Crowded Out? The Implications of Nonresident Enrollment Growth on Enrollment for Resident Students at Public Universities

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Abstract: As state funding for public higher education stagnated public universities have increased nonresident enrollment. Public universities argue that nonresident tuition revenue is necessary to finance access for resident students, while state policymakers are concerned that nonresident enrollment reduces opportunities for residents. This study investigated whether nonresident enrollment growth crowded-out resident enrollment at public research universities using an instrumental variable estimation strategy. For the sample of all public research universities, no relationship was found between nonresident enrollment and resident enrollment. For prestigious public research universities, nonresident enrollment growth had a negative effect on resident enrollment. The findings suggest that policymakers should not be concerned that nonresident enrollment is crowding out residents, except perhaps at prestigious public universities.

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Crowded Out? The Implications of Nonresident Enrollment Growth on Access for Resident Students at Public Universities

Public research universities have historically been engines of economic growth, educating professional and civic leaders and helping low-income state residents to obtain a high quality education (Gerald & Haycock, 2006). As state funding for public higher education has stagnated, policy advocates have become concerned that state flagship universities are prioritizing access for nonresident students over access for residents (Burd, 2015b). Figure 1 shows national trends in average resident and nonresident freshman fall enrollment between 1992-93 and 2014-15. Resident enrollment increased at an annualized rate of 2.5% in between 1992-93 and 2002-03, began to slow in the early 2000s, and has remained flat since 2008-09. In contrast, in between 2004-05 and 2014-15 average nonresident enrollment increased by an annualized rate of 4.8%.

The state of California provides a sharp example of policy debates about nonresident enrollment growth at flagship state universities and access for state residents. Resident freshman enrollment in the University of California System shrank from 33,530 in 2006-07 to 32,630 in 2015-16, a 2.7% decrease (University of California, 2016). By contrast, nonresident freshman enrollment grew from 1,788 to 8,926 over the same time period, nearly a 400% increase. While system-wide state appropriations declined by 30%, from $2.91 billion in 2007-08 to $2.03 billion in 2011-12 (Author calculations based on IPEDS data, 2014 CPI), nonresident enrollment growth helped fuel dramatic gains in net tuition revenue, from $1.78 billion in 2007-8 to $3.4 billion in 2013-14 (a 91% increase). The University of California System argued that nonresident enrollment growth is necessary to maintain quality and access for California residents:

> Those funds subsidize the education of California students, especially as state funding has declined. More importantly, the increase in nonresident students has helped the University maintain its commitment to the California Master Plan by ensuring that every eligible California student receives an offer of admission. (Peacock, 2015, p. 1)

In June 2015, concern about access for Californians prompted the California Legislature to offer $25 million in additional state funding if the University of California System increased resident enrollment by 5,000 between 2014-15 and 2016-17 (Assembly Bill 93, 2015). In November 2015, the University of California approved a plan to increase resident enrollment by 10,000 within three years (University of California, 2015). However, in January 2016 after learning that resident freshman enrollment declined in fall 2015 while nonresident enrollment continued to climb, Assembly Members McCarty and Medina proposed capping nonresident enrollment at 15.5% of undergraduate enrollment (Assembly Bill 1711, 2016). Assembly Member Medina said, "Unfortunately, despite a strong directive and additional funding . . . It is clear that additional statutory guidance is necessary to ensure all qualified California students have a fair chance at a world class University of California education" (as cited in Koseff, 2016, p. Para 3).

Several other states have recently enacted policy changes targeted at nonresident enrollment. For example, in June 2015, the Iowa Board of Regents approved a plan whereby 60% of state appropriations would depend on resident enrollment count, thereby creating an incentive for Iowa public universities to increase resident enrollment (Burd, 2015a). In contrast, in October 2015 the University of Wisconsin Board of Regents voted to remove the cap on nonresident enrollment, citing the declining college-age population and shrinking state funding as rationales (Board of Regents of the University of Wisconsin System, 2015). The resolution requires the University of Wisconsin-Madison to enroll at least 3,600 resident freshman each year and Chancellor Blank proclaimed a "commitment to admit Wisconsin students at the same rate as we have for many years" (Blank, 2015, p. 2).

A question in policy debates between state legislatures and public universities is whether there is an effect of increased nonresident enrollment on access for resident students. Universities argue that tuition revenue from nonresident students is necessary to fund access for residents amidst state budget cuts (e.g., Blank, 2015; Peacock, 2015). However, some policymakers argue that nonresident students may be crowding out resident enrollment (Koesef, 2016). Unfortunately, this debate suffers from a lack of empirical research.

The goal of this paper is to examine the effect of nonresident enrollment growth on resident enrollment. We restrict the sample to research universities as prior research has shown that they are the most responsive in enrolling nonresident students due to changing fiscal conditions (Jaquette & Curs, 2015). We utilized an instrumental variable estimation strategy as nonresident enrollment may be endogenous as enrollment management strategies for resident and nonresident enrollment are likely jointly determined. Specifically, we used state merit aid expenditure in other states as an instrument for nonresident enrollment because prior research shows that state merit aid reduces out-migration (Orsuwan & Heck, 2009; Zhang & Ness, 2010). For the sample of all public research universities, we found no relationship between nonresident enrollment and resident enrollment. However, for prestigious public research universities, nonresident enrollment growth had a negative effect on resident enrollment.

Literature Review

We review empirical research on the determinants of resident enrollment, our dependent variable, focusing first on state policy determinants and second on institutional determinants. We conclude this section by developing logical arguments that explain how changes in nonresident enrollment may affect resident enrollment.

State Policies Affecting Access to Higher Education for Residents

Traditionally, states subsidize access to higher education for residents by providing appropriations to public institutions, which enable institutions to keep tuition levels below the cost of education (Winston, 1999). Extant research has shown that declines in state appropriations cause resident tuition price to rise (Koshal & Koshal, 2000; Rizzo & Ehrenberg, 2004). In turn, a robust empirical literature shows that resident tuition price is negatively associated with enrollment at public universities (Heller, 1997; Hemelt & Marcotte, 2011).

State grant aid programs also subsidize enrollment for state residents. A large empirical literature finds that state merit aid programs positively affect enrollment at in-state public universities (e.g. Cornwell, Mustard, & Sridhar, 2006; Singell, Waddell, & Curs, 2006; Zhang, Hu, & Pu, 2016). Additionally, merit aid programs have been shown to reduce out-migration of residents for higher education (Orsuwan & Heck, 2009; Toutkoushian & Hillman, 2012; Zhang & Ness, 2010).

State need-based aid programs subsidize enrollment for low-income residents. Research generally finds that need-based aid programs increase access for resident students, particularly increasing the probability of attending a public four-year institutions (e.g. Castleman & Long, 2013; Toutkoushian, Hossler, DesJardins, McCall, & Canche, 2015). Less is known about the effect of need-based aid on outmigration.

Institutional Policies Affecting Access to Higher Education for State Residents

Though state policies affect enrollment, this paper focuses on institutional behaviors that affect enrollment for state residents. Hossler and Bean (1990) define enrollment management as “efforts to influence the characteristics and the size of enrolled student bodies” (p. xiv). Enrollment management strategies typically pursue three broad enrollment goals (Cheslock & Kroc, 2012). The access goal refers to access for first-generation students, low-income students, underserved-minority students, and, for public universities, state residents. Second, the academic quality goal refers to enrolling high-achieving students. Third, the revenue goal refers to enrolling students that generate tuition revenue. Given that institutional resources are limited, the pursuit of one enrollment goal may divert resources away from another enrollment goal.

Cuts in state funding creates pressure for public universities to prioritize tuition revenue and concern about rankings creates pressure to increase academic quality. Enrollment management behaviors associated with the pursuits of tuition revenue and academic profile may work against the access goal. For example, the emphasis selective institutions place on standardized test scores has increased over time (Alon & Tienda, 2007) and emphasizing test scores as opposed to grades systematically disadvantages low-income and under-served minority applicants (Alon, 2009; Niu & Tienda, 2010). Additionally, the purpose of institutional aid has changed over time, from a historical focus on need-based aid to increase access for low-income students to a focus on merit-based aid to increase academic profile (Doyle, 2010; Ehrenberg, 2000). More recently, universities are developing aid strategies to maximize net tuition revenue (Bosshardt, Lichtenstein, Palumbo, & Zaporowski, 2010). Many public research universities have developed institutional aid policies that specifically target nonresident applicants (Burd, 2015b).

While public research universities historically prioritized access for resident students (Groen, 2004), in recent years many institutions have pursued nonresident enrollment growth. Nonresident students contribute to the enrollment goal of revenue generation because they pay higher net tuition when compared to resident students (Ma, Baum, Pender, & Bell, 2015). Jaquette and Curs (2015) showed that public universities, particularly research universities, increased nonresident freshman enrollment following declines in state appropriations.

The pursuit of nonresident students may force a tradeoff with other enrollment management goals, for example racial and socioeconomic diversity. Nonresident students tend to be more affluent and are less likely to be Black or Latino than resident students. Jaquette, Curs, and Posselt (forthcoming) found that growth in the share of nonresident students at public research universities is associated with a declining share of low-income students, and to a lesser extent the share of underrepresented minority students.

Prior research has not analyzed the effect of nonresident enrollment growth on resident enrollment. Winters (2012) examined the effect of resident enrollment demand on nonresident enrollment, finding that public universities increased resident enrollment and decreased nonresident enrollment in response to growth in resident student cohorts. However, given the recent growth in nonresident enrollment and stagnation of resident enrollment, state policymakers are concerned that nonresident students are crowding out enrollment opportunities for residents.

The Relationship between Nonresident and Resident Enrollment

This section describes potential mechanisms that could explain the relationship between nonresident and resident enrollment. Drawing from standard microeconomic theory, the effect of nonresident enrollment on resident enrollment depends on organizational constraints and preferences. When short-term enrollment supply is perfectly inelastic, then nonresident enrollment growth necessarily crowds out resident enrollment. Capacity constraints may arise at public universities for several reasons. First, universities may lack the necessary physical capital (e.g., dorms, classrooms) or labor (e.g., faculty, student service professionals) to enroll additional students. Second, when revenue declines universities may decide to restrain enrollment growth in order to retain a desired level of resources per student (Bound & Turner, 2007). Third, universities may be unwilling to increase enrollment out of concern that enrollment growth would negatively affect academic prestige, as enrollment growth could decrease selectivity, expenditure per student, or the academic profile of admitted students (Winston, 1999).

For the majority of institutions that are willing to increase enrollment capacity, the hypothesized relationship between nonresident enrollment and resident enrollment depends on the relative importance placed on the institution's enrollment management goals. At universities that prioritize access for residents, tuition revenue from nonresident enrollment growth can be used to increase resident enrollment. For example, universities can practice a high-tuition, high-aid model by allocating tuition revenue from nonresident students towards need-based aid for resident students (Mumper, 2003; Turner, 2006).

Alternatively, nonresident enrollment growth may negatively affect resident enrollment if nonresident tuition revenue is used to pursue prestige metrics, such as increasing expenditure per student or attracting students with higher standardized test scores. Burd (2015b) argues that public universities have used tuition revenue from nonresident students to increase merit aid for nonresident students. As competition for nonresident students intensifies, institutions may need to devote more recruiting, admissions, and financial aid resources towards nonresident markets. Finally, the cost of competing for nonresident students has likely increased as students expectations of institutional quality and quality-of-life amenities have increased (Jacob, McCall, & Stange, 2013, Long, 2004). Combined, growing nonresident enrollment may require increasing capital and auxiliary expenditures and shifting expenditure away from activities that promote access for resident students.

For the majority of institutions, the relationship between nonresident and resident enrollment is an empirical question as we are unable to make a definitive prediction of the direction of the relationship. However, we expect a negative relationship between nonresident enrollment and resident enrollment for intuitions likely to face short-term capacity constraints, in particular high-prestige universities that are reticent to grow total enrollment. In the next section, we describe our empirical strategy to identify the relationship between nonresident and resident enrollment.

Research Design

Empirical Framework

In this study we seek to identify the effect of nonresident enrollment growth on resident enrollment. Equation (1) shows a general institution-specific linear panel model, where $Resident_{it}$ is a measure of resident enrollment for university *i* in time *t*; $Nonresident_{it}$ is nonresident enrollment, with β as is its associated population coefficient; W_{it} is a matrix of time-varying covariates (including indicators for academic year); a_i is the unit-varying, time-invariant institution-specific effect; and e_{it} is the unit-varying, time-varying idiosyncratic effect.

(1) $$Resident_{it} = \beta Nonresident_{it} + \mathrm{W}_{\mathrm{it}}'\delta + a_i + e_{it}$$

Because nonresident enrollment is not randomly assigned, it is likely that nonresident enrollment is correlated with unobserved factors that affect resident enrollment. Thus, β is likely estimated with biased when estimated through standard regression techniques.

Two key assumptions must be satisfied in order to interpret the coefficient estimate on nonresident enrollment as a causal effect (Wooldridge, 2002). First, after controlling for covariates there should be relationship between nonresident enrollment and the unit-varying, time-invariant error component (a_i). Therefore, to control for unobserved time invariant heterogeneity we utilize an institution-level fixed effects estimator which eliminated the potential correlation between $Nonresident_{it}$ and a_i.

Second, after controlling for covariates there should be no relationship between nonresident enrollment and the unit-varying, time-varying error component (e_{it}). Given that fixed effects estimators satisfy the first assumption, we are primarily concerned about bias due to violations of the second assumption that occur when within-institution variation in nonresident enrollment is correlated with within-institution variation in the error term. Bias in $\hat{\beta}$ may occur if important covariates related to both $Nonresident_{it}$ and $Resident_{it}$ are excluded from the set of control variables.

In observational studies, attempts to control for all sources of bias through the inclusion of covariates are usually unsuccessful (Angrist & Pischke, 2009; Murnane & Willett, 2011). Further, scholarship on enrollment management suggests that strategies related to resident and nonresident enrollment are jointly determined (Cheslock & Kroc, 2012; Hossler & Bontrager, 2014). Therefore, it is preferable to identify a source of variation in nonresident enrollment that is exogenous. This paper attempted to isolate exogenous variation in nonresident enrollment using an instrumental variables approach.

Instrumental variables identification strategy. The instrumental variables estimation strategy calculates a consistent estimate of the population parameter, β, by using an instrumental variable, Z_{it}, to isolate exogenous variation in $Nonresident_{it}$. The logic is that variation in Z_{it} affects $Nonresident_{it}$ which in turn affects $Resident_{it}$. We applied a two-stage least squares approach to estimate the instrumental variables framework. The first stage equation (equation 2) models the effect of the instrument, Z_{it}, on the endogenous regressor, $Nonresident_{it}$, controlling for covariates, W_{it}:

(2) $$Nonresident_{it} = \beta Z_{it} + W'_{it}\delta + a_i + e_{it}$$

The outcome equation (equation 3) models the effect of $Nonresident_{it}$ on $Resident_{it}$, controlling for the same W_{it}. In equation (3) the actual values of $Nonresident_{it}$ were replaced with the first-stage predicted values of $Nonresident_{it}$. The predicted values, $\widehat{Nonresident_{it}}$, capture variation in the endogenous regressor that is explained by the exogenous instrument:

(3) $$Resident_{it} = \beta \widehat{Nonresident_{it}} + W'_{it}\delta + a_i + e_{it}$$

Angrist and Pischke (2009) describe four assumptions necessary for the instrumental variables framework to estimate a consistent causal effect with heterogeneous potential outcomes. First, the relevance assumption states that the instrument affects the endogenous regressor. Second, the independence assumption states that the instrument is as good as randomly assigned with respect to the outcome and potential treatment assignments. Third, the exclusion restriction assumption states that the instrument only affects the outcome through the endogenous regressor. Fourth, the monotonicity assumption states that, for all units in which the instrument affects the endogenous regressor, the instrument has the same directional effect on the endogenous regressor. After describing the data in the next section, we describe our candidate instrument and discuss the instrumental variables assumptions in greater detail.

Data and Variables

Data. We created an institution-level panel dataset, incorporating institution-level data from the Integrated Postsecondary Education Data System (IPEDS) and state-level data from various sources.

Analytical sample. The analytical sample consists of all public 4-year institutions defined as research-extensive or research-intensive by the 2000 Carnegie Classification. The analysis period consists of academic years 1992-93 through 2013-14, though some sensitivity analyses utilize a shorter period. The sample starts in 1992-1993 due to the availability of the migration component of the IPEDS fall enrollment survey and ends at the 2013-2014 academic year due to the availability of the instrumental variable.

Institution-year observations with missing values for resident enrollment and nonresident enrollment were dropped from analyses and not imputed. Missing institution-level covariates were imputed using the average of the within-panel one-year lag and lead observations. All variables have been log transformed to reduce heteroskedasticity due to large variation in the size and scope of higher education institutions. Descriptive statistics for institution-level variables (prior to the log transformation) are presented in Table 1 for the analytical sample of 2,669 institution-year observations, which includes 18 years of data and 159 public 4-year research universities.

Dependent variable. Resident freshman enrollment was collected from the Residence and Migration sub-component of the IPEDS Fall Enrollment survey. These data identify the number of freshman at the fall census date from each state, U.S. territory, and those migrating from a foreign country. We defined resident freshman enrollment as a student whose state of residence was the same as the state in which the institution is located.

Prior to 2000-01 the IPEDS Resident and Migration survey sub-component was collected in odd academic years (e.g., the 1992-93). Starting in 2001-02 institutions could voluntarily submit this sub-component in even academic years. Non-missing observations from voluntary years were included in the primary analytical sample. Sensitivity analyses which excluded observations from voluntary years are discussed in the robustness portion of the results section.

Independent Variable. The independent variable is nonresident freshman enrollment. This measure is defined as the number of freshman whose state of residence differed from the state which the institution is located and includes students from U.S. territories and students migrating from a foreign country. These data were also collected from the Residence and Migration sub-component of the IPEDS Fall Enrollment survey.

Control Variables. Choice of control variables was based upon two rationales. First, to increase precision and reduce omitted variables bias, we included time-varying covariates that plausibly affected resident enrollment and were correlated with nonresident enrollment. Thus, we include factors related to institutional demand for both resident and nonresident students. Second, we include variables to minimize threats to the independence and exclusion assumptions of the instrumental variables procedure.

At the institution-level, we included tuition and fees for resident students, tuition and fees for nonresident students, and average institutional grants. To capture variation in institutional quality and resources we include expenditures per full time equivalent (FTE) student for the following categories: instruction, research, public service, services (academic, student, and institutional support), and auxiliary enterprises. To control for changing higher education conditions in other states we included a geographically weighted measures of nonresident tuition levels and public research university capacity (the ratio of the population of 18-year olds relative to resident public research university enrollment). Considering the factors that could be correlated with resident enrollment at a particular institution and state-level merit-based aid generosity in other states, we control for the following state-level economic indicators: per capita income, annual unemployment rate, and total state population by the following age groupings: 12 to 17, 18 to 24, and 25 to 44. Finally, we control for state expenditures on needs-based and merit-based aid.

Instrumental Variable

To make our explanation concrete, imagine that we are trying to estimate the effect of nonresident enrollment on resident enrollment at the University of Alabama. We searched for instruments likely to affect demand for the University of Alabama of students from outside of Alabama but unrelated to demand for the University of Alabama by Alabama residents. To construct our instrument, we exploited state geographical boundaries and the fact that state financial aid policy only benefits students who resided within a state prior to their choice of higher education institutions.

State merit-based aid expenditure in other states served as the instrument for nonresident enrollment. An increase in state merit-based aid generosity decreases the relative cost of attending college in-state, thus increasing the likelihood that a student attends an in-state institution. A robust literature consistently finds that state merit aid decreased out-migration (e.g., Orsuwan & Heck, 2009; Zhang & Ness, 2010). Additionally, the vast majority of state merit-based aid programs restrict program eligibility to state residents whom enroll at an in-state institution. Therefore, we argue that state merit-based aid generosity in a particular state (e.g., Georgia) is unrelated to the decision of residents from another state (e.g., Alabama) to attend an institution in their own state (e.g., the University of Alabama).

Using the same geographic boundary argument, we explored other potential instruments. State need-based aid generosity in other states and the population of 18-year olds in other states were the two most promising alternative instruments. Unfortunately, neither of these instruments were strongly related to nonresident enrollment. Therefore, we do not include these instruments in our primary analyses but discus their inclusion in the robustness subsection of the presentation of the results.

Construction of the Instrument. The instrumental variable is defined as the weighted average of state-level merit aid expenditure per 18 to 24 year old in other states, with respect to a focal university. We constructed this variable using a gravity model approach often employed in interstate migration research (Alm & Winters, 2009; Cooke & Boyle, 2011). This approach assumes that migration from a sending state (e.g., Georgia) to a focal institution (e.g., University of Alabama) is positively related to the population in the sending state. In addition, migration is negatively related to the distance between the focal university and the sending state, with distance defined as the spherical distance from the focal university to the population centroid of the sending state.

For each unique combination of focal institution i, sending state s (excluding the state of the focal institution) and academic year t, we construct a time-varying weight (w_{ist}) that assigns higher weights to states that have larger populations (Pop_{st}) and are close to the focal institution ($Distance_{is}$). Specifically, w_{ist} is defined as:

(4) $$w_{ist} = \left(\frac{Pop_{st}}{Distance_{is}}\right) / \sum_{s=1}^{S} \left(\frac{Pop_{st}}{Distance_{is}}\right)$$

For each institution *i* in each year *t*, the sum of the weights across all states equals 1. Alternative specifications in which the weighting scheme was allowed to decay more rapidly (by squaring distance) or more slowly (by taking the square root of distance) generally produced weaker instruments but did not qualitatively alter the primary results (finding are discussed in the robustness section).

After calculating the weights for each intuition-state-year combination, we construct a weighted average of state merit-based aid generosity in other states ($Merit_{st}$) through the following calculation:

(5) $\overline{Merit_{it}} = \sum_{s=1}^{S}(w_{ist} * Merit_{st})$

Assumptions of the instrumental variables framework. The credibility of instrumental variables results depends on satisfying four assumptions (Angrist & Pischke, 2009).

Relevance assumption. First, the relevance assumption states that the instrument affects the endogenous regressor. The instrumental variables coefficient estimate is based solely on variation in the endogenous regressor that is conditionally correlated with the instrument. Murray (2006) shows that instrumental variables estimates are biased in the direction of the bias of an ordinary least squares estimate and that the bias of the instrumental variables estimate is inversely related to the strength of the conditional correlation between the instrument and the endogenous regressor.

Several formal tests exist for the null hypothesis that the instrument is uncorrelated with the endogenous regressor (Murray, 2006). Table 2, column 1 presents the estimated results of the first stage regression (Equation 2) of state merit-based aid in other states on nonresident enrollment. We found a negative relationship between state merit-based aid in other states and nonresident enrollment. Specifically, nonresident enrollment was found to decline by 0.5% as merit-based aid in other states increased by 1%. Both an F-test of excluded instruments test (F = 13.22, p<.01) and a Kleibergen-Paap underidentification test (χ^2 = 11.00, p<.01) indicated that the instrument identified relevant variation in nonresident enrollment. Furthermore, the Kleibergen-Paap weak identification test statistic was 13.22 (equivalent to the F-test of excluded instruments in this just-identified single endogenous variable model) indicating that the maximum relative bias was likely between 10% (critical value of 16.38) and 15% (critical value of 8.96) (Stock & Yogo, 2005).

Independence assumption. Second, the independence assumption states that, conditional on covariates, the instrument is independent from omitted variables that affect resident enrollment (Angrist & Pischke, 2009). Satisfying the independence assumption requires that, after including covariates state merit-based financial aid generosity in other states is uncorrelated with omitted variables that affect resident enrollment. This would imply that state merit-based aid in other states is as good as randomly assigned in that it has no systematic relationship with omitted factors affecting resident enrollment.

The independence assumption cannot be tested directly (Wooldridge, 2002), rather, it rests on the plausibility of a logical argument. We argue that geographical boundaries which restrict the availability of state merit-based aid to residents implies that state merit-based aid in one state is as good as randomly assigned with respect to residents of other states.

Because states adopt merit aid to compete with other states for the “best and brightest” students (Doyle, 2006), one possible concern with our argument is the potential for an arms race in state merit aid expenditure. Under this scenario, increased state merit aid spending in Georgia may cause Alabama to increase state merit aid spending, which may affect resident enrollment at the University of Alabama. We mitigate this potential problem by controlling for state expenditure on merit- and need-based aid within the focal institution’s state. Second, we control for economic factors in an institution’s state because economic factors are correlated across state boundaries and may be related to generosity of merit-based aid in other states.

Exclusion restriction assumption. The exclusion restriction assumption states that the only path through which the instrument ($\overline{Merit_{\iota t}}$) affects the dependent variable is through the endogenous regressor (Angrist & Pischke, 2009). This assumption can be violated even when the random assignment assumption is fulfilled (Angrist & Pischke, 2009). Continuing with the previous example, the exclusion restriction assumption is satisfied if increased generosity in Georgia merit-based aid only affects resident enrollment at the University of Alabama through its effect on the number of Georgia students matriculating to the University of Alabama.

A potential pathway concern is that increases in state merit aid causes resident enrollment in that state to increase to the point that in-state universities become capacity constrained and restricts nonresident enrollment. For example, increased generosity of state merit aid in Georgia could have increased resident enrollment at Georgia institutions. In turn, Georgia institutions may have had less capacity to enroll Alabama students, which may have caused the number of Alabama residents who attended the University of Alabama to increase. Winters (2012) found that when resident cohorts grew in size, institutions decreased nonresident enrollment and increased nonresident tuition. To overcome this pathway concern, we controlled for research university capacity through the weighted (same methodology as the instrument) ratio of state population of 18 year-olds divided by resident freshman enrollment at other states. When this ratio increased, public universities in other states (e.g., Georgia) faced capacity constraints due to increased resident enrollment, implying that they may not have enrolled as many nonresident students from the focal institution's state (e.g., Alabama).

Results

The Effect of Nonresident Enrollment on Resident Enrollment

Table 3 presents fixed effects (column 1) and instrumental variables (column 2) estimates of the relationship between nonresident enrollment and resident enrollment. Instrumental variables coefficients are estimated using a two-stage least squares estimator. In all models, robust standard errors clustered at the institution level are presented.

The fixed effects estimates indicate that a 1% increase in nonresident enrollment was associated with a 0.1% increase in resident enrollment. The point estimate from the instrumental variables procedure was negative, although insignificantly different from zero. Differences in magnitude and statistical significance between the fixed effects versus the instrumental variables results could be due to endogeneity bias in the fixed effects estimates or inefficiency in the instrumental variables estimates. Even if the positive point estimate from the fixed effects model is to be believed, the magnitude of this relationship is practically small and indicative of an enrollment management strategy in which nonresident enrollment is grown without restricting access for resident students.

Because a valid instrument only affects the dependent variable through its effect on the endogenous regressor, reduced form estimates of the effect of the instrument on the dependent variable are an important diagnostic check for instrumental variables analyses (Murray, 2010). If nonresident enrollment crowded out resident enrollment, we expect that increased state merit-based aid in other states to have had a positive effect on resident enrollment through decreased nonresident enrollment. Table 2, column 2 presents the reduced form estimates when the instrument $\overline{Merit_{it}}$ is substituted for the endogenous regressor in outcome Equation 3. Consistent with the instrumental variables results, the reduced form results indicated that neighboring state merit-based aid did not have a direct relationship with resident enrollment.

Heterogeneity by Institutional Type

In the case of truly inelastic supply we expect that any increase in nonresident enrollment must have come at the expense of resident enrollment. Prestigious universities are likely to restrict capacity to potential students in order to maintain or increase prestige. We utilized the 2004 US News and World Report National University Rankings as a proxy for prestige. The US News methodology rewards institutions based upon measured enrollment attributes, such as: spending per student, standardized test scores, and admissions rates. We separate institutions into two categories based on 2004 rankings: Prestigious institutions which are rated in the top 50 of national universities (17 of which are public universities), and less-prestigious institutions which includes all other public research universities (the remaining 142 institutions in the sample).

Figure 2 which restricts the sample to the 17 prestigious public universities, visually supports the argument that nonresident students may have crowded out resident students. Specifically, beginning in 2007 a decline in resident enrollment was observed while nonresident enrollment began to grow rapidly.

To statistically investigate the heterogeneity of this response across institutional prestige, we extended the empirical framework to allow the coefficient on nonresident enrollment to vary by institutional prestige (represented by the indicator variable D_{it}). To estimate an instrumental variables model with an interacted endogenous variable, two first stage equations are needed to model the effect of the instrument($\overline{Merit_{it}}$) on the endogenous regressor $Nonresident_{it}$ and the interaction term $Nonresident_{it} * D_{it}$ (Murnane & Willett, 2011; Wooldridge, 2002):

(6) $Nonresident_{it} = \beta\overline{Merit_{it}} + \beta_D(\overline{Merit_{it}} * D_{it}) + \mathrm{W}_{\mathrm{it}}'\delta + a_i + e_{it}$

(7) $(Nonresident_{it} * D_{it}) = \beta\overline{Merit_{it}} + \beta_D\overline{Merit_{it}} * D_{it}) + \mathrm{W}_{\mathrm{it}}'\delta + a_i + e_{it}$

Table 4 presents the first stage results for equation 6 (column 1) and equation 7 (column 2). In general, the first stage estimates were consistent with both directional and statistical significance expectations. At less-prestigious institutions a 1% increase in neighboring state merit-based financial aid lead to a 0.6% decrease in nonresident enrollment. For the prestigious institutions, a 1% increase in neighboring state merit-based financial aid lead to a 0.2% increase in nonresident enrollment (calculated through the addition of the four coefficients associated with state merit aid, 0.216 = -0.55 + 0.305 + -0.142 + 0.603), although the joint coefficient was insignificantly different from zero (p > .1). It is not surprising that neighboring state merit-based financial aid has an insignificant effect on nonresident enrollment at prestigious institutions because demand by Georgia residents for prestigious out-of-state institutions (e.g., University of Virginia) is less likely to be sensitive to changes in Georgia merit aid generosity than demand by Georgia residents for less-prestigious out-of-state institutions. The Kleibergen-Paap weak identification test statistic was 5.42, indicating that the maximum relative bias was likely between 10% (critical value of 7.03) and 15% (critical value of 4.58) (Stock & Yogo, 2005).

The outcome equation (equation 8) for the interacted instrumental variables equation contains the predicted values from both first stage equations.

(8) $$Resident_{it} = \beta \widehat{Nonresident}_{it} + \beta(\widehat{Nonresident}_{it} * D_{it}) + \mathrm{W}'_{\mathrm{it}}\delta + a_i + e_{it}$$

Table 5 presents fixed effects (column 1) and instrumental variables (column 2) estimates of the relationship between nonresident enrollment and resident enrollment when allowing the relationship to differ between more- and less-prestigious institutions. For the less-prestigious institutions, a 1% increase in nonresident enrollment was associated with a 0.1% increase in resident enrollment in the fixed effects estimator and a positive but insignificant relationship in the instrumental variables estimator insignificant relationship between nonresident enrollment and resident enrollment.

The coefficient on the interaction between nonresident enrollment and the prestige indicator indicates whether the effect of nonresident enrollment was different for prestigious institutions and less-prestigious institutions. For the fixed effects model (column 1), this interaction coefficient is negative and insignificant, and the combined marginal effect for prestigious institutions indicated a positive but insignificant point estimate (0.07, p=.39). For the instrumental variables model (column 2), the interaction effect is negative and statistically significant. The marginal effect for prestigious institutions indicates that 1% increase in nonresident enrollment leads to a 0.2% (=0.016 - 0.197) decrease in resident enrollment (p=.07).

The instrumental variables estimate for prestigious institutions suggested a crowding-out effect on resident enrollment as institutions increased nonresident enrollment. For the average prestigious public research institution during the final year of our sample 2012-2013, this relationship suggest that, roughly, for an increase of 15 nonresident freshman (1% of 1,467) resident freshman enrollment would decrease by 9 (0.2% of 4,277). Alternatively, between 2012-13 and 2014-15 nonresident enrollment grew by an average of 5.4% per year, which translates to an increase in 80 nonresident students having crowded out 46 resident students. Thus, although the coefficients expressed as elasticities appear to be relatively small, the estimated crowd-out effect is practically important.

Robustness to Alternative Specifications

Alternative instruments. An important concern with the instrumental variables estimation strategy are potential biases in the coefficient of interest due to a weak instrument. In the models estimated previously, weak identification tests using Stock and Yogo (2005) critical values indicated that the maximum relative size distortion associated with our models was less than 15%. The failure to reject a hypothesis of less than 10% may lead one to question the strength of our instrumental variable. To assess the potential biases in our estimates we re-estimated the primary models with alternative constructions of the instrumental variable.

Table 6 presents the coefficient of interest, the effect of nonresident enrollment on resident enrollment, for alternative constructions of the instrumental variable. Column 1 represents the equivalent model as presented in Table 3, with columns 2 through 7 present the re-estimation of this model with alternative sets of instruments. Panel B represents the same progression of models for the interaction model presented in Table 5.

Estimates were robust to alternative distance weighting schemes in the construction of the merit-based aid instrument. Specifically, we allowed the weighting scheme to be more sensitive to distance by squaring distance (column 2) and less sensitive to distance by taking the square root of distance (column 3). For both alternative instrument distance treatments, estimates of the relationship between nonresident and resident enrollment were qualitatively similar to the linear treatment. However, in both cases the underidentification and weak identification statistics indicated that the alternatively weighted instruments did not predict nonresident enrollment as well as the linearly weighted instrument.

Following the approach of Angrist and Krueger (1991), which interacted the instrument with plausibly exogenous factors in an effort to increase the relative strength on the instrument. Columns 4 and 5 present results when the merit aid instrument was interacted with indicators of the institution’s Census division (eight divisions) and 2004 US News and World Report tier (four tiers). In general, the point estimates of the coefficient of interest were qualitatively similar, although in the census division model the interaction term is not statistically significant. In each case, although the overall ability of the set of instruments to explain the endogenous regressor improved as measured by the underidentification test, the maximum relative size distortion increased due to the added instrumental variables.

Following the same geography-based logic made for the choice of state merit-based aid as a valid instrument, one could argue for the inclusion of state needs-based aid as an alternative instrument. Estimates using both state merit- and need-based aid as instruments (column 6), find qualitatively similar results as models with solely merit-based aid as an instrument. The inclusion of needs-based aid decreases the strength of the instruments in explaining nonresident enrollment and increased the maximum relative size distortion of the instrumental variables estimate.

Following the approach of Winters (2012) we included the state population of 18 year-olds in other states in addition to state merit-based aid in the instrument set (column 7). Estimates of the relationship between nonresident and resident enrollment are similar to our base model. Similar to the addition of needs-based aid, measures of the strength of the relevance of the instrument indicate the combined population and merit-based aid instrument set is weaker than using merit-based aid alone.

Alternative pathway controls. Another concern with instrumental variables estimates is the potential presence of alternative pathways between the instrument and outcome. Drawing from the findings of Winters (2012), we described earlier the following alternative pathway: increased merit based-aid in Georgia positively affected demand at in-state institutions by Georgia residents, which crowded out Alabama residents from Georgia institutions and, in turn, increased the number of Alabama residents attending in-state institutions. Our primary analyses attempted to mitigate this pathway concern by controlling for nonresident tuition price and a proxy for research university capacity in other states.

Starting in 2002, IPEDS began to provide more detailed information regarding institutional selectivity measures. As a robustness check, we include the weighted average (based upon distance from the focal institution to competitor public research universities in other states) acceptance rate and nonresident tuition in place of the state-level measures of public university capacity and nonresident tuition. The tradeoff for including better measures of institutional selectivity is the reduced sample size due to data availability (about 30% of our sample was lost).

Table 7, presents the results of this sensitivity check where column 1 presents the findings from the preferred specification, column 2 presents the preferred specification restricted to the years for which the new pathways controls were available, and column 3 presents the results using the new pathways controls in place of the original controls. For the pooled model, results are similar across all three specifications presented in Table 7. The primary difference is that the instruments were considerably weaker when the sample was restricted to the shorter time period for both specifications of control variables. For the interaction model, the negative relationship is found for prestigious institutions in all three specifications. The point estimates were more negative in both of the shorter period models, although the corresponding confidence intervals were much larger.

Alternative samples. Table 8 presents results comparing the coefficient of interest for models that utilized observations from years where the IPEDS Residence and Migration survey was voluntary (columns 1 and 2) compared to specifications where those observations were dropped (columns 3 and 4). The findings are qualitatively similar across both samples. The key difference of interest resides in the instrumental variables estimates of the interaction model, where the prestigious interaction coefficient is not statistically significantly different from zero, although the overall point estimate was similar. Thus, while statistical significance is lost the overall estimate of the effect size remained relatively stable across samples.

Discussion

During the 2000s, resident enrollment growth at public research universities started to stagnate while nonresident enrollment growth increased. More recently, state policymakers became concerned that the increased emphasis on the enrollment of nonresident students could be coming at the expense of access for resident students (Burd, 2015a, 2015b). This study provided empirical evidence as to whether increased enrollment growth of nonresident students at public research universities crowded out access for resident students.

For the full sample of research universities, instrumental variables results did not indicate that enrollment growth of nonresident students crowded out the enrollment of resident students. Thus, the evidence suggests that public research universities likely pursued an enrollment strategy to increase nonresident students independent of their resident enrollment strategy. However, at prestigious public research universities evidence was found that increased nonresident enrollment caused a decrease in resident enrollment. In fact, between 2012-13 and 2014-15 at the average prestigious university the model predicted that 46 resident students were crowded out by the average annual increase of 80 nonresident students. These findings are consistent with prestigious universities having inelastic short term supply, in which an increase in nonresident enrollment must come at the expense of a resident student.

We urge caution when interpreting the findings due to several limitations. First, we remain somewhat concerned about the overall strength of the merit-based aid instrument to predict nonresident enrollment. In the preferred specifications, weak identification tests indicated that the maximum size distortion of our instrumental variables estimates was less than 15% when ideally a strong instrument would be less than 10%. Thus, some potential bias is likely to remain in our instrumental variables estimates, which is particularly important when interpreting the findings for prestigious institutions. A second limitation exists due to the regional nature of large scale state merit-based aid programs. Although state merit-based aid programs exist across the country, the largest are concentrated in the southern part of the United States. Thus, variation in the instrumental variable was largest in the south which may limit the generalizability of the findings as instrumental variables estimates a local average treatment effect.

The findings suggest that policymakers should not be concerned that nonresident enrollment growth at public research universities is crowding out resident enrollment, except perhaps at the most prestigious universities. Thus, policy changes aimed at the growth of nonresident enrollment at public universities, such as nonresident enrollment caps, are unlikely to increase access for resident enrollment. Furthermore, restricting nonresident student enrollment may be against state fiscal interests as nonresidents students have been found to pay more in net-tuition and future state taxes than resident students (Groen & White, 2004). This is particularly true for the prestigious institutions, which are most likely to be attracting high academic ability students to their state. Given the significant increase in interstate migration for higher education more research is needed to understand whether such policies are beneficial to, or may harm, state economic development goals.

Similarly, future research is needed to better understand the benefits and consequences of the pursuit of nonresident enrollment at public research universities. For example, research has shown that spending on instruction and student services is positively associated with graduation rates (Webber, 2012; Webber & Ehrenberg, 2010). Public universities often argue that nonresident enrollment growth is necessary to maintain quality instruction and student services amidst state funding cuts. However, prior research has not addressed how public universities are spending revenue from nonresident tuition. Therefore, more research is needed to investigate whether universities are expending these revenues on practices shown to improve educational outcomes as opposed to expenditures on non-academic improvement, such as recreation centers, athletics, and other facilities.

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Table 1: Descriptive Statistics

	(1) Full Sample	(2) US News Top 50 Institutions	(3) Less-prestigious Institutions
Resident enrollment	2,519.4	3,904.3	2,361.6
	(1,562.5)	(2,001.0)	(1,421.8)
Nonresident enrollment	648.4	913.0	618.2
	(650.2)	(804.6)	(623.4)
Z_{it}: Merit-aid expenditure in other states	73.98	73.53	74.03
	(37.57)	(33.53)	(38.01)
Instate tuition and fees	5,562.4	6,641.0	5,439.3
	(2,875.0)	(3,359.7)	(2,788.8)
Nonresident tuition and fees	14,530.1	20,720.7	13,823.2
	(6,600.3)	(8,463.8)	(5,959.0)
Institutional grants per FTE	1,450.6	2,464.2	1,335.0
	(1,300.9)	(1,936.8)	(1,152.5)
Instructional expenditure per FTE	8,595.0	15,646.0	7,793.9
	(4,674.1)	(7,123.6)	(3,505.8)
Research expenditure per FTE	5,161.6	13,336.8	4,232.3
	(5,627.3)	(6,925.9)	(4,622.7)
Public service expenditure per FTE	1,745.6	2,196.0	1,694.4
	(2,074.7)	(2,013.7)	(2,075.7)
Academic and support services expenditure per FTE	5,435.6	9,101.4	5,019.1
	(2,877.0)	(3,716.7)	(2,439.7)
Auxiliary expenditure per FTE	3,266.2	6,104.0	2,943.7
	(2,696.3)	(4,997.9)	(2,061.1)
Nonresident tuition and fees in other states	14,924.5	14,570.6	14,964.8
	(5,095.1)	(4,957.8)	(5,110.0)
Ratio of the population of 18 year olds to enrollment	12.52	12.15	12.57
in public research institutions in other states	(1.404)	(1.194)	(1.421)
Observations	2,669	273	2,396
Institutions	159	17	159

Note. Sample means are reported with standard deviations in parentheses.

Table 2: Relevance of the Instrument: First Stage and Reduced Form Estimates

	(1)	(2)
	First stage	Reduced form
Y:	*Nonresident$_{it}$*	*Resident$_{it}$*
Z_{it}: Merit-aid expenditure in other states	-0.553***	0.0202
	(0.153)	(0.0721)
Instate tuition and fees	0.118	-0.0137
	(0.165)	(0.0848)
Nonresident tuition and fees	-0.264**	0.0260
	(0.113)	(0.0493)
Institutional grants per FTE	0.0429**	0.000193
	(0.0200)	(0.00689)
Instructional expenditure per FTE	0.0248	-0.178**
	(0.155)	(0.0847)
Research expenditure per FTE	-0.0588	0.0108
	(0.0455)	(0.0251)
Public service expenditure per FTE	-0.0114	-0.00727
	(0.0303)	(0.0147)
Academic and support services expenditure per FTE	-0.276**	-0.168***
	(0.120)	(0.0523)
Auxiliary expenditure per FTE	0.114**	0.0446
	(0.0472)	(0.0302)
Per-capita income in own state	4.173***	1.194***
	(1.090)	(0.375)
Unemployment rate in own state	-0.958	1.309**
	(1.331)	(0.519)
Nonresident tuition and fees in other states	0.697	0.00539
	(0.494)	(0.246)
Ratio of the population of 18 year olds to enrollment	-0.0401**	-0.00756
in public research institutions in other states	(0.0174)	(0.00862)
Expenditure on merit aid in own state	0.00774**	0.000394
	(0.00317)	(0.00105)
Expenditure on needs-based aid in own state	0.00734	0.00188
	(0.00535)	(0.00244)
Population of 12-17 year olds in own state	1.491***	0.951***
	(0.506)	(0.173)
Population of 18-24 year olds in own state	-0.646*	0.447***
	(0.367)	(0.170)
Population of 25-44 year olds in own state	-1.826***	-0.358*
	(0.568)	(0.198)
Year indicators	Yes	Yes
Institution fixed effects	Yes	Yes
Observations	2,669	2,669
R-squared	0.333	0.452
Institutions	159	159
F-test of excluded instruments	13.12***	
Underidentification test	10.97***	
Weak Identification Test	13.12	

Note. Robust standard errors clustered at the institution-level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 3: The Effect of Nonresident Enrollment on Resident Enrollment

	(1) Fixed Effects	(2) Instrumental Variables
Nonresident enrollment	0.0901**	-0.0366
	(0.0363)	(0.131)
Instate tuition and fees	-0.0251	-0.00935
	(0.0759)	(0.0892)
Nonresident tuition and fees	0.0546	0.0163
	(0.0498)	(0.0591)
Institutional grants per FTE	-0.00386	0.00176
	(0.00733)	(0.00974)
Instructional expenditure per FTE	-0.182**	-0.177**
	(0.0782)	(0.0872)
Research expenditure per FTE	0.0165	0.00865
	(0.0234)	(0.0278)
Public service expenditure per FTE	-0.00662	-0.00769
	(0.0132)	(0.0151)
Academic and support services expenditure per FTE	-0.141***	-0.178***
	(0.0479)	(0.0657)
Auxiliary expenditure per FTE	0.0354	0.0488
	(0.0269)	(0.0309)
Per-capita income in own state	0.924**	1.347**
	(0.377)	(0.655)
Unemployment rate in own state	1.291**	1.274***
	(0.505)	(0.478)
Nonresident tuition and fees in other states	-0.0429	0.0309
	(0.247)	(0.259)
Ratio of the population of 18 year olds to enrollment in public research institutions in other states	-0.00336	-0.00902
	(0.00886)	(0.0108)
Expenditure on merit aid in own state	-0.000640	0.000677
	(0.00110)	(0.00159)
Expenditure on needs-based aid in own state	0.00121	0.00215
	(0.00256)	(0.00262)
Population of 12-17 year olds in own state	0.849***	1.006***
	(0.172)	(0.246)
Population of 18-24 year olds in own state	0.476***	0.423**
	(0.169)	(0.177)
Population of 25-44 year olds in own state	-0.194	-0.424
	(0.191)	(0.294)
Year indicators	Yes	Yes
Institution fixed effects	Yes	Yes
Observations	2,669	2,669
R-squared	0.477	0.427
Institutions	159	159

Note. Robust standard errors clustered at the institution-level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 4: Interaction Model: Relevance of the Instrument: First Stage and Reduced Form Estimates

	(1)	(2)	(3)
	First stage		Reduced form
Y:	*Nonresident*$_{it}$	*Nonresident*$_{it}$ * *Prestigious*$_{i}$	*Resident*$_{it}$
Neighboring state merit-aid expenditure	-0.550***	-0.142**	0.0190
	(0.153)	(0.0657)	(0.0720)
Neighboring state merit-aid expenditure * Prestigious institution	0.305**	0.603***	-0.114***
	(0.137)	(0.139)	(0.0269)
Control variables	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes
Institution fixed effects	Yes	Yes	Yes
Observations	2,669	2,669	2,669
R-squared	0.346	0.311	0.460
Institutions	159	159	159
F-test of excluded instruments	8.096***	11.06***	
Underidentification test	9.209***		
Weak Identification Test	5.424		

Note. Robust standard errors clustered at the institution-level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 5: Interaction Model: The Effect of Nonresident Enrollment on Resident Enrollment

	(1) Fixed Effects	(2) Instrumental Variables
Nonresident enrollment	0.0975***	0.0164
	(0.0354)	(0.145)
Nonresident enrollment	-0.0287	-0.197**
* Prestigious institution	(0.0801)	(0.0968)
Control variables	Yes	Yes
Year indicators	Yes	Yes
Institution fixed effects	Yes	Yes
Observations	2,669	2,669
R-squared	0.478	0.407
Institutions	159	159

Note. Robust standard errors clustered at the institution-level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 6: Robustness of Findings to Alternative Choices of Instruments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Instrument set:	Merit aid	Merit aid	Merit aid	Merit aid by Census division	Merit aid by US News Tier	Merit and need-based aid	Merit aid and 18 year-old population
Distance weight:	Linear	Squared	Square root	Linear	Linear	Linear	Linear
Panel A: Pooled model							
Neighboring state merit-aid expenditure	-0.0366	0.0845	-0.0531	-0.101	-0.124	-0.0359	-0.0367
	(0.131)	(0.147)	(0.163)	(0.0701)	(0.104)	(0.128)	(0.131)
Observations	2,669	2,669	2,669	2,669	2,669	2,669	2,669
R-squared	0.427	0.477	0.412	0.362	0.333	0.427	0.427
Institutions	159	159	159	159	159	159	159
Underidentification test	10.97***	6.369***	6.905***	22.32***	17.39***	11.72***	11.23***
Weak Identification Test	13.12	9.775	6.912	4.843	5.639	6.986	6.722
Maximum relative IV size distortion[a]	15%	15%	20%	>25%	>25%	>25%	>25%
Panel B: Interaction model							
Neighboring state merit-aid expenditure	0.0164	0.115	-0.000735	-0.0201	0.218	0.0141	0.0276
	(0.145)	(0.160)	(0.182)	(0.111)	(0.153)	(0.141)	(0.141)
Neighboring state merit-aid expenditure * Prestigious institution	-0.197**	-0.254**	-0.184*	-0.124	-0.303***	-0.202**	-0.206**
	(0.0968)	(0.103)	(0.110)	(0.149)	(0.0917)	(0.0966)	(0.0932)
Observations	2,669	2,669	2,669	2,669	2,669	2,669	2,669
R-squared	0.407	0.434	0.399	0.407	0.413	0.403	0.411
Institutions	159	159	159	159	159	159	159
Underidentification test	9.209***	6.332***	5.752***	10.68***	13.29***	10.24**	10.69**
Weak Identification Test	5.424	4.371	2.898	1.903	4.182	3.007	3.106
Maximum relative IV size distortion[a]	15%	20%	>25%	>25%	>25%	>25%	>25%

Note. Robust standard errors clustered at the institution-level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

[a] The percentage represents the maximum relative size distortion based upon critical values in Stock and Yogo (2005).

Table 7: Robustness of Findings to Alternative Pathway Controls

	(1)	(2)	(3)
Pathways Controls:	Original[a]	Original[a]	Revised[b]
Sample Period:	1993-2014	2002-2014	2002-2014
Panel A: Pooled model			
Neighboring state merit-aid expenditure	-0.0366	-0.0271	0.00539
	(0.131)	(0.178)	(0.171)
Observations	2,669	1,877	1,877
R-squared	0.427	0.144	0.188
Institutions	159	159	159
Underidentification test	10.97***	4.964**	5.076**
Weak Identification Test	13.12	5.832	5.964
Maximum relative IV size distortion[a]	15%	>25%	>25%
Panel B: Interaction model			
Neighboring state merit-aid expenditure	0.0164	0.120	0.0952
	(0.145)	(0.201)	(0.185)
Neighboring state merit-aid expenditure * Prestigious institution	-0.197**	-0.626*	-0.609*
	(0.0968)	(0.354)	(0.342)
Observations	2,669	1,877	1,877
R-squared	0.407	-0.334	-0.330
Institutions	159	159	159
Underidentification test	9.209***	4.526**	4.635**
Weak Identification Test	5.424	2.525	2.703
Maximum relative IV size distortion[a]	15%	>25%	>25%

Note. Robust standard errors clustered at the institution-level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

[a] The original pathway controls include weighted (by population and distance) average state-level measures for nonresident tuition and fees and research university capacity (18 year-old population / resident enrollment at public research universities).

[b] The revised pathway controls include weighted (by distance) average institution-level measures for the percent of applicants admitted, instate tuition and fees, nonresident tuition and fees, and institutional grants.

Table 8: Robustness of Alternative Samples

	(1)	(2)	(3)	(4)
Sample Period:	1993-2014		1993-2014	
Voluntary Residence and Migration Observations:	Yes		No	
	Fixed Effects	Instrumental Variables	Fixed Effects	Instrumental Variables
Panel A: Pooled model				
Neighboring state merit-aid expenditure	0.0901**	-0.0366	0.0624**	-0.0487
	(0.0363)	(0.131)	(0.0314)	(0.127)
Observations	2,669	2,669	1,746	1,746
R-squared	0.477	0.427	0.532	0.491
Institutions	159	159	159	159
Underidentification test		10.97		10.37
Weak Identification Test		13.12***		12.25***
Maximum relative IV size distortion[a]		15%		15%
Panel B: Interaction model				
Neighboring state merit-aid expenditure	0.0975***	0.0164	0.0868**	-0.0103
	(0.0354)	(0.145)	(0.0384)	(0.142)
Neighboring state merit-aid expenditure * Prestigious institution	-0.0287	-0.197**	-0.0947**	-0.143
	(0.0801)	(0.0968)	(0.0389)	(0.0937)
Observations	2,669	2,669	1,746	1,746
R-squared	0.478	0.407	0.539	0.497
Institutions	159	159	159	159
Underidentification test		9.209***		8.673***
Weak Identification Test		5.424		5.020
Maximum relative IV size distortion[a]		15%		15%

Note. Robust standard errors clustered at the institution-level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

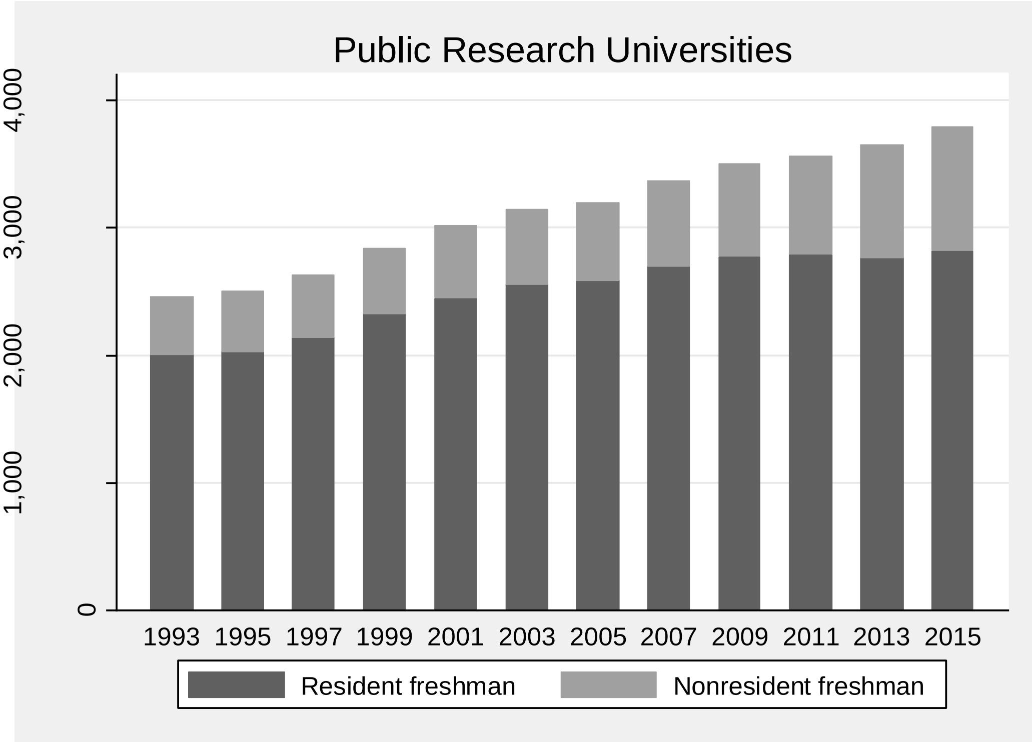

Figure 1: Average First-time freshman enrollment at public research universities. Based upon authors' calculations of IPEDS data for the total sample (N=159).

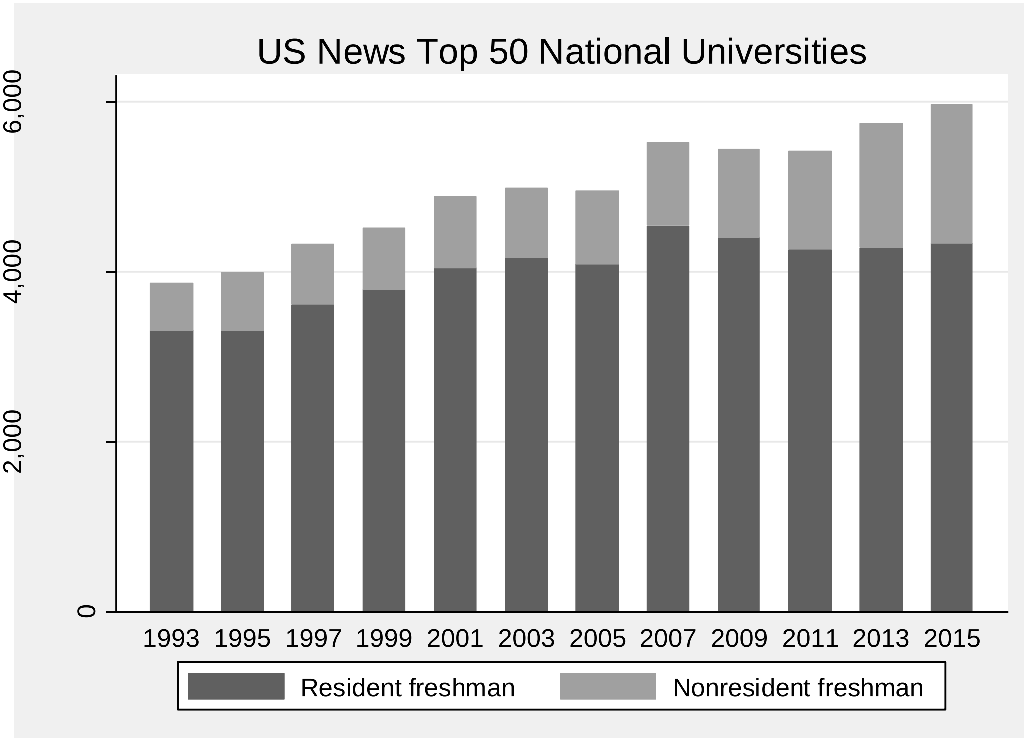

Figure 2: Average First-time freshman enrollment at prestigious (ranked in the top 50 of national universities by US News and World) public research universities (N=17). Based upon authors' calculations of IPEDS data.