Income-targeted financial aid and patterns of post-secondary matriculation

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September 2008

Preliminary version. Not to be quoted without author consent.

Abstract

Using the universe of Pell Grant recipients, arguably a complete snapshot of all low-income students enrolled in post-secondary education in the United States, we empirically examine whether there is discernable variation in the matriculation patterns of low-income students around changes in institutional financial-aid policy that target low-income students with need-based aid. We consider efficacy as borne out in both institutional enrollments of low-income students and in the institutions' geographic basins of attraction. While regularities in the data suggest that these initiatives have had little broad influence, large enrollment responses at flagship public institutions and significant changes to the distance low-income students travel to certain types of institutions are documented.

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Acknowledgements: We thank Peter Stiffler for helpful research assistance. Any and all errors remain the authors' responsibility.

1 Introduction

In the preceding decade, over thirty of the top post-secondary institutions in the United States have formally introduced financial-aid programs that explicitly target students from low-income families. Using the universe of Pell Grant recipients in academic years 1999 through 2007, we empirically examine whether there is discernable variation in the matriculation patterns of low-income students around such changes in institutional financial-aid policy.

Despite significant resources being spent on need-based financial aid in the United States, the gap between low- and high-income students' matriculation rates has not only persisted but has widened in the last three decades (Ellwood and Kane, 2000). Thus, understanding matriculation patterns is not unimportant, especially given the suggestion from existent literature that enticing an otherwise non-college bound, low-income student to matriculate to college with federal aid is not easily accomplished.[1] Knowing this, we continue to see large transfers toward need-blind admissions and "all need met" policies.

Income-targeted need-based programs have grown out of recognition on the part of policy pundits and university administrators that the rising real cost of college and student debt level have potentially threatened the access of low-income students to college. For example, Mishel, Bernstien, and Allegretto (2007) find that the real wage premium rose by 27 percent between 1993 and 2005, whereas the Trends in College Pricing (2005, Table A1) indicate that real tuition and fees rose by 63 (43) percent at private (public) universities. Snyder, Tan, and Hoffman (2006) show that, commiserate with the rising real cost of college, students are: (1) more likely to require student aid to attend college; (2) covering a smaller portion of their college costs with grants; and (3) taking out nearly twice the level of debt in real terms than in the previous decade (i.e., over $20,000 in 2004). Because federal need-based aid programs have not kept pace with the growing costs of col-

[1] Enrollment effects in general populations of students have been weak (e.g., Hansen 1983; Kane 1994; Kane 1995; Heller 1997; McPherson and Schapiro 1998; Kane 2001). There is evidence of Pell influencing more-narrowly-defined groups of institutions or students. For example, Kane (1995), although not separating needy from non-needy students, suggests that Pell increases overall enrollment at public two-year colleges. Seftor and Turner (2002) report increased access for non-traditional students, using variation in the Pell-eligibility formula in the late 1980s that increased the generosity of the program for financially independent students.

lege (e.g., McPherson and Shapiro, 1998), a growing number of institutions and some states have attempted to bridge the funding gap in an attempt to ensure the accessibility of college to needy students.

Yet, relatively few studies have examined whether these targeted need-based aid programs actually improve access. Most recent work has exploited natural experiments in federal, state, and institutional aid programs to focus more generally on whether an exogenous increase in financial aid increases the likelihood that students apply, enroll, and graduate from college (e.g., Angrist, 1993; Dynarksi, 2000; Bound and Turner, 2002; Long, 2004; Cornwell, Mustard & Sridhar, 2006). This research speaks to the potential efficacy of targeted-aid programs because prior work generally does find consistent evidence that improved generosity of financial aid improves some college outcomes, albeit often by a relatively small amount (e.g., Long, 2004; Dynarksi, 2004; Bettinger 2004; Singell, Waddell, and Curs, 2006). Our analysis builds on this literature, examining how institutional need-based aid programs affect several dimensions of access in comparison to similar institutions that do not adopt these programs using data for all Pell students that comprise the nearly the universe of low-income students. Moreover, the absolute size of the Pell program permits us to examine the impact of these targeted need-based programs across several different sets of public and private universities, which allows us to demonstrate that the access effects of these programs differ distinctly across institution types (e.g., between public versus private universities, selective versus less selective institutions, in-state versus out-of-state students).

Two recent studies have examined the efficacy of targeted need-based aid programs, both using institution-specific data for two elite and highly selective schools. In general, they suggest that credit constraints play an important role in the college behavior of low-income students. First, Avery, et al. (2006) uses administrative and Census data to evaluate the first year of Harvard University's Financial Aid Initiative (HFAI), which increased aid for the recruiting of low-income students. Using estimated family incomes for "plausible U.S. applicants," they find that HFAI attracted a larger and slightly poorer pool of applicants and that, once admitted, enrolled at rates similar to the prior year. This suggests that HFAI was effective at recruiting low-income students due to some untapped supply of qualified students who do not to apply to Harvard in the absence of such aid. However, it is unclear the extent to which Harvards program merely expands their own pool of high-quality students at the expense of other selective institutions.

Second, Rothstein and Rouse (2007) use data for unnamed selective university that adopted a non-loan policy under which the loan component of financial aid awards was replaced with grants. This natural experiment is used to assess the causal effect of student debt on employment outcomes. They find that that debt affects students' academic decisions during college and induces graduates to choose substantially higher-salary jobs and lowers their probability of choosing poorly paid "public interest" jobs. Moreover, the paper provides suggestive evidence that credit constraints and debt aversion potentially interfere with a student's ability to optimize over the life cycle. In particular, debt is found to reduce students' donations to the institution in the years after they graduate and increases the likelihood that graduates default on a pledge made during their senior year.

As a general approach, we think of efficacy being evidenced in two distinct ways. First, we consider efficacy as borne out in institutional enrollments. Do financial aid policies targeting low-income students change the number of low-income students matriculating to campus? We find scant evidence that these policies have influenced low-income enrollment levels. Second, we consider efficacy as borne out in an institutions geographic basin of attraction. Do these policies change the geographic dispersion of the low-income students they successfully enroll? Are these institutions able to better draw in low-income students for having these policies in place? With an objective of aid policies being to facilitate the match between needy students and institutions, any change in an institutions basin of attraction can be interpreted as an indication that a prior constraint on the matriculation of low-income students has been adjusted. We show that these basins of attraction do change, with mass (i.e., in a distributional sense) tending to shift toward relatively greater distances traveled by enrolling low-income students at some treated institutions. We will interpret this evidence as suggestive that aid policies such as those analyzed can to relax geographic barriers to the enrollment of low-income students. While evaluating the match itself is not the focus of our investigation, we expect that such movement is in the direction of improving the potential matching of students with institutions.

In the following section we describe the data used in our analysis and provide some summary statistics that are new to the literature given our access to the entire "Pell recipient file" of the United States Department of Education. In Section 3, we discuss the various types of changes to financial aid policy that fall within our sample period and thereby set up our empirical test of institutional low-income enrollment levels around these changes. In the process of doing so, we identify the institutions composing our treatment groups and the sets of institutions to serve as controls against which we will measure any difference in differences. In Section 4, we develop a model of the enrollment response to the adopted aid initiatives. In Section 5, we examine student-traveled distance both parametrically and non-parametrically. In Section 6 we offer some concluding remarks and discuss several important questions that remain unanswered.

2 Data

Our primary data source is the Pell "recipient file" held by the offices of the U.S. Department of Education, obtained through our request under the Freedom of Information Act (FOIA). While the dataset includes all Pell recipients over the academic years 1999 through 2007 (at roughly 6 million observations per year), we use only those students who are recorded as first-time recipients in their first grade. To receive federal aid, a student must rst complete a Free Application for Federal Student Aid (FAFSA) form, which provides aid administrators with the information needed to determine the size of an applicants Pell Grant.[2] Related research has relied on indirect measures for the number of low-income students, such as minority enrollments or other student background measures that are correlated with income (e.g., Kane 1994; Dynarski 2004).[3] To the contrary, our analysis exploits unique student-level Pell data to directly examine the effects of changes in aid policy on low-income students. Furthermore, the program size ensures that our data constitute nearly the population of poor students attending U.S. higher-educational institutions.

[2]At this time we do not use the information contained in the level of grant. Refinements to the the larger dataset are being made over time. For example, we currently drop student observations where we are unable to match them an institution within the IPEDS dataset using the mapping of Pell-provided institution code to IPEDS institution code. We are confident that this problem is primarily one that exists outside of the well-established institutions we analyze here. As this type of data issue would keep entire institutions from appearing in our final sample, that we have the entire sets of COFHE, top liberal arts colleges and state flagships is consistent with having no missing students at these institutions. (The student counts provided by the Department of Education match those of the University of Oregon.)

[3]See Curs, Singell, and Waddell (2007b) for a more comprehensive review of the related literature and a summary of the history and chronology of the Pell program.

To this Pell data, we integrate detailed institution-level information from the Post-Secondary Education Data System (IPEDS). We further supplement these data with information from the 2000 US Census, using standard zip-level aggregates on income, population, and educational measures.

3 Income-targeted financial aid

While there has been a general move toward relaxing budgetary constraints on low-income students with targeted aid, there are a variety of methods by which this is administered. Four typical allocation mechanisms are "no loan" policies that eliminate loans for low-income students (e.g., Princeton, Rice, UNC Chapel Hill, Virginia, Pennsylvania),[4] "loan cap" policies that institute a low cap on student loans for low-income students (e.g., Brown), "no parental contribution" policies that eliminate the parental contribution but retain the student contribution or the standard self-help level (e.g., Yale, Stanford), and "Pell Grant match" policies that match the student's Federal Pell Grant and, while not meeting all need by definition, reduces the self-help level but does not necessarily leave the student without the need of loans (e.g., the Minnesota system).

While subtleties exist that can arguably set each program apart from the others in some way, out intent is to broadly define such initiatives as roughly comparable to each other and look to the empirical question of whether these policies have evidenced themselves in outcomes in any significant ways. We use a difference-in-differences approach to determine whether any significant changes arise in our two key measures of efficacy. That is, we measure differences (i.e., in the enrollment of low-income students and in the institution's basin of attraction) observed at treated institutions against differences observed over the same time period at institutions which did not adopt targeted-aid programs. For each of the three groupings of institution the ordering of our empirical tests is as follows. First, we consider the ten COFHE institutions which instituted income-targeted aid within the sample period.[5] As a set control institutions we adopt the entire set of COFHE institutions.[6]

[4]Some variation still exists within such a group. For example, Princeton has eliminated loans from the aid packages of all students.

[5]Consortium on Financing Higher Education

[6]COFHE member institutions include Amherst College, Barnard College, Brown University, Bryn Mawr College, Carleton College, Columbia University, Cornell University,

Second, we consider three liberal-arts colleges which instituted such aid initiatives and measure them against the set of top-40 liberal arts colleges.[7] Third, we consider the ten flagship public institutions that had instituted targeted aid by 2007 and compare them to the set of control institutions defined as the rest of the flagship publics.[8]

While certain institutions have been noted above, Table 1 identifies the set of institutions at which income-targeted initiatives were implemented within our sample period of 1999 through 2007. In particular, the delineation of institutions in the table itself is consistent with the three-fold approach we adopt in our subsequent analysis.

Table 1: Institutions with targeted financial aid (with year of adoption)

COFHE institutions	**Top-40 liberal arts colleges**	**Flagship public institutions**
Brown (99)	Swarthmore (06)	North Carolina (03)
Princeton (01)	Amherst (07)	Virginia (04)
Yale (05)	Davidson (07)	Tennessee (05)
Rice (05)		Minnesota (05)
Penn (05)		Michigan (06)
Stanford (06)		Florida (06)
Swarthmore (06)		Indiana (07)
MIT (06)		Maryland (07)
Amherst (07)		Washington (07)
Columbia (07)		Illinois (07)

Dartmouth College, Duke University, Georgetown University, Harvard University, Johns Hopkins University, MIT, Mount Holyoke College, Northwestern University, Oberlin College, Pomona College, Princeton University, Rice University, Smith College, Stanford University, Swarthmore College, Trinity College, University of Chicago, University of Pennsylvania, University of Rochester, Washington Univ. in St.Louis, Wellesley College, Wesleyan University, Williams College, and Yale University.

[7] As categorized by US News & World Report, the top forty liberal arts colleges can be found at A list of these institutions is available at www.usnews.com/sections/education.

[8] There are 75 flagship public institutions. A list of these institutions is available at www.usatoday.com/news/education/2006-08-30-tuition-survey_x.htm.

4 Enrollment response

In order to accommodate the policy implementation across multiple time periods, we set up the following difference-in-difference model with a full set of time-period indicators and a policy indicator defined to be unity for institutions and time periods that are subject to the policy. This obviously imposes the restriction that the estimated influence of the policy is independent of year, which we will later relax. As a general framework, then, we are interested in the estimate of β in the following model:

$$\ln(LIE_{it}) = \alpha_i + \gamma_t + \mathrm{T}_{it}\beta + \mathbf{x}_{it}\delta + \epsilon_{it}, \tag{1}$$

where LIE_{it} captures low-income enrollment at institution i in academic year t (which we measure as the number of Pell recipients), and T_{it} is the treatment variable, defined to be unity for institutions and time periods (i, t) that are subject to the treatment. The model has a full set of institution effects in α_i, and a full set of time effects in γ_t. As institution fixed effects will not account for other time-varying factors that influence LIE_{it}, we include the log-cost of attendance (e.g., tuition, fees, etc.) and the log-population of first-year low-income students enrolled in four-year institutions in $\mathbf{x}_{it}$. Institution-specific errors are captured in ϵ_{it}.

In Table 2 we report the results of three separate empirical tests of the efficacy of targeted-aid programs as measured by the enrollment of low-income students. With respect to our key variable of interest, in all three tests (i.e., separately within the groups of COFHE institutions, the top-40 liberal arts colleges and the public flagship institutions), point estimates suggest that targeting low-income students has benefited students in terms of enrollment. However, other than in the case of flagship institutions, traditional confidence intervals include zero. In terms of economic significance, note that the estimated coefficient on the treatment variable of Column (3) suggests that the relative increase in enrollment of low-income students at the flagship institutions adopting aid reform as described above is roughly 98 students (at the mean enrollment level).[9]

While not the focus of our investigation, we note that low-income enrollments at liberal arts colleges are positively associated with costs of attendance, with an elasticity of roughly 0.6 suggesting that a 10-percent increase in costs yields a 3-student increase in low-income enrollment at the mean

[9]Mean low-income enrollment at flagship institutions in 1999 is 753 students.

enrollment level.[10] The positive elasticity suggests that it may be difficult to separate price effects from the correlation of price and quality (either real or perceived, since time-invariant institutional heterogeneity is absorbed in the error structure of the model). We also note that the population of low-income students attending four-year institutions has the expected sign, and magnitudes that seem quite plausible given the selectivity of the institutions within our sample (i.e., elasticities of roughly 0.01).

Table 2: Enrollment responses to targeted financial aid

The dependent variable is equal to the log of low-income enrollment (which we measure as the number of Pell recipients) at institution i in academic year t. All specifications include a full set of institution effects and a full set of time effects, over annual institution-level observations.

	COFHE institutions (1)	**Top-40 liberal arts colleges** (2)	**Flagship public institutions** (3)
Treatment period (i.e., targeted aid)	0.085	0.117	0.123*
	(0.099)	(0.152)	(0.068)
ln(Cost of attendance)	-0.030	0.608**	-0.138
	(0.269)	(0.262)	(0.127)
ln(Population of Pell students at 4-yr inst.)[a]	0.017	0.008*	0.014***
	(0.015)	(0.005)	(0.004)
Constant	4.042	-2.843	7.131***
	(2.858)	(2.721)	(1.199)
Institution fixed effect	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes
Observations	270	331	648
Unique institutions	30	37	72
R^2	.05	.05	.07

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses

[a] measured in thousands of students.

[10]Mean low-income enrollment at liberal-arts institutions in 1999 is 47 students.

Given the discontinuities in costs of attendance (i.e., tuition) at flagship institutions corresponding to state borders, in Table 3 we separately estimate the enrollment of low-income students at flagship public institutions by residency status. Doing so reveals that the meaningful pattern behind the above significance is the responsiveness of flagship enrollments to the aid regime, as relaxed specifications points to a significant response only in the enrollment of in-state, low-income students. While the point estimate remains positive, the estimated coefficient on the treatment variable of Column (2) is not outside of standard confidence intervals. By this we conclude that these need-based policies may be effective at attracting needy in-state students to the state flagship institution (and away from other now-more-costly in-state institutions?). The estimated coefficient from the model of in-state, low-income enrollment has the implication that treatment yields, on average, a 104-student increase in in-state low-income enrollment over those flagships not adopting similar programs.[11]

[11] Mean in-state, low-income enrollment at flagship institutions in 1999 is 620 students.

Table 3: Flagship institutions' enrollment responses to targetted financial aid

The dependent variable is equal to the log of low-income enrollment (which we measure as the number of Pell recipients) at institution i in academic year t. All specifications include a full set of institution effects and a full set of time effects, over annual institution-level observations.

	In-state enrollment (1)	**Out-of-state enrollment** (2)
Treatment period	0.156**	0.0552
(i.e., targeted aid)	(0.0693)	(0.0857)
ln(Cost of attendance)	-0.276**	0.400**
	(0.130)	(0.161)
ln(Population of Pell	0.0128***	0.0238***
students at 4-yr inst.)[a]	(0.00389)	(0.00481)
Constant	8.241***	-0.0979
	(1.227)	(1.517)
Institution fixed effect	Yes	Yes
Year fixed effect	Yes	Yes
Observations	648	648
Unique institutions	72	72
R^2	.08	.07

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses

[a] measured in thousands of students.

Table 3 also reveals the importance of residency status in considering the elasticity of enrollment to costs of attendance, as in-state student enrollment now falls significantly as costs increase, with an elasticity of roughly -0.3 suggesting that a 10-percent increase in costs yields a 17-student decrease in low-income enrollment at the mean in-state enrollment level in the sample.[12] Out-of-state low-income enrollment again rises as costs increase, with the same 10-percent increase in tuition yielding a 5-student increase in

[12]Mean in-state low-income enrollment at flagship institutions in 1999 is 620 students.

low-income enrollment at the mean out-of-state enrollment level in the sample.[13] Again, here we suspect that it may be difficult to separate price effects from the perceived time-series correlation of price and quality (which is not absorbed into the error structure if time varying).

5 Basins of attraction

5.1 Mean distance

In order to accommodate the policy implementation across multiple time periods, we set up the following difference-in-difference model with a full set of time-period indicators and a policy indicator defined to be unity for institutions and time periods that are subject to the policy. This obviously imposes the restriction that the policy has the same effect in every year, which we will later relax.

As a general framework, we are interested in the estimate of β in the following model:

$$\ln(Distance_{it}) = \alpha_i + \gamma_t + \mathrm{T}_{it}\beta + \mathbf{x}_{it}\delta + \epsilon_{it}, \tag{2}$$

where $Distance_{it}$ captures the mean distance (km) traveled by enrolling low-income enrollment (i.e., Pell recipients) at institution i in academic year t, and T_{it} is the treatment variable, defined to be unity for institutions and time periods that are subject to the treatment. As in the enrollment model, the distance models include a full set of institution effects in α_i, and a full set of time effects in γ_t, the log-cost of attendance (e.g., tuition, fees, etc.) and the log-population of first-year low-income students enrolled in four-year institutions in $\mathbf{x}_{it}$. Institution-specific errors are captured in ϵ_{it}.

In Table 4 we report the results of three separate empirical tests of the efficacy of targeted-aid programs in terms of the mean distance traveled by low-income students to the treated institutions relative to control institutions. In all three tests, point estimates suggest that targeting low-income students has increased the geographic reach of institutions in enrolling low-income students. However, in no case are we confident in such an interpretation of our results as confidence intervals on the key variable of interest include zero.

[13] Mean in-state low-income enrollment at flagship institutions in 1999 is 132 students.

Table 4: Distance responses to targeted financial aid

The dependent variable is equal to the log of the mean distance (km) traveled to each institution i by enrolling low-income students in academic year t. All specifications include a full set of institution effects and a full set of time effects, over annual institution-level observations.

	COFHE institutions (1)	**Top-40 liberal arts colleges** (2)	**Flagship public institutions** (3)
Treatment period	0.058	0.049	0.005
(i.e., targeted aid)	(0.055)	(0.165)	(0.040)
ln(Cost of attendance)	-0.381**	-0.045	0.165**
	(0.148)	(0.284)	(0.075)
ln(Population of Pell	0.031***	0.017***	0.007***
students at 4-yr inst.)[a]	(0.008)	(0.049)	(0.002)
Constant	9.777***	6.410**	3.696***
	(1.578)	(2.953)	(0.711)
Institution fixed effect	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes
Observations	270	331	648
Unique institutions	30	37	72
R^2	.21	.14	.17

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses

[a] measured in thousands of students.

5.2 Nonparametric analysis: Kernel densities

While somewhat instructive, querying only the mean distance traveled by low-income students is a rather blunt approach to investigating the efficacy of the income-targetting programs in our sample period. This is particularly the case with targeted-aid programs that are explicitly designed to address access issues at the tail of the income distribution and where issues of diversity (that can include geographic proximity) are of paramount concern. Thus, our objective here will be to to analyze changes in the *distributions* of geographic distance between each students home address and the institution in which the student enrolls as a first-time freshman before and after a policy change, and to determine whether the changes in the distributions differ for institutions with changes in targeted financial aid policy. The basic tool for this descriptive analysis is a nonparametric estimator of the underlying density function. Before doing so, we normalize distances in order to ensure that the comparisons are made from the same relative distance. This normalization amounts to dividing the distance between the home address of each student enrolling in a given institution in a given academic year by the mean distance traveled by all students to that institution in a base year (e.g., 1999). Thus, in both the time series and cross section, we are able to make comparisons that net out the level differences in distances traveled by students to institutions. Prior to kernel estimation, we also net out the idiosyncratic component of students' distances traveled by regressing distance traveled on student EFC, state of origin, and several attributes associated with their places of origin.[14] Doing so eliminates, in part, the systematic variation in students' distances traveled that is exogenous to institutions.

Using x_B to represent the normalized distance before the policy change, the kernel density function estimate at a target value x is

$$\hat{f}_B(x) = \frac{1}{nh} \sum_{i=1}^{n} K\left(\frac{x_{Bi} - x}{h}\right), \tag{3}$$

where n is the number of observations and K is the assumed kernel function. As our sample spans 1999 through 2007, the earliest time that can capture the "before" regime in equation (3) is 1999. The final year of our sample is 2007. Given the prevalence of 2007 initiations, the "after" regime will be represented by the 2007 academic year. Similarly, then, the density at the target value x after the policy change is

$$\hat{f}_A(x) = \frac{1}{nh} \sum_{i=1}^{n} K\left(\frac{x_{Ai} - x}{h}\right). \tag{4}$$

While the year of introduction of targeted financial aid varies across institutions which launch such programs, in no case does an institution revert to previous policy. That is, once an institution has declared a move toward

[14]This includes population, percent white, percent black, percent hispanic, percent with college degree, median household income and per-capita household income.

meeting the needs of low-income students they do not backtrack on the stated policy. Thus, one approach to the question of whether there are systematic changes to the basins of attraction exhibited by these institutions is to make comparisons between the earliest year available (i.e., 1999) and the latest year available (i.e., 2007).

The change in the density between "before" and "after" regimes is simply

$$\Delta(x) = \hat{f}_B(x) - \hat{f}_A(x). \tag{5}$$

The difference between the changes in densities across treatment (i.e., T) and control (i.e., C) institutions is then

$$\Psi_{\mathrm{TC}}(x) = \Delta_{\mathrm{T}}(x) - \Delta_{\mathrm{C}}(x). \tag{6}$$

We use a Gaussian kernel for all calculations: $K(u) = \varphi(u)$, where φ is the standard normal density function. We then calculate the density functions at 400 equally spaced alternative values of x, and then use graphs to summarize the results.[15]

Confidence intervals can be calculated easily for the estimated density functions, given by equations (3) and (4). Following Silverman (1986) or Pagan and Ullah (1999), the 95-percent confidence interval for an estimated density at target point x is

$$\hat{f}(x) \pm 1.96(nh)^{-1/2}\left[f(x)\int K^2(\Psi)d\Psi\right]^{1/2}. \tag{7}$$

For the Gaussian kernel, $K^2(\Psi)d\Psi = 0.2821$. Though the analytic standard errors are easy to calculate for $\hat{f}(x)$, they are more complex for the differences in densities because they require an estimate of covariance terms.[16] Thus, we use a simple bootstrap algorithm to construct our standard-error

[15] The kernel density function is the same conceptually as a smoothed histogram. The degree of smoothing is controlled by the bandwidth, h. Following Silverman (1986), we use a simple rule of thumb to determine the bandwidths: $h = 1.06 var(x) n^{-.20}$. With three categories of states and two time periods, this rule of thumb implies six alternative bandwidths. Though wider bandwidths tend to yield a higher degree of monotonicity and smoother distributions, experimentation with alternative bandwidths produced only minor variation in the appearance of the estimated density functions. We use the average of the six values of h as the bandwidth for all calculations.

[16] The formula for the variance is $var(\hat{\Delta}(x)) = var(\hat{f}_B(x)) + var(\hat{f}_B(x)) - 2cov(\hat{f}_B(x), \hat{f}_A(x))$.

estimates. We draw with replacement from each regimes series before and after the policy change, and re-calculate the density functions (equations 3 and 4) and their changes (Equation 5). After repeating this process for 100 replications, the bootstrap standard error estimates are simply the sample standard deviations of the 100 estimates.[17] The bootstrap estimator provides estimates of $var(\hat{f}_B(x))$, $var(\hat{f}_A(x))$, and $var(\hat{\Delta}(x))$ for each group of states. Following these calculations, the variance for the differences between density changes is simply $var(\hat{\Psi}_{\mathrm{TC}}(x)) = var(\hat{\Delta}_{\mathrm{T}}(x)) + var(\hat{\Delta}_{\mathrm{C}}(x))$ because by construction states are in the same regime for all periods, which results in the $cov(\hat{\Delta}_{\mathrm{T}}(x), \hat{\Delta}_{\mathrm{C}}(x)) = 0$.

5.3 Nonparametric analysis: Difference in density differences by targeted-aid regime

Recall our earlier discussion of treatment and control groups defined around the sets of institutions which introduced various initiatives within our sample period (see Table 1). Here, we will again follow the three tests, the general definitions being initiatives 1) within COFHE institutions, 2) within the top-40 liberal arts colleges, and, 3) within the set of flagship public institutions.

In Table 5, we report the normalized average distances traveled to institutions in the first and last years available. While the analysis of Section 5.1 points to no significant difference in mean distances traveled around the policy, the distance distributions do shift. Given our three tests, in Table 5 we provide these statistics broken down by the groupings defined above. In terms of general tendency, distance traveled by students increased on average over 1999 to 2007 increased at COFHE institutions, and at the nation's best liberal-arts colleges. However, these broad trends differ at flagship public institutions, where mean distance has fallen over the same interval at both control and treated institutions. These statistics are consistent with having defined control groups in a reasonable way.

[17]The bootstrapped standard-error estimates are virtually identical to the analytic errors for the estimated density functions.

Table 5: Mean normalized distances traveled to institutions, by group

Group		1999	2007
COFHE institutions	Control	1.000	1.038
		(1.067)	(1.124)
	Treated	1.000	1.073
		(1.035)	(0.972)
Top-40 liberal arts institutions	Control	1.000	1.690
		(1.301)	(1.663)
	Treated	1.000	1.474
		(1.171)	(1.023)
Flagship institutions	Control	1.000	0.934
		(0.429)	(0.413)
	Treated	1.000	0.952
		(0.358)	(0.352)

Standard deviations in parentheses

With respect to the COFHE institutions and liberal-arts colleges there are no discontinuities in tuition corresponding to state borders as there are with the public flagship institutions we analyze herein. Thus, in any analysis of distances traveled by low-income students to institutions one must pay some attention to the possible transfer of tuition discontinuity into observed distances traveled. Thus, in Table 6 we report the breakdown of distances traveled to flagship institutions by each student's residency status. Comparing 1999 and 2007 classes, there are clear differences in the behaviour of in-state students (who pay heavily subsidized tuitions) and out-of-state students (who face much higher tuition levels) with respect to distance traveled. In fact, at both control and treated flagship institutions over the time interval considered, the institutions' basins of attraction seem to increase within their own states, yet decrease outside of their own states. Given this tendency, we separate our subsequent analysis of flagship institutions by this residency status.

Table 6: Mean normalized distances traveled to flagship institutions, by residency status

Group		1999	2007
Flagship institutions	Control	1.000	0.934
		(0.429)	(0.413)
	Treated	1.000	0.952
		(0.358)	(0.352)
In-state students only	Control	1.000	1.126
		(0.377)	(0.405)
	Treated	1.000	1.149
		(0.535)	(0.493)
Out-of-state students only	Control	1.000	0.912
		(0.508)	(0.456)
	Treated	1.000	0.931
		(0.441)	(0.410)

Standard deviations in parentheses

In each panel of Figure 1 we show the estimated density functions for distance traveled in 1999 and 2007 for each group of institutions and by treatment/control designation. The graphs generally suggest that distance traveled to institutions tended to shift right (in a distributional sense) over this interval of time for each group of institutions. The exception to this general pattern is apparent in the distribution of distance traveled to flagship institutions by out-of-state students (i.e., Panel D), where the distribution of distance seems to have changed in the opposite direction (i.e., suggesting that out-of-state markets are getting geographically "smaller"). The same information is shown in a different way in Figure 2, where we plot the 95-percent bootstrap confidence intervals for the changes in the estimated densities from 1999 to 2007 (again, for each group and separately for treatment and control institutions). In each group, there appears to be a change over time toward mass (in the distribution of distances traveled) being lost at close distances and being picked up farther away (i.e., at or beyond the mean distance of 1). Again we see the same exception in flagship institutions' ability to draw out-of-state students, where the distribution of distance traveled seems to pick up mass below the mean and lose mass at higher distances.

Interesting results emerge when we compare the change in densities before and after the policy change. Panel A in Figure 3 shows the 95-percent bootstrap confidence intervals for the difference in density differences for COFHE institutions that did and did not adopt need-based aid programs, i.e., $\Delta_{\mathrm{T}}(x) - \Delta_{\mathrm{C}}(x)$, where the function $\Delta(x)$ is given in equation (5). Relative to the non-treatment group of institutions, point estimates suggest that COFHE institutions which adopted new targeted-aid programs had a reduction in the mass of distances (roughly) below the standardized mean of 1 (i.e., 1,345km), with the mass being made up at distances above their historical means. While the differences below the mean are not statistically significant, the increase in mass above the is statistically significant. While most of the mass at COFHE institutions continues to be below the mean distance traveled, it reasonably follows that these COFHE institutions have increased their basins of attraction, on average, in drawing low-income students to campus.

Panel B in Figure 3 shows the 95-percent bootstrap confidence intervals for the difference in density differences for Pell students attending top-40 liberal arts institutions that did and did not adopt targeted need-based aid programs. Interestingly, Panel B shows that those that adopted these targeted need-based aid programs, while experiencing a similar (although more muted) reduction in the mass of distances at the tails and increase in the mass just above the normalized mean of 1 observed for the COFHE institutions, these difference in density differences are not significantly different from zero at any point. Thus, these aid targeted need-based aid programs appear to be relatively less effective at attracting needy students from more-distant locations to top-40 liberal arts colleges than to COFHE schools.

Panel C (D) in Figure 3 shows the 95-percent bootstrap confidence intervals for the difference in density differences for in-state (out-of-state) low-income students attending flagship public institutions that did and did not adopt targeted need-based aid programs. The results clearly show that these targeted aid programs had a distinctly different effect on the relative distanced traveled by in-state versus out-of-state low-income students at institutions adopting need-based aid reform. Specifically, the mass of in-state low-income students traveling approximately around the mean distance (i.e., 106.7km) appears to increase, arguably coming from just above the mean distance. this is consistent with in-state markets for low-income students tending toward being relatively smaller at aid-reformed flagship institutions.

For out-of-state students (i.e., Panel D) targeted need-based aid programs appear to increase the mass of students on either side of the mean distance (i.e., 995.3km) with a reduction in the mass of students attending from the mean distance. Precision being a factor, we hesitate to make strong conclusions from these patterns.

That the outcomes of these targeted-aid programs differ so distinctly with the type of institution being considered (and, by extension, the type of student) suggests that the effect of these aid programs on an institutions basin of attraction relates to the distance that certain types of student these institutions have historically been able to attract. For example, COFHE institutions – which have generally drawn from a more-distant pool of needy students than top-40 liberal arts colleges have – are more effective at using need-based aid programs to provide access to needy students from more-distant locales. Alternatively, similar forms of income-targeted need-based aid offered at flagship public institutions – whose primary mission is to serve the best and brightest from within their given states – may tend to reinforce pre-existing conditions, attracting more (in the distributional sense) "typical" in-state Pell students and fewer "typical" out-of-state Pell students. Although this paper does not directly address the general equilibrium effects of such programs on the placement of needy students, it follows that need-based aid programs such as those analyzed may lead to greater segregation of needy students based on their observed attributes.

6 Conclusion

We use unique individual data on all freshman Pell students between 1999 and 2007 that constitute essentially the population of poor students entering higher education over this period to examine the access effects of the introduction of targeted need-based aid programs at COFHE, top-40 liberal arts, and flagship public institutions. In particular, we use a series of difference-in-difference regressions to examine whether institutions that adopted targeted need-based aid programs experienced a significant increase in either the number of Pell students or distanced traveled to campus by their enrolling Pell students (i.e., a proxy measure for student access) relative to those same types of institutions that did not adopt such aid programs over the time period under consideration.

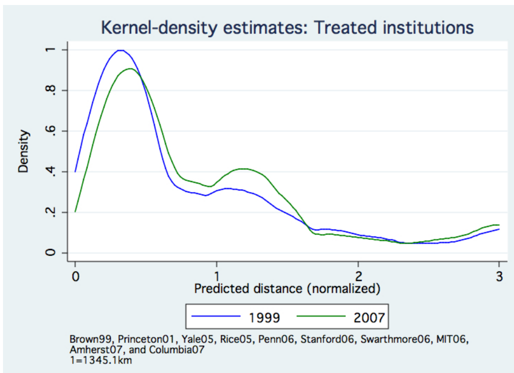
Nowhere do we find that targeted need-based aid programs decreased the enrollments of low-income students as point estimates in enrollment models are uniformly positive. Likewise, no parametric model of the distance traveled by low-income students to each institution suggests that reform has had a negative effect on matriculation patterns.

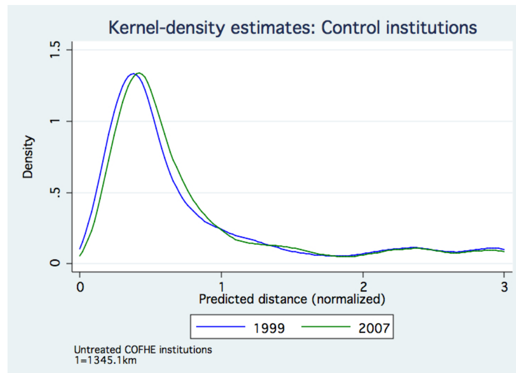
Large and significant increases in the enrollment of in-state low-income students appear at flagship public institutions with such aid reforms. Specifically, estimates suggest that on average, flagships adopting such reforms experience a large and significant 104-student increase in in-state low-income enrollment over those not adopting reforms. However, targeted-aid programs that are explicitly designed to address access issues at the lower tail of the income distribution and where issues of diversity (that can include geographic proximity) are of paramount concern, suggest that a focus on the mean distance traveled by low-income students may be an incomplete test of the efficacy of the income-targeting programs.

We subsequently adopt a non-parametric kernel density approach that examines the difference in the distribution of distances traveled by Pell students before and after the targeted aid programs are adopted and in comparison to similar institutions (i.e., other COFHE, top-40 liberal arts colleges, and flagship public institutions) that did not adopt a targeted aid program under the period of consideration. This non-parametric analysis of institutions' basins of attraction reveal significant distributional effects of these income targeted aid programs at certain institutions, namely, aid-reforming COFHE institutions. While the observed decreases below the historical average-distance traveled are not statistically significant, their is an increase in the mass above the historical mean that is statistically significant, suggesting that the COFHE institutions adopting aid reforms as described above have tended to increase their basins of attraction in drawing low-income students to campus.

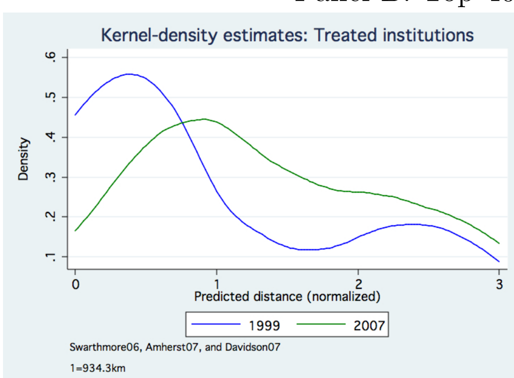
Figure 1: Kernel-density estimates over distances traveled, by group

Panel A: COFHE institutions





Panel B: Top-40 liberal arts colleges



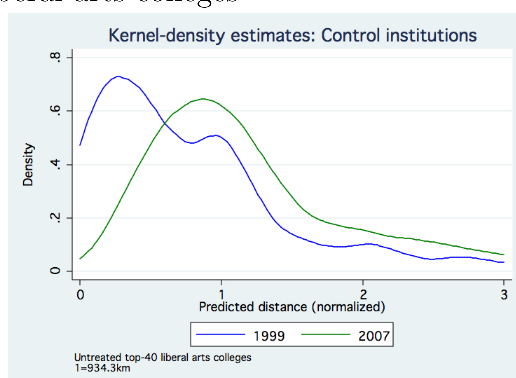


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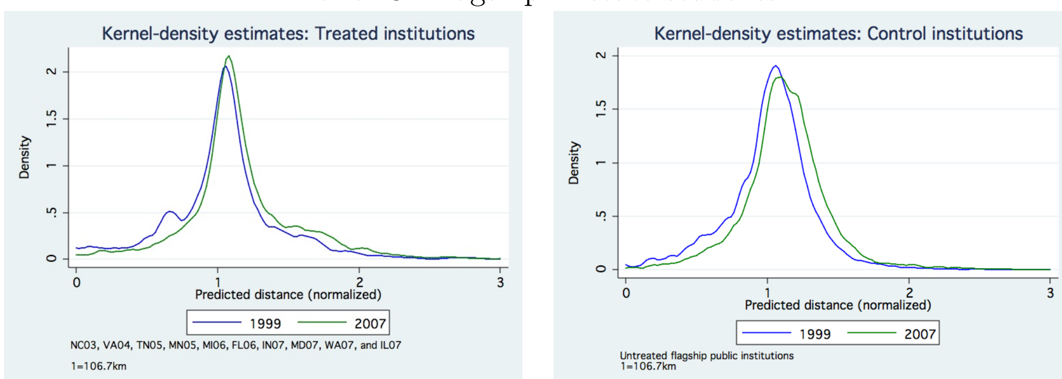
Figure 1: ... continued

Panel C: Flagship in-state students



Panel D: Flagship out-of-state students

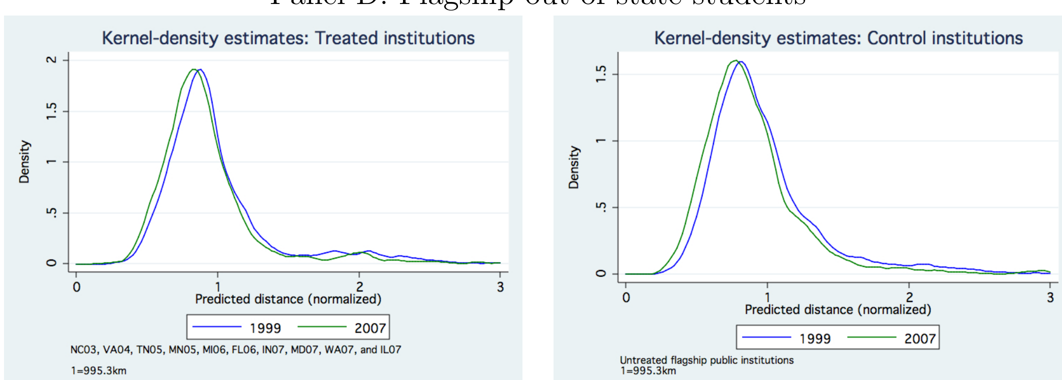
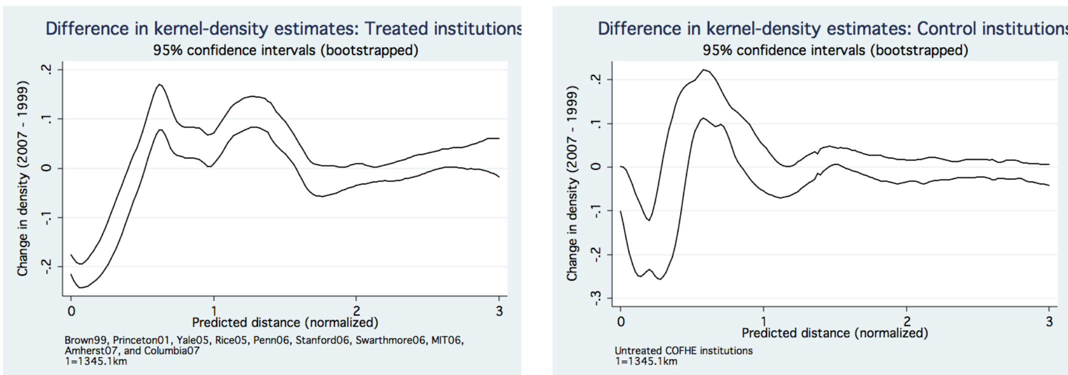


Figure 2: Difference in kernel-density estimates, by group

Panel A: COFHE institutions



Panel B: Top-40 liberal arts colleges

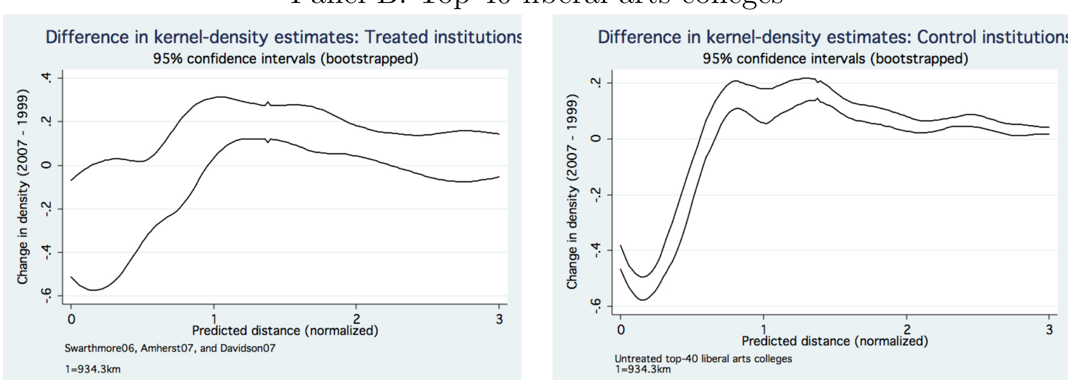


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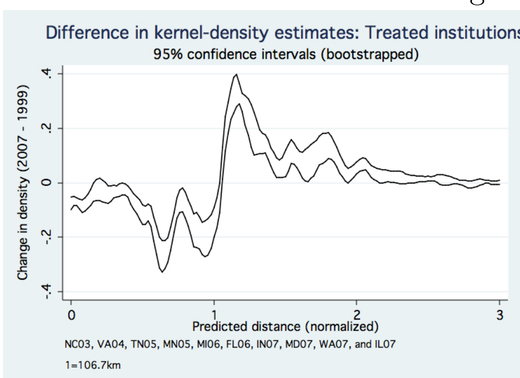
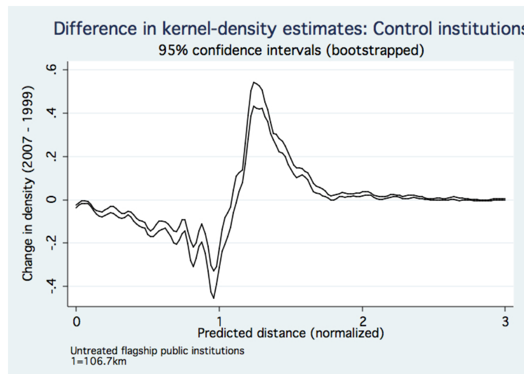
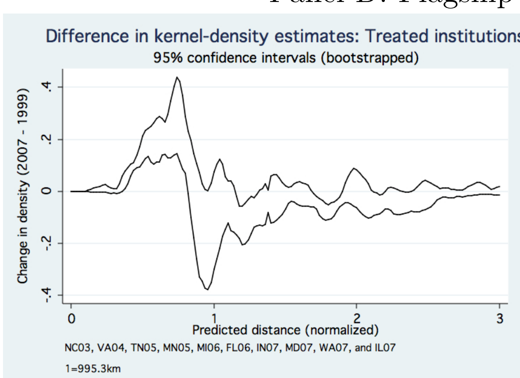
Figure 2: ... continued

Panel C: Flagship in-state students





Panel D: Flagship out-of-state students



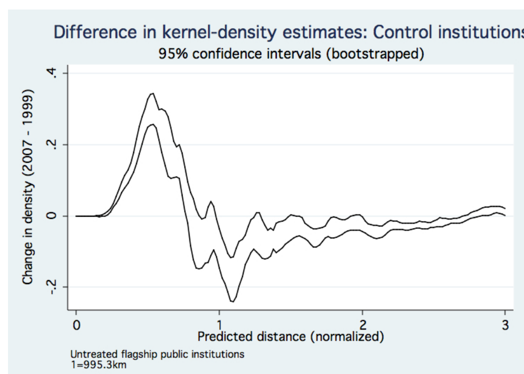
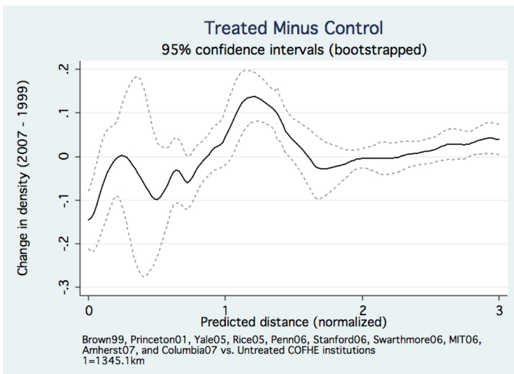
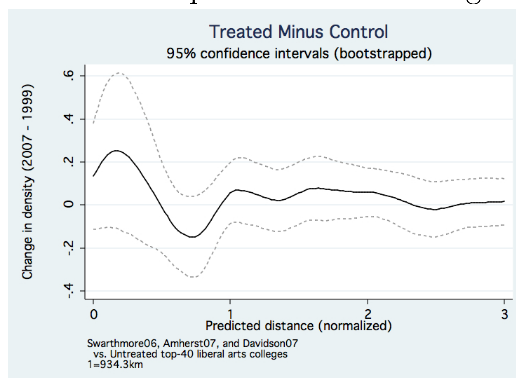


Figure 3: Difference in kernel-density estimates, by group

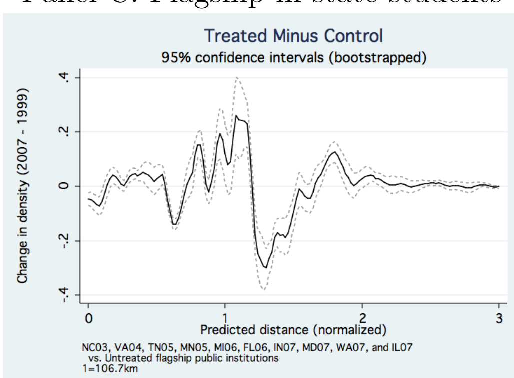
Panel A: COFHE institutions



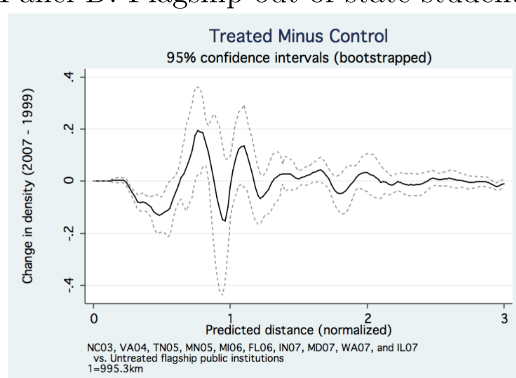
Panel B: Top-40 liberal art colleges



Panel C: Flagship in-state students



Panel D: Flagship out-of-state students



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