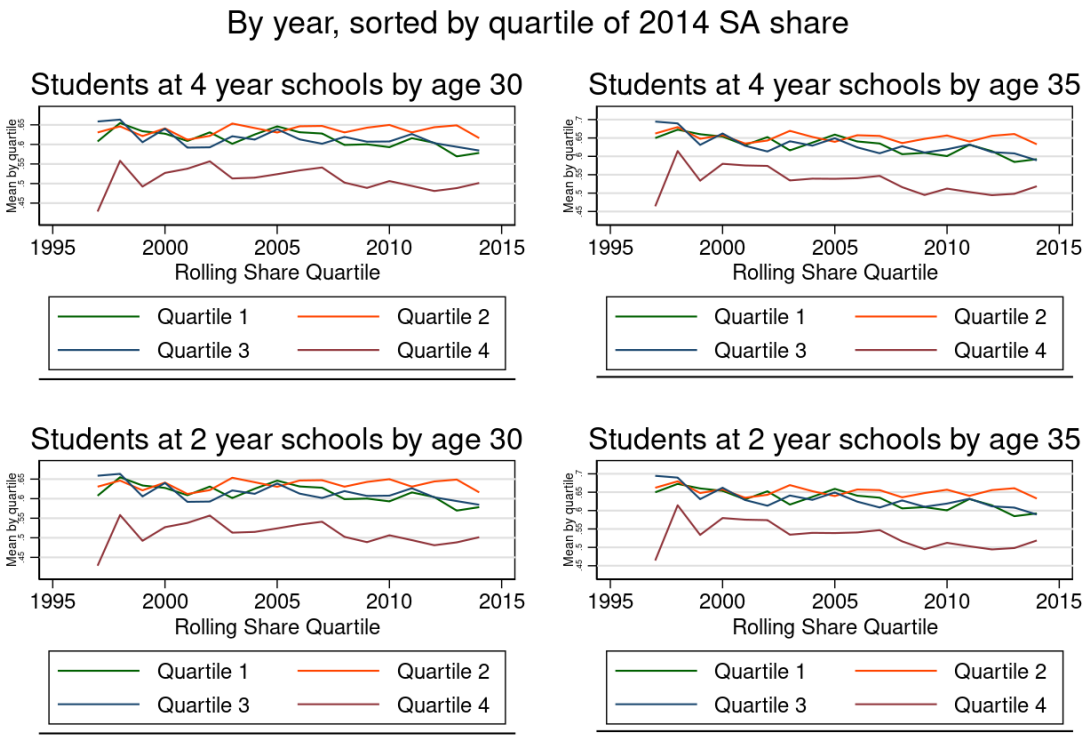
The figure shows pre-2005 trends in the variable by quartile of the 2005 baseline share.

Figure A-11: Pre-Trends Relative to 2005 – Own Home



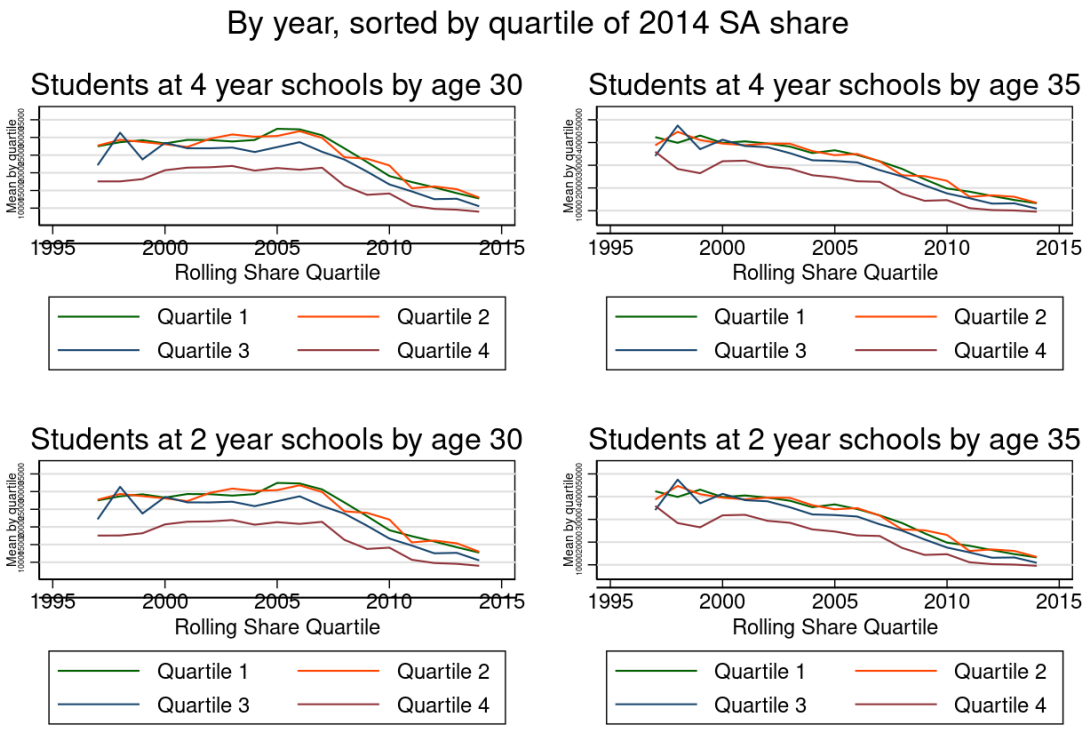
The figure shows pre-2005 trends in the variable by quartile of the 2005 baseline share.

Figure A-12: Pre-Trends Relative to 2014 – Ever Originate Student Loan



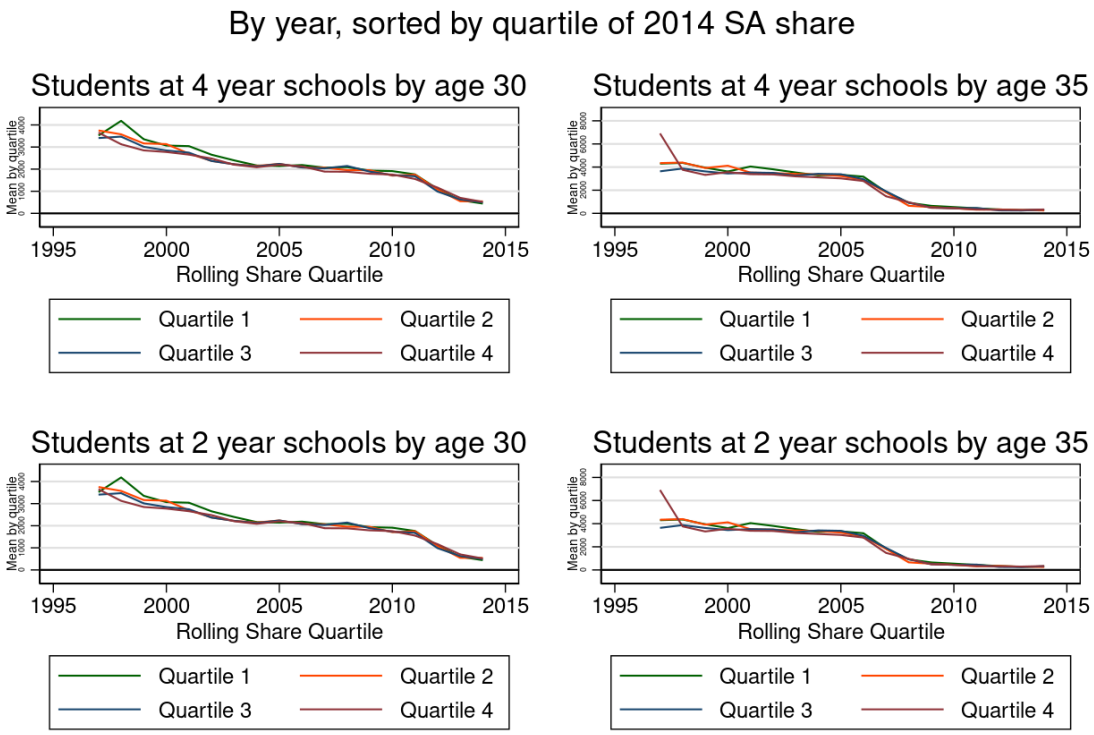
The figure shows pre-2014 trends in the variable by quartile of the 2014 baseline share.

Figure A-13: Pre-Trends Relative to 2014 – Student Loan Amount



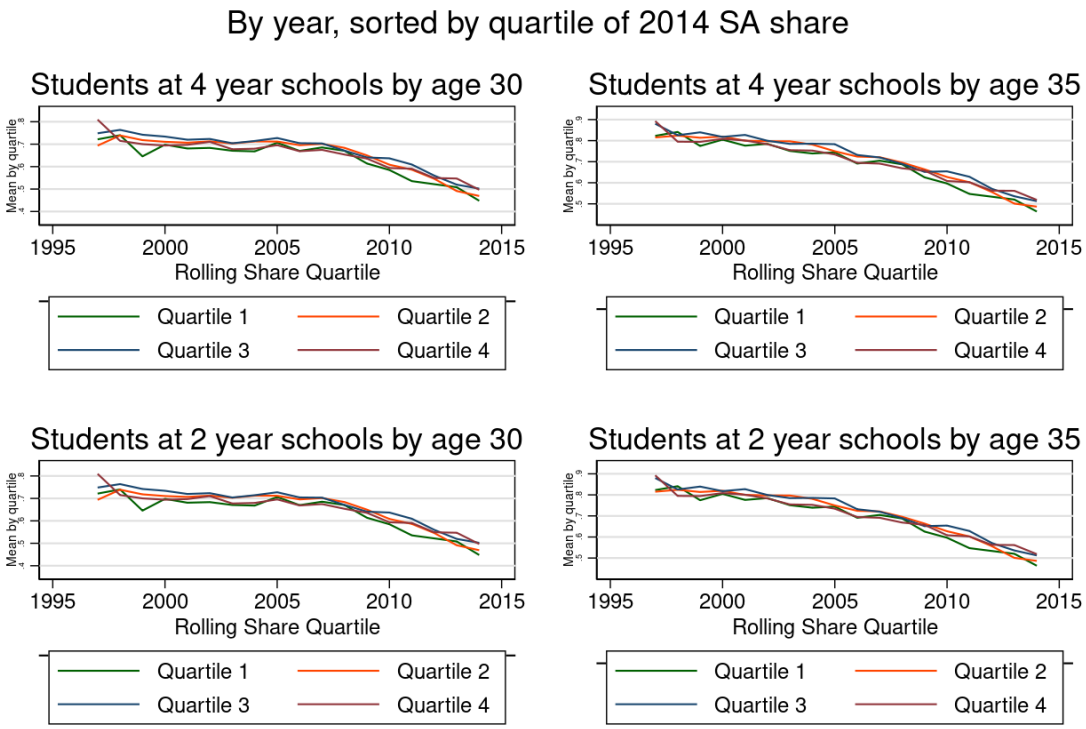
The figure shows pre-2014 trends in the variable by quartile of the 2014 baseline share.

Figure A-14: Pre-Trends Relative to 2014 – Credit Card Balance



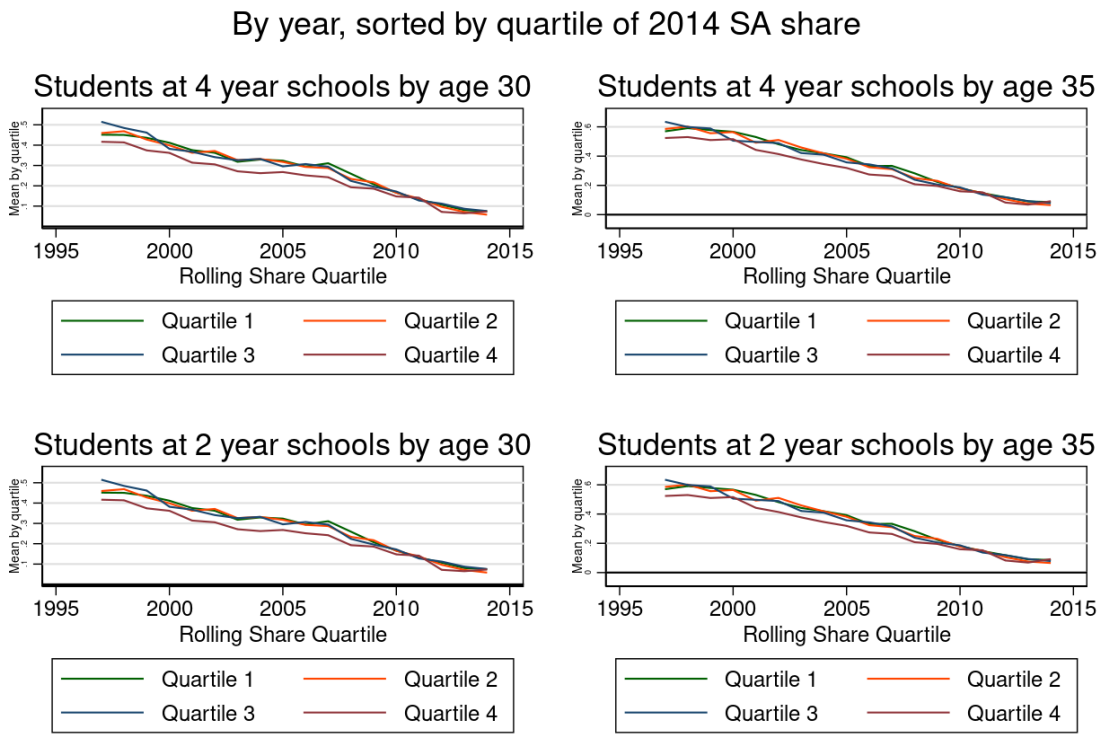
The figure shows pre-2014 trends in the variable by quartile of the 2014 baseline share.

Figure A-15: Pre-Trends Relative to 2014 – Own Car



The figure shows pre-2014 trends in the variable by quartile of the 2014 baseline share.

Figure A-16: Pre-Trends Relative to 2014 – Own Home



The figure shows pre-2014 trends in the variable by quartile of the 2014 baseline share.

Table A-1: Cohorts Studied

Panel A: FRBNY/Equifax Consumer Credit Panel			
Age at Outcome	**Birth cohorts**	**Freshman Age**	**Freshman Year**
22	1975-1996	19 [18]	2006 [2006]
25	1975-1993	19 [18]	2005 [2005]
25-30	1975-1988	20 [18]	2003 [2003]
30-35	1975-1983	21 [19]	2001 [2000]
Panel B: National Student Clearinghouse			
Age at Outcome	**Birth cohorts**	**Freshman Age**	**Freshman Year**
25	1975-1989	19 [18]	2003 [2003]
30	1975-1984	20 [18]	2001 [2000]
By ever	1975-1996	19 [18]	2004 [2004]

Source: Merged FRBNY/Equifax Consumer Credit Panel and National Student Clearinghouse data as described in the text. Mean value in cells; median value in brackets.

Table A-2: Summary Statistics of National Student Clearinghouse Variables

	4-year Schools		2-year Schools	
Variable	**mean**	**std. dev.**	**mean**	**std. dev.**
Student Loans:				
Transfer to 4-year – age 30			0.18	0.38
Transfer to 4-year – ever			0.23	0.42
Transfer to More Selective School – age 30	0.04	0.20	0.11	0.31
Transfer to More Selective School – ever	0.06	0.23	0.15	0.35
Years of Postsecondary Education – age 25	4.12	2.39	2.72	2.51
Years of Postsecondary Education – age 30	5.08	2.97	3.41	3.05
Years of Postsecondary Education – ever	4.80	2.84	3.72	2.99
Earn BA+ – age 25	0.56	0.50	0.17	0.37
Earn BA+ – age 30	0.65	0.48	0.20	0.40
Earn BA+ – ever	0.51	0.50	0.19	0.40
Highest Degree = BA – age 25	0.47	0.50	0.13	0.34
Highest Degree = BA – age 30	0.43	0.49	0.14	0.35
Highest Degree = BA – ever	0.37	0.48	0.14	0.35
Earn Graduate Degree – age 25	0.09	0.29	0.03	0.13
Earn Graduate Degree – age 30	0.22	0.42	0.06	0.23
Earn Graduate Degree – ever	0.14	0.35	0.05	0.22
Earn STEM Degree – age 25	0.20	0.40	0.09	0.28
Earn STEM Degree – age 30	0.24	0.43	0.10	0.31
Earn STEM Degree – ever	0.19	0.39	0.10	0.30
Earn Business Degree – age 25	0.16	0.37	0.06	0.25
Earn Business Degree – age 30	0.19	0.39	0.08	0.28
Earn Business Degree – ever	0.14	0.34	0.07	0.25
Earn Liberal Arts Degree – age 25	0.20	0.40	0.10	0.30
Earn Liberal Arts Degree – age 30	0.23	0.42	0.11	0.32
Earn Liberal Arts Degree – ever	0.17	0.38	0.11	0.31
Earn Vocational Degree – age 25	0.17	0.38	0.10	0.31
Earn Vocational Degree – age 30	0.20	0.40	0.14	0.34
Earn Vocational Degree – ever	0.15	0.36	0.12	0.32

Source: Merged Consumer Credit Panel, National Student Clearinghouse, and IPEDS data as described in the text. The 1975-1989 birth year cohorts constitute the sample for age 25 outcomes, the 1975-1984 birth year cohorts constitute the sample for age 30 outcomes, and the 1975-1996 cohorts constitute the sample for the "ever" outcomes. All tabulations are done at the individual-level.

Table A-3: Selection into College as a Function of the Once-Lagged Instrument Among First-Year Students

	4-Year		2-Year	
Dependent Variable	Estimate	Effect Size ($\frac{\$1000}{Student}$)	Estimate	Effect Size ($\frac{\$1000}{Student}$)
HH Income (Census Block)	-1569.37*	-589.55	-2046.96*	-982.23
	(888.53)		(1064.58)	
HH Income (Zip - Tax Data)	-409.45	-153.81	-2282.90**	-1095.44
	(825.70)		(805.33)	
Above Median HH Income (Zip)	-0.02**	-0.008	0.01	0.005
	(0.01)		(0.02)	
Percent White	-0.01	-0.004	-0.01	-0.005
	(0.01)		(0.01)	
Percent Black	0.00	0.000	-0.02**	-0.010
	(0.00)		(0.00)	
Percent Married	-0.00	0.000	0.00	0.000
	(0.00)		(0.01)	
Percent HH with Kids<18	-0.00	0.000	-0.01	0.000
	(0.00)		(0.01)	
Percent BA+	-0.01	-0.004	-0.02**	-0.010
	(0.00)		(0.01)	
Percent Some College	0.00	0.000	-0.00	0.000
	(0.00)		(0.00)	
Percent No College	0.00	0.000	0.02**	0.010
	(0.00)		(0.01)	
Median Gross Rent	-16.45	-6.18	-40.97***	-19.66
	(13.82)		(14.98)	
Median Home Value	-8289.05*	-3113.84	-24000***	-11516.31
	(4813.48)		(6892.72)	
LFP Rate	-0.01*	-0.004	-0.01**	-0.005
	(0.00)		(0.00)	
Employment Rate	-0.00	0.000	0.00	0.000
	(0.00)		(0.00)	
Total Population	-3.09	-1.16	-7.83	-3.76
	(45.58)		(83.72)	
Total Population ≥25	-22.93	-8.61	-14.76	-7.08
	(26.83)		(45.76)	

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text and the 3-year rolling baseline shift-share instrument. All regressions include freshman cohort-by-age and institution fixed effects as well as controls for baseline state appropriations share, state college-age population, and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. Each cell in the "Estimate" columns is a separate regression. The instrument is in units of $100 per college-age resident. The "Effect Size" shows the reduced form estimate divided by the first stage in Table 4. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table A-4: First Stage Estimates For Age 22 and 25 NSC-CCP Samples

	Panel A: 4-Year			
Birth years	1975-1988		1975-1983	
Age	22		25	
	(1)	(2)	(3)	(4)
$\widetilde{SA}$	24.49***	26.12***	23.82***	26.39***
	(1.90)	(0.91)	(2.05)	(0.92)
Institution FE	N	Y	N	Y
Observations	26683	26668	21330	21311

	Panel B: 2-Year			
Birth years	1975-1988		1975-1983	
Age	22		25	
	(1)	(2)	(3)	(4)
$\widetilde{SA}$	16.90***	19.76***	17.55***	20.50***
	(0.91)	(0.98)	(1.00)	(1.00)
Institution FE	N	Y	N	Y
Observations	40633	40614	30802	30777

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text. All regressions include cohort-by-age and institution fixed effects as well as controls for state college-age population and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. State appropriations is expressed in $ per student and the state appropriations instrument is expressed in $ per college-age resident in the state. The 1975-1988 birth year cohorts constitute the sample for age 22 NSC-CCP outcomes and the 1975-1983 birth year cohorts constitute the sample for age 25 NSC-CCP outcomes. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table A-5: First Stage Estimates For NSC Sample

	Panel A: 4-Year		
Birth years	1975-1989	1975-1984	1975-2000
Age	25	30	Ever
	(1)	(2)	(3)
$\widetilde{SA}$	26.39***	25.62***	25.93***
	(0.92)	(1.04)	(0.96)
Observations	21311	11085	26882
	Panel B: 4-Year, Degree Attainment Outcomes		
Birth years	1975-1989	1975-1984	1975-2000
Age	25	30	Ever
	(1)	(2)	(3)
$\widetilde{SA}$	26.39***	25.71***	25.97***
	(0.92)	(1.04)	(0.96)
Observations	21311	10966	26575
	Panel C: 2-Year		
Birth years	1975-1989	1975-1984	1975-2000
Age	25	30	Ever
	(1)	(2)	(3)
$\widetilde{SA}$	20.50***	19.63***	19.72***
	(1.00)	(1.11)	(0.99)
Observations	30777	15242	46875
	Panel D: 2-Year, Educational Attainment Outcomes		
Birth years	1975-1989	1975-1984	1975-2000
Age	25	30	Ever
	(1)	(2)	(3)
$\widetilde{SA}$	20.50***	19.66***	19.74***
	(1.00)	(1.12)	(0.99)
Observations	30777	14994	46107

Authors' estimation of equation (4) using the NSC data as described in the text. All regressions include cohort-by-age and institution fixed effects as well as controls for state college-age population and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. State appropriations is expressed in $ per student and the state appropriations instrument is expressed in $ per college-age resident in the state. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table A-6: First Stage Results: Birth Cohorts 1975-1988 and 1975-1983 Using Fixed Baseline Instrument

Dep. Var.: State Appropriations per Student				
	Panel A: 4-Year			
Birth years	1975-1986		1975-1981	
Age	25-30		30-35	
	(1)	(2)	(3)	(4)
$\widetilde{SA}$	19.53***	23.21***	15.31***	21.48***
	(1.79)	(0.98)	(1.97)	(1.35)
Institution FE	N	Y	N	Y
Observations	18948	18935	9001	8979
	Panel B: 2-Year			
Birth years	1975-1986		1975-1981	
	(1)	(2)	(3)	(4)
$\widetilde{SA}$	13.70***	17.47***	12.12***	18.02***
	(0.96)	(0.92)	(1.24)	(1.04)
Institution FE	N	Y	N	Y
Observations	25753	25739	11629	11592

Authors' estimation of equation (4) using the linked CCP-NSC data described in the text. All regressions include cohort-by-age fixed effects as well as controls for state college-age population and a dummy for 1980 and later birth cohorts. Cohorts are defined as the year in which each individual first entered college, and college sectors (4-year/2-year) refer to the sector in which each individual was first enrolled in college. State appropriations is expressed in $ per student and the state appropriations instrument is expressed in $ per college-age resident in the state. The 1975-1988 birth year cohorts constitute the sample for age 25-30 outcomes and the 1975-1983 birth year cohorts constitute the sample for age 30-35 outcomes. The state appropriations instrument ($\widetilde{SA}$) uses the 1989 fixed baseline state appropriations share. Standard errors clustered at the state-by-cohort level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table A-7: Summary Statistics of Variables Used to Examine Mechanisms

	4-Year		2-Year	
Variable	Mean	SD	Mean	SD
In-State Tuition/Fees	5,425.75	2,262.78	2,455.82	1,584.89
Out-of-State Tuition/Fees	14,017.07	4,799.89	5,702.79	2,440.31
Net Tuition Revenue	5,369.09	2,902.14	1,362.14	866.99
Capital Grants & Gifts	389.34	639.93	67.14	245.50
Capital Appropriations	592.62	741.36	275.84	471.11
Local Appropriations	39.79	359.81	1,115.88	1,294.66
Federal Appropriations	59.48	751.91	11.89	159.68
Total Appropriations	6,404.42	3,900.89	3,260.79	1,357.67
Instructional Expenditures	7,431.01	5,052.00	2,859.97	1,146.78
Research Expenditures	3,279.22	4,806.11	2.62	13.61
Academic Support Expend.	1,948.38	1,545.29	542.67	309.97
Student Services	1,162.00	587.25	674.64	349.91
Institutional Support	1,875.40	1,084.83	957.53	484.93
Faculty/Student Ratio (FTE)	0.06	0.02	0.05	0.02
Fraction International	0.019	0.026	0.007	0.016
Fraction Out-of-State	0.149	0.134	0.052	0.078

Source: IPEDS data from years that overlap with enrollment of the CCP-NSC sample.