Economics of Education Review 30 (2011) 203–214

Contents lists available at ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

Do no-loan policies change the matriculation patterns of low-income students?

Glen R. Waddell[a,b,*], Larry D. Singell Jr.[a]

[a] Department of Economics, University of Oregon, Eugene, OR 97403-1285, USA
[b] IZA, Bonn, Germany

ARTICLE INFO

Article history:
Received 13 May 2010
Received in revised form 25 October 2010
Accepted 26 October 2010

JEL classification:
I23
I21
J24

Keywords:
Low income
Financial aid
No loan
Pell

ABSTRACT

We examine whether there is discernable variation in the matriculation patterns of low-income students at public flagship institutions around changes in institutional financial-aid policies that target resident, low-income students with need-based aid. Overall, our results suggests that need is not being met on the extensive margin and that enrollment levels actually fall with the introduction of targeted aid. However, the enrollments of more-needy students tend to fall less and students matriculating to aid-innovating institutions tend to have more financial need after the introduction of income-targeted aid. This suggests that along the intensive margin income-targeted aid may still be benefiting the most needy. We also find that institutions that introduce income-targeted aid subsequently enroll more-geographically distant students, suggestive of improved matching.

Published by Elsevier Ltd.

1. Introduction

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 in the last three decades but has widened (e.g., Ellwood & Kane, 2000; Havemen & Wilson, 2007). As such, it is important that we understand the underlying patterns of matriculation and any potential sensitivity to policy aimed at narrowing this gap. With student-level data for the universe of Pell Grant recipients from 1999 to 2007, we build on existing literature by examining along several dimensions of access whether income-targeted institutional-aid programs influence the matriculation patterns of low-income students. In particular, we analyze the introduction of "no-loan" programs at public flagship institutions whereby anticipated borrowing (i.e., student loans) are replaced with institutional grants to a degree sufficient to cover tuition, fees, room and board.

Public flagships represent an important gateway into higher education for many low-income students and the Pell Grant — with \$13.7 billion of appropriations in 2007/2008 — is the largest source of need-based aid available to students.[1] Yet, need-based aid programs have grown, in part, out of a recognition that the rising real cost of college and student debt levels have potentially threatened the access of low-income students to college. For example, while real tuition and fees increased by 43 (63) percent at public (private) universities between 1993 and 2005 (*Trends in College Pricing 2005*, The College Board,

* Corresponding author at: Department of Economics, University of Oregon, Eugene, OR 97403-1285, USA.
E-mail address: waddell@uoregon.edu (G.R. Waddell).

[1] Approximately 25 percent of all college going students in 2007/2008 received financial assistance through Pell Grants (*Source*: Federal Pell Grant End-of-Year Report 2007–2008, U.S. Department of Education, Washington, D.C.).

0272-7757/\$ – see front matter. Published by Elsevier Ltd.
doi:10.1016/j.econedurev.2010.10.004

Table A1), the real wage premium rose by 27 percent (Mishel, Bernstein, & Allegretto, 2007). Consistent with tightening financial constraints, Snyder, Tan, and Hoffman (2006) show that students are now more likely to require student aid to attend college, that students are covering a smaller portion of their college costs with grants, and, that they are taking out nearly twice the level of debt in real terms than in the previous decade (e.g., over \$20,000 in 2004). Partly because federal need-based aid programs have not kept pace with rising costs (McPherson & Schapiro, 1998), a growing number of institutions and states have moved toward providing need-based aid.

In our consideration of the efficacy of income-targeted aid initiatives, we consider three measures of efficacy. First, we consider efficacy as measured in terms of institutional enrollments. Establishing the nature of any link between these programs and enrollment is itself nontrivial, as the extant literature suggests that this population of students can be quite inelastic with respect to aid (e.g., Curs, Singell, & Waddell, 2007). Where conditions allow for a direct efficacy test we find no significant enrollment responses to the policy innovations overall, although the enrollment of students with less relative need does decrease at treated institutions. Broadly, our results suggest that institutions may have focused increases in aid on their most-needy students, when faced with constraints on the number of aid awards available (and ultimately the number admitted needy students).

Second, we consider efficacy as evidenced by the realized need of matriculating students, using the expected family contributions (*EFC*) as a measure of family income. In general, *EFC* represents the amount the applicant-student and/or the student's family can be expected to contribute toward financing the degree being sought. This expected contribution is estimated from information regarding family income, allowances against this income, the number of children, and family assets. To focus on the typical college-age student, our analysis uses only dependent students who by definition rely on parental contributions to attend college. We find that these aid innovations have lead to a relative shift toward enrolling low-income Pell students. That those who matriculate to aid-innovating institutions after the introduction of targeted-aid programs are more needy is consistent with the enrollment responses favoring more-needy students.

Third, we consider efficacy as evidenced in institutions' geographic basins of attraction — whether these policies change the geographic dispersion of matriculating low-income students. As other costs are unchanging over the period of our analysis, we contend that any increase in an institution's basin of attraction represents an improved ability to draw in low-income students. As an objective of these policies is to facilitate the matching of needy students with institutions, any increase in basin of attraction is consistent with prior constraints on the matriculation of low-income students being relaxed. We show that these "basins of attraction" do change, with needy students drawn to aid-innovating institutions from farther distances after the introduction of "no loan" policies. We interpret this as suggestive that aid can relax geographic barriers to the enrollment of low-income students. While evaluating the match itself remains outside our purview, we expect that such movement is in the direction of improving the potential matching of students with institutions.

In the following section we discuss the related literature and motivate our analysis. In Section 3 we set up our empirical test given the changes to financial-aid policy that fall within the period of data availability. In the process of doing so, we address our identification strategy and the way in which one should consider "treatment" and "control" groups for the purpose of the analysis. In defining this test, we will also address why certain alternative specifications (e.g., conducting equivalent analyses for private institutions) are inappropriate. We also introduce the data used in our analysis in Section 3. We then devote separate sections to each of the three relationships we examine; enrollment in Section 4, financial need in Section 5, and basins of attraction in Section 6. We then offer concluding remarks and discuss several important questions that remain unanswered.

2. Literature

Much of the recent work examining student responses to aid has risen out of natural experiments in federal, state, and institutional-aid programs (e.g., Angrist, 1993; Bound & Turner, 2002; Cornwell, Mustard, & Sridhar, 2006; Long, 2004). Among them, there are several examples where general aid initiatives (i.e., not income-targeted) have been used to consider the matriculation of low-income students (e.g., Dynarski, 2003; Hansen, 1983; Kane, 1994).

For example, Kane (2007) analyzes the D.C. Tuition Assistance Grant Program (DCTAG), which was made available to all residents of the District of Columbia regardless of income, and finds that the number of first-time, freshman Pell applicants and recipients from D.C. increased between 1999 and 2001, around the time of the 1999 introduction of DCTAG. However, Pell administrative data show that 41 of the 50 states experienced increases in the number of Pell recipients over this period, which suggests that the increases in D.C. may not be attributable to the policy alone. Kane (2007) does report that the increases in D.C. "were considerably larger than the changes observed in Maryland or Virginia." He also finds that the take-up rate associated with DCTAG is relatively stable across neighborhood-level income deciles, suggesting that responsiveness is not income dependent.

Using SAT data and a controlled design, Abraham and Clark (2006) also analyze the DCTAG, reporting that applications and enrollment rates at eligible institutions rise with the program relative to a control group. This increase is also shown to be primarily at less-selective institutions, which is consistent with Kane (2007), who reports that the enrollment impact was largest at nonselective public four-year colleges and, in particular, at predominantly black institutions.

Georgia's HOPE Scholarship, which was also more general in application than the initiatives we study here, had eligibility requirements that varied by income. As such, Dynarski (2000) considers the potential influence of aid adjustments on low-income populations by exploiting this variation in eligibility. She finds that college enrollment among those from higher-income families (i.e., above $50,000) responds to the scholarship while that from lower-income families does not. In a second paper, Dynarski (2004a) also finds that "HOPE had a substantially greater effect on white attendance than black and Hispanic attendance," which she attributes to the lower average incomes in these groups. Georgia's HOPE Scholarship, however, had an explicit merit component, thus confounding any differential responsiveness across income and ability measures. Likewise, studying California's Cal-Grant financial-aid program, Kane (2003) exploits variation in college price due to merit-based financial aid, and finds large impacts of grant eligibility, although these findings are most appropriately generalized to high-performing students. In particular, findings such as these may not generalize to the broader low-income population.

There are several studies that look at the Pell Grant specifically, recognizing the prominence of the Pell Grant in the federal financial aid system. Using variation in the Pell-eligibility formula in the late 1980s, Seftor and Turner (2002) report increased access for non-traditional students around the increase in generosity of the program for financially independent students. Singell, Waddell, and Curs (2006) use institutional level data to show that the HOPE scholarship increased the relative number of Pell students attending institutions in Georgia, particularly at less-selective four-year institutions.

Last, consider two recent studies that directly examine the efficacy of need-based aid initiatives that are similar to what we study but targeted at academically able at elite institutions. 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 to low-income students. Using estimated family incomes for plausible Harvard applicants, they find that HFAI attracted a larger and slightly poorer pool of applicants who, once admitted, enrolled at rates similar to the prior year. This is consistent with HFAI being effective at recruiting low-income students out of some otherwise-untapped supply of qualified students who would not have applied to Harvard in the absence of such aid.[2] Second, Linsenmeier, Rosen, and Rouse (2006) examine the enrollment effects of changes in financial aid policies (i.e., replacing loans with grants) at an anonymous institution (but clearly selective given its attributes). They report that matriculation by low-income students rose insignificantly in response to the introduction of the no-loan program, but that low-income minority students were more responsive, with matriculation rates rising some 8–10 percent. Again, however, these findings are most appropriately generalized to high-performing students and not the general low-income population.

The prospect of encouraging low-income students to matriculate to the top U.S. institutions is an important consideration. However, analyzing the enrollment patterns of low-income students at these elite institutions is arguably to consider a very special case from among the population of low-income students engaging with post-secondary education. In fact, our data indicate that of the 384,386 first-year Pell students attending four-year institutions in 2000, roughly 0.3 percent enrolled at Ivy League institutions. In the same year, 15.5 percent are found at public flagships, some 49-times more. The U.S. Department of Education reports that for incoming first-year undergraduates in 2000, Harvard University enrolled 108 Pell students (of 1600) and Princeton University enrolled 50 (of 1250). At the same time, the average public flagship enrolled roughly 750 first-time Pell students. The Anchorage campus of the University of Alaska, while enrolling fewer first-time Pell students than any other state flagship in 2000, still enrolled twice as many Pell students (i.e., 128) as Princeton. With our focus on a wider set of institutions that are more relevant to low-income students, the external validity of our findings is less a concern. Not surprisingly, Pell students who enroll at public flagships also tend to originate from areas with lower income and lower college-going rates.[3] Our focus yields a much wider set of institutions that serve a far greater number and proportion of low income students.

3. Identification

3.1. Why public flagships?

While there are thirty targeted aid programs introduced over the 1999 through 2007 period, the conditions that yield a valid empirical test requires that we exploit only a subset: those programs initiated at public institutions that limited potential beneficiaries to their own state residents. Namely, we exploit such policies at Minnesota (introduced in 2005), Tennessee (2005), Michigan (2006), Florida (2006), Indiana (2007), Washington (2007), and Illinois (2007). The rationale for this group defining our identification strategy is revealed in considering the actual underlying experiment.[4]

[2] Rothstein and Rouse (2007) also studies a need-based aid initiative, although focusing on debt accumulation and employment outcomes as a result of the initiative. Using data for a selective university that adopted a no-loan policy under which the loan component of financial aid awards was replaced with grants, they assess the causal effect of student debt on employment outcomes, reporting that debt affects students' academic decisions during college and can be associated with graduates choosing higher-salary jobs and not choosing poorly paid "public-interest" jobs. Suggestive evidence is also provided that credit constraints and debt aversion interfere with students' abilities to optimize over the life cycle. For example, debt is found to reduce students' donations to the institution in the years after they graduate and increases the likelihood that graduates default on pledges made in their senior years.

[3] Our data indicate that the typical Pell student at a public flagship originates from a ZIP code with per-capita income of $20,830 and with 16 percent of adult residents having college degrees, while those at Ivy League schools are coming from ZIP codes with $24,815 and with 18 percent of adult residents having college degrees, on average (*Source*: 2000 U.S. Census).

[4] Among income-targeted programs, there are four typical allocation mechanisms. Given our sample, and with the exception of the University of Minnesota, we are considering "no-loan" policies that eliminate or substantially reduce loans required by low-income students. (In 2005, Minnesota adopted a "Pell Grant match" policy that match the student's Federal Pell Grant.) The other types of mechanism include, "loan cap" policies that institute a low cap on student loans for low-income students (e.g., Brown), and "no parental contribution" policies that eliminate the parental contribution but retain the student contribution or the standard self-help level (e.g., Yale, Stanford).

In particular, note that treatment is not at the institution level but, rather, at the student level. By extension, where an institution does not limit the set of potential beneficiaries by some exogenous mechanism (e.g., state of residence) there is no well-defined group of comparable students against which one can measure the response to treatment. That is, where no low-income students are excluded from treatment the "control group" is empty.[5] As none of the private institutions have limited the scope of their initiatives by any exogenous measure (e.g., a student's state of residence), they will not contribute to our test. We also do not identify responsiveness at four public flagship institutions that introduced targeted aid but did not limit the potential recipients exogenously to in-state students.[6] In testing the efficacy of targeted-aid programs, then, our strategy exploits the existence of state borders and the practice of public universities offering income-targeted aid only to residents within their respective states. Specifically, we identify treatment through changes in the matriculation of treated residents (with respect to their own flagship) relative to the patterns exhibited by residents of states where a flagship institution did not offer income-targetted aid (with respect to their flagship).

To illustrate our strategy more specifically, consider a flagship public institution in state i that newly introduces a program of financial assistance to low-income residents in state i who matriculate to the state's flagship. Low-income residents of this state may or may not respond to the policy (which is what we aim to determine). However, as residents of state $j \neq i$ (i.e., nonresidents from i's perspective) are excluded from the institution's offer, we expect no direct response from j's residents.[7] In fact, residents of state j face the same relative margins in their matriculation decisions before and after the introduction of targeted aid in state i. Moreover, in the absence of the flagship institution in state j initiating a similar program, we would expect no behavioral response whatsoever from j's residents, toward their own flagship institution in particular. It follows that we adopt low-income resident of all states $j \neq i$ as a control group against which to measure the response of i's low-income students to the policy change at i's flagship. We will therefore make the claim that policies were effective only to the extent that i-students' patterns of matriculation changed with the policy innovation at i's flagship institution *relative to* what we patterns we observe in j-students' matriculation to j's flagship. In this way, the experiments we perform is a comparison of the matriculation patterns of low-income students when their own public flagship institution initiates a change in policy and the matriculation patterns of low-income students in states where no such initiative is adopted. For example, we measure whether Pell students in treated states (e.g., Michigan) behave differently after their flagship (e.g., the University of Michigan) introduces targeted aid by comparing their behavior to the matriculation patterns of Pell students in untreated states (e.g., Ohio) with respect to their own flagship institution (e.g., The Ohio State University), which did not introduce such a policy.

3.2. The role of nonresidents

Before we continue, note that the practice of distinguishing residents from nonresidents in the allocation of financial assistance would provide an opportune falsification exercise were student behavior the only contributor to nonresident enrollment (i.e., nonresident students do not fall under the direct treatment of the initiated policy changes at these flagship institutions). However, there may be unobserved changes in institutional policy toward needy students that are coincident with the aid initiative and influence the realized matriculation behavior of nonresidents. We report estimates for residents and nonresidents separately. As nonresident students are not being directly treated by the policy changes, any systematic difference in the realized matriculation patterns of nonresident students at treated flagship institutions may indicate that other unobserved institutional behaviors around the policy changes are having some influence on these outcomes. Any response in out-of-state, low-income enrollment could also reflect an institution-led substitution between in-state and out-of-state low-income students.[8]

3.3. 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, we use only those students who are recorded as first-time, dependent recipients in their first year at one of the 70 official state flagship institutions.[9] Independent students are dropped from the analysis as they are recognized by the literature as having

[5] As state flagships "treat" all resident students in the state, there is likewise no within-state experiment to be run. One could imagine a regression-discontinuity approach to the research question, using students around the margin of receiving financial support or around the margin of admission based on non-financial consideration. See van der Klaauw (2002) for an example of an RD approach to examining the relationship between financial aid and enrollment.

[6] These four are North Carolina (adopted in 2003), Virginia (2004), Michigan State (2006), and Maryland (2007). We drop these institutions and their associated students.

[7] Indirectly, they may appear to have responded if, for example, in-state enrollments rise sufficiently to crowd out nonresident students. However, to the extent that nonresidents (i.e., students who do not reside in i) are crowded out by any increase in resident matriculants to i, the bias will be in favor of finding no effect. For example, if students in i respond to i-specific policy and crowd out students from j, we would expect j's flagship to gain resident students *relative to* i's flagship institution.

[8] Note that to the extent residents of i (i.e., part of j's nonresident population) forgo an out-of-state alternative when they respond to i's policy (e.g., they forgo the flagship in state $j \neq i$), the bias will be in favor of finding relative differences in matriculation patterns between treatment and control. For example, as students in i respond to i-specific policy, j's flagship may well lose nonresident students *relative to* treated flagship institutions, which would yield a positive coefficient on the treatment variable in the nonresident model.

[9] Several states have multiple flagships. For example, California has two flagship institutions (in Berkeley and in Los Angeles), as does Texas (i.e., Austin and A&M), Colorado (i.e., Colorado-Boulder and Colorado State University), and several others. However, branch campuses are not included in this analysis.

different attributes and patterns of behavior (e.g., Seftor & Turner, 2002). We also drop student-level observations where we are unable to match them with an institution within the IPEDS dataset.[10]

To receive federal aid, a student must first complete a Free Application for Federal Student Aid (FAFSA) form, which provides aid administrators with the information needed to determine the size of an applicant's Pell Grant. 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., Dynarski, 2004b; Kane, 1994). 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 likely represent the most comprehensive collection of low-income students attending U.S. higher educational institutions.

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.

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

$$\ln(LIenroll_{it}) = \mathbf{x}_{it}\delta + T_{it}\beta + \alpha_i + \gamma_t + \epsilon_{it}, \quad (1)$$

where $LI(enroll_{it})$ captures the 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 includes a full set of institution effects, α_i, and a full set of time effects, γ_t. As institution fixed effects will not account for other time-varying factors that influence $LI(enroll_{it})$, $\mathbf{x}_{it}$ includes the log-cost of attendance (e.g., tuition, fees) and the state-level log-population of first-year, low-income students enrolled in four-year institutions within each state.[11] Institution-specific errors are captured in ϵ_{it} and are corrected for clustering at the institution level. An identifying assumption in the above model is that low-income students in treatment and control groups are on similar trends in low-income enrollment before the introduction of targeted aid. We find no evidence of differential trending.[12]

In Column (1) of Table 1 we report the estimated coefficients of Eq. (1). With respect to the treatment variable, the point estimate suggests that targeting low-income students has not increased the low-income enrollment levels among the treated. Given the degree of noise in the estimated coefficient, however, we conclude that while targeted-aid policies cannot be credited with attracting needy in-state students to state flagship institutions, there is insufficient evidence to claim that the policies have been detrimental. This is consistent with Linsenmeier et al. (2006), who find no significant overall enrollment effect associated with similar policy changes at an elite institution.

Given the possible heterogeneity that exists within the Pell population, in columns (2) and (3) we cut the data by whether students report having zero or positive *EFC* and consider the enrollments of these two groups separately. When identified separately, point estimates on the enrollment of Pell students with no expected family contribution — the most needy among Pell students — are positive but not significant. However, the enrollment of those with the least financial need falls at treated institutions. The negative enrollment response is inconsistent with the expected student response to treatment, suggesting that institutional factors may constrain the enrollment response of needy students. We see a similar pattern in nonresident enrollment around the policy changes, which we report in columns (5) and (6). Collectively, this is consistent with increased generosity limiting the ability of institutions to service as many needy students, particularly those with relatively less financial need.

Before continuing, note that Table 1 also highlights the importance of residency status in considering the elasticity of enrollment to costs of attendance. In fact, while the negative enrollment response to variation in costs among resident, low-income students is insignificant, non-resident enrollments increase significantly as costs increase, with the relatively less-needy nonresident students enrolling in significantly greater number. 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).[13] Of the population of needy students, the asym-

[10] This type of data issue would keep entire institutions from appearing in our final sample, so that we have the entire set of state flagships is consistent with having no missing students at these institutions. We note that the student counts provided in the "recipient files" of the Department of Education are a perfect match with the administrative records of our own institution, the University of Oregon, supporting a confidence in the data.

[11] The total enrollment of low-income students is intended to capture any time-varying supply shifts in the eligible populations from which the school draws students. Our results are not sensitive to whether we control for these patterns. Note, however, that if the treatment induces low-income students to attend on the extensive margin or substitute from two-year to four-year schools, one might worry that the estimated coefficient on this variable itself is biased upward, so interpreting point estimates on these controls should be done with some caution.

[12] The existence of differential pre-treatment trends, where such to exist, would introduce bias into the estimated treatment coefficient. For example, if resident, low-income enrollment was rising faster at treated flagship institutions then this methodology would falsely attribute any continuation of this trend to the targeted-aid programs themselves. Modeling ln ($LIenroll_{it}$) as cubic in t, we find no significant differences by treatment and control group in pre-treatment trends.

[13] Numerous empirical studies over the last 40 years have found a positive relationship between tuition and enrollment, particularly for non-resident students (e.g., Curs & Singell, 2002; Leslie & Brinkman, 1987; Wetzel, O'Toole, & Peterson, 1998). Prior work has explained that such an anomalous finding likely reflects the fact that students and their families regard tuition as an indicator of institutional quality that is correlated with the likely economic return to the investment and that this signal may be particular important for non-resident students who have less

Table 1
Institution-level enrollment of low-income students around the adoption of income-targeted aid.

	Residents			Nonresidents		
	$EFC \geq 0$	$EFC = 0$	$EFC > 0$	$EFC \geq 0$	$EFC = 0$	$EFC > 0$
Treatment period	−0.047 (0.048)	0.001 (0.059)	−0.083 * (0.048)	−0.181 ** (0.081)	−0.117 (0.096)	−0.225 ** (0.093)
ln(Cost of attendance)	−0.082 (0.271)	−0.120 (0.364)	−0.049 (0.227)	0.441 * (0.263)	0.277 (0.322)	0.507 ** (0.248)
ln(Resident population) [a]	1.054 *** (0.137)	0.818 *** (0.208)	1.093 *** (0.117)	0.267 (0.267)	0.128 (0.301)	0.296 (0.267)
ln(Nonresident population) [a]	2.420 (3.582)	−5.195 (4.438)	5.375 (3.591)	0.480 (9.838)	−0.464 (11.962)	0.065 (9.931)
Constant	−32.743 (46.489)	65.958 (58.012)	−71.614 (46.536)	−7.590 (127.493)	5.915 (154.950)	−3.450 (128.662)
Institution controls	Yes	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations/institutions	630/70	630/70	630/70	630/70	630/70	630/70
R^2	0.35	0.34	0.43	0.06	0.06	0.10
Mean enrollment	835.9	258.9	577.1	175.5	50.13	125.4

The dependent variable is equal to the log of low-income freshman enrollment (i.e., 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. Standard errors in parentheses, corrected for clustering at the institution level.

[a] Populations are of Pell students only.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table 2
Resident EFC around the adoption of income-targeted aid at flagship public institutions.

	Linear probability (1)	Conditional logit [a] (2)	Probit (3)	Tobit (4)
Treatment period	−0.021 * (0.011)	−0.031 * (0.016)	−0.019 * (0.011)	−0.195 (0.123)
ln(Cost of attendance)	0.028 (0.020)	0.043 (0.031)	0.026 (0.019)	0.308 (0.232)
ln(Resident Pell population)	−0.003 (0.047)	−0.015 (0.067)	0.009 (0.048)	−0.197 (0.488)
ln(Nonresident Pell population)	1.661 *** (0.602)	2.550 *** (0.815)	1.719 *** (0.651)	17.002 *** (6.360)
Constant	−21.126 ** (7.996)			−215.945 *** (83.470)
Institution fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations/institutions	526,488/70	526,488/70	526,488/70	526,488/70
	$F(12, 419975) = 148.5$	$\chi^2(12) = 510.5$	$\chi^2(81) = 13397.98$	$\chi^2(81) = 13869.63$

In columns (1)–(3), the dependent variable is equal to one if the student's expected family contribution (*EFC*) is positive (i.e., a family contribution is expected to be made). Estimated coefficients are reported in Column (1), estimated logit coefficients are reported in Column (2) and estimated probit marginal effects are reported in Column (3). In Column (4), the dependent variable is equal to student s's *EFC* at institution i in academic year t, continuous for positive values and left-censored at zero. All specifications include a full set of institution and time indicators. Standard errors in parentheses, corrected for clustering at the institution level.

[a] As discussed in the text, we exploit the equivalency of the Cox proportional hazard likelihood and the conditional logit likelihood in achieving convergence.
* $p < 0.1$
** $p < 0.05$.
*** $p < 0.01$.

metry by relative need is consistent with the relatively well off being more responsive to perceived quality. An alternative explanation is that where institutions enjoy excess demand from out-of-state students and are in search of higher revenues they admit more nonresident students while raising nonresident tuition. The state-level 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 1).

direct information for out-of-state schools (Brewer, Eide, & Ehrenberg, 1999; Zhang, 2007). The unique and changing nature of the higher education market has been well documented in the literature (Hoxby, 1997; Winston, 1999). Nonetheless, prior theoretical work has established that the demand for college should be downward sloping and predict that increases in institutional aid should raise the relative demand for the aid innovating institution (Rothschild & White, 1995).

5. Family income

As in the enrollment model above, our objective here is to allow the data to reveal any changes in expected family contributions (*EFC*) that systematically relate to the timing of "no-loan" policy implementation, using untreated students as "control" observations. With *EFC*, however, the implementation of such analysis is less straightforward, as *EFC* is determined formulaically by the FAFSA and is a function of student attributes (e.g., increasing in family income, number of dependent children in the student's family). As no family is expected to contribute negatively, we only observe the student-specific *EFC* after the application of a non-negativity constraint.

In Table 2, we provide several alternative specifications to transparently document that our results are not peculiar to a given empirical approach and to reveal to the extent possible the variation that exists in *EFC* around the introduction of these aid initiatives. In Column (1), we first report a linear probability model of the form

$$Prob(EFC_{sit} > 0) = \mathbf{x}_{sit}\delta + \mathrm{T}_{sit}\beta + \alpha_i + \gamma_t + \epsilon_{sit}, \quad (2)$$

where EFC_{sit} captures the expected family contribution of resident low-income student s at institution i in academic year t. In (2), T_{sit} is the treatment variable, defined to be unity for students and time periods that are subject to the treatment. Again, as in the enrollment model, we include a full set of institution indicators in α_i, a full set of time effects in γ_t, the log-cost of attendance (e.g., tuition, fees) and the log-population of first-year low-income students enrolled in four-year institutions in $\mathbf{x}_{sit}$. Student-level errors are captured in ϵ_{sit} and are corrected for clustering at the institution level. As was the case in Eq. (1), the identifying assumption of Eq. (2) requires that low-income students in treatment and control states trend similarly prior to the introduction of treatment. We again find no evidence of differential trending in the data.[14]

The dependent variable is set to unity for all positive *EFC* and is otherwise zero, yielding coefficients on the treatment variable that capture the marginal change in the propensity for a low-income student at a given institution to have at least some financial assistance expected from the student's family. The results of the baseline model in Column (1) indicate that around the introduction of targeted aid programs there is a significant decrease in the probability that matriculating students have the expectation of financial assistance from family. That is to say, conditional on enrollment, treated low-income students are roughly 3-percent less likely to have positive *EFC* (i.e., at the mean of .69).

In Column (2), we report the estimated coefficients of a model that is comparable to a conditional logit model, which bounds the projected probabilities to the unit interval. However, with so many observations in each group and multiple positive outcomes per group, estimating a conditional logit model is extremely taxing. We therefore exploit the equivalence of the conditional logistic regression likelihood function and the Cox proportional hazard likelihood function in Column (2).[15] Doing so again reveals that associated with the introduction of targeted aid programs, there is a significant decrease in the likelihood that students will matriculate with the expectation of family assistance. Conditional on enrollment, this specification suggests that treated low-income students are roughly 4-percent less likely to have positive *EFC* (i.e., at the mean of .69).

In Column (3) we report on a probit specification with institution-level indicator variables. While not yielding the unbiased estimation of parameters as in the Chamberlain procedure of Column (2), this alternative specification yields an estimate of the key parameter of interest that yields qualitatively similar results to those previously presented. As the treatment coefficient estimate does not substantively change across columns (1)–(3), we are inclined to suggest that neither the unboundedness of the OLS specification nor the potential bias from the incidental parameter problem in the probit specification are cause for concern. Moreover, neither the sign nor economic significance across these alternatives — treated students being roughly 2 percent less likely to have levels of family wealth that yield positive *EFC* — raise concern that our results are sensitive in any meaningful way.

Given the nature of the Pell Program's formulaic determination of need, *EFC* has a continuous component (which is increasing in family wealth, all else being equal) that has information of interest in considering the question of efficacy. However, the true expected family contribution is a latent variable, $EFC^\star$, such that $EFC_{sit} = \max(0, EFC^\star_{sit})$. In Column (4), we report the estimated coefficients of a Tobit-equivalent to the specification in Eq. (2). Here again we see evidence that targeted aid has drawn students of relatively lower means, with the estimated coefficient on the treatment variable implying a 19.5-percent decrease in the underlying family contribution (i.e., $EFC^\star$). This point estimate loses significance, however, which is not unexpected given the additional flexibility of the Tobit specification and the relative loss of students with positive *EFC* observed at treated institutions. With the enrollment analysis in mind, these results yield a consistent pattern — while need is not being met on the extensive margin (i.e., enrollments are falling), this response on the intensive margin is entirely consistent with income-targeted aid benefiting the most needy (i.e., enrollments of the relatively needy fall less and matriculating Pell students exhibit more need).

We report the results of the same set of specifications for nonresident students in Table 3, where we see a similar pattern in sign but find no significant differences in *EFC* coincident with targeted aid. Overall, given that the number of needy students does not increase at flagship institutions, which may be the result of capacity and/or budget constraints, these findings may be seen as somewhat provocative as they can imply that "improved" access may actually displace some (relatively wealthy) needy students in favor of others.

6. Basins of attraction

Avery et al. (2006), Linsenmeier et al. (2006) and Rothstein and Rouse (2007) suggest that the types of programs under analysis should permit a better matching of students with institutions. Here, we consider the geographic basins of attraction around each university in our sample of flagships — the area from which the institutions draw low-income students. In particular, we anticipate that to the extent credit constraints on low-income students are

[14] Modeling $Prob(EFC_{sit} > 0)$ as cubic in t, there are no differences by treatment and control group in pre-treatment trends.

[15] For example, the -clogit- command in Stata uses an exact calculation to handle multiple positive outcomes. Thus, if there are 5000 observations per group and 1000 positive outcomes within a group, -clogit- must calculate how many ways 1000 subjects can be drawn from a pool of 5000. $\prod_{k=4001}^{5000} k$ exceeds that which can be recorded, even in double precision. Our models fail to converge using Stata on a Linux box with 16 GB of RAM. In order to achieve convergence, then, we exploit that -stcox- can be used to estimate -clogit- models. While there is no reason to do so in general, -stcox- offers alternative methods by which one can handle the multiple positive outcomes within group. The default method it exploits is the Breslow approximation to the exact calculation, by which we are able to handle multiple positive outcomes in our data. At the time of writing, a nice demonstration of equivalency is available at stayconsistent.wordpress.com.

Table 3
Nonresident EFC around the adoption of income-targeted aid at flagship public institutions.

	Linear probability (1)	Conditional logit (2) [a]	Probit (3)	Tobit (4)
Treatment period	−0.009 (0.011)	−0.011 (0.015)	−0.009 (0.011)	−0.107 (0.121)
ln(Cost of attendance)	0.057 *** (0.016)	0.083 *** (0.025)	0.054 *** (0.015)	0.626 *** (0.172)
ln(Resident Pell population)	0.015 (0.026)	0.009 (0.036)	0.025 (0.027)	−0.019 (0.268)
ln(Nonresident Pell population)	0.627 (0.877)	0.709 (1.309)	0.802 (0.851)	4.808 (9.493)
Constant	−8.145 (11.421)			−63.142 (122.550)
Institution fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations/institutions	110,488/70	110,488/70	110,488/70	110,488/70
	$F(12, 88142) = 23.60$	$\chi^2(12) = 76.92$	$\chi^2(81) = 2279.41$	$\chi^2(81) = 2480.57$

In columns (1)–(3), the dependent variable is equal to one if the student's expected family contribution (*EFC*) is positive (i.e., a family contribution is expected to be made). Estimated coefficients are reported in Column (1), estimated logit coefficients are reported in Column (2) and estimated probit marginal effects are reported in Column (3). In Column (4), the dependent variable is equal to student *s*'s *EFC* at institution *i* in academic year *t*, continuous for positive values and left-censored at zero. All specifications include a full set of institution and time indicators. Standard errors in parentheses, corrected for clustering at the institution level. *$p < 0.1$; **$p < 0.05$.

[a] As discussed in the text, we exploit the equivalency of the Cox proportional hazard likelihood and the conditional logit likelihood in achieving convergence.

*** $p < 0.01$.

Table 4
Resident proximity-response to the adoption of income-targeted aid at flagship public institutions, by proximity quantiles.

	10th (1)	25th (2)	Median (3)	75th (4)	90th (5)
Treatment period	0.237 *** (0.028)	0.133 *** (0.026)	0.064 *** (0.011)	0.007 ** (0.003)	−0.004 (0.003)
ln(Cost of attendance)	−0.175 *** (0.015)	−0.017 (0.015)	0.065 *** (0.008)	0.015 *** (0.003)	0.002 ** (0.001)
ln(Resident Pell population)	0.118 ** (0.055)	−0.105 *** (0.038)	0.048 (0.031)	0.070 *** (0.013)	0.027 ** (0.013)
ln(Nonresident Pell population)	2.253 * (1.168)	−1.695 * (0.865)	−0.447 (0.865)	0.523 ** (0.228)	0.339 (0.226)
Constant	−27.071 * (15.276)	25.693 ** (11.165)	8.980 (11.277)	−2.835 (3.032)	0.533 (2.990)
Institution controls	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes
Observations	420,056	420,056	420,056	420,056	420,056
Distance at quantile (km)	3.8	15.6	66.6	171.0	295.2

The dependent variable is equal to the log of the *X*th distance quantile between home addresses and institutions', where *X* is given by the column heading. All specifications include a full set of institution effects and a full set of time effects, over annual institution-level observations. Bootstrapped standard errors in parentheses.

* $p < 0.01$

** $p < 0.05$

*** $p < 0.1$.

systematically relaxed with the introductions of additional aid opportunities, improved matching may well be exhibited in the distances from which students travel to attend the state's flagship institution.

Fig. 1 produces the kernel-density estimates of the proximity of low-income students to their own institutions in 2000, demonstrating that the distribution of campus proximity is heavily skewed right.[16] (In 2000, the median proximity was 66 km while the mean proximity was 118 km.) This pattern is expected and occurs in all sample years. In some sense, the market for low-income-students at state flagships is small, with 25 percent of (first-year, dependent) Pell students at flagship institutions in our sample reporting their official home residence within 13.7 km of campus.

Given potential heterogeneity in responses across this range of student proximity, we consider a quantile-regression approach to estimating difference-in-difference models of the type,

$$\ln(Distance_{sit}) = \mathbf{x}_{sit}\delta + T_{sit}\beta + \alpha_i + \gamma_t + \epsilon_{sit}, \quad (3)$$

where $Distance_{sit}$ captures the distance (km) between (resident, low-income) student *s*'s home address and institution *i* in academic year *t*, and T_{sit} is the treatment variable, defined to be unity for students and time periods that are

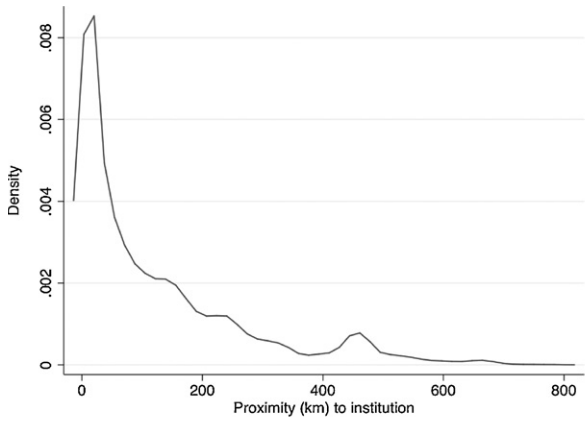

Fig. 1. The proximity of resident, low-income students. This figure reproduces kernel density estimates of the (km) distance between each state's flagship institution(s) and the reported residences of first-year Pell students attending their own-state flagship institutions in 2000 as resi dents.

[16] Kernel estimates derived from an Epanechnikov kernel function.

Table 5
Resident proximity-response to the adoption of income-targeted aid at flagship public institutions, by proximity quantiles and EFC.

	10th (1)	25th (2)	Median (3)	75th (4)	90th (5)
Panel A: *EFC* = 0					
Treatment period	0.245 *** (0.058)	0.240 *** (0.035)	0.048 ** (0.021)	0.008 (0.005)	−0.001 (0.003)
Institution controls	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes
Observations	126,355	126,355	126,355	126,355	126,355
Distance at quantile (km)	3.4	14.2	63.1	168.3	300.3
Panel B: *EFC* > 0					
Treatment period	0.218 *** (0.040)	0.076 *** (0.026)	0.062 *** (0.011)	0.012 *** (0.005)	−0.005 * (0.003)
Institution controls	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes
Observations	293,704	293,704	293,704	293,704	293,704
Distance at quantile (km)	4.0	16.4	68.3	172.0	293.1

The dependent variable is equal to the log of the *X*th distance quantile between home addresses and institutions', where *X* is given by the column heading. All specifications include a full set of institution effects and a full set of time effects, over annual institution-level observations. Bootstrapped standard errors in parentheses. All specifications include control variables as in Table 4.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

subject to the treatment by institution i.[17] As in the enrollment model, student-proximity models include a full set of institution effects, α_i, a full set of time effects, γ_t, and in $\mathbf{x}_{sit}$ the log-cost of attendance (e.g., tuition, fees) and the state-level log-population of first-year, low-income students enrolled in four-year institutions. State-level differences in the average distance travelled by matriculating students are implicitly captured with the institution fixed effects. Student-level errors are captured in bootstrapped errors, ϵ_{sit}.

In Table 4 we establish that a positive treatment effect exists over a large range of the distribution of proximity — that is, with treatment the distances travelled by matriculating students increases — but that the treatment diminishes as the distance from campus increases.[18] In Panel A of Fig. 2 we report the coefficient estimate and confidence intervals corresponding to the treatment variable derived from equivalent specifications performed over the entire range of proximity quantiles. Overall, the clear implication is that there are significant increases in an institution's reach where the institution has introduced income-targeted aid.

Table 5 reports similar specifications on separate samples of Pell students according their expected family contributions. Results appear insensitive to *EFC* in most ways, other than to suggest that the larger response seen from more-proximate students falls off sooner among the relatively less needy. Note also that the relatively needy travel less to their institutions throughout most of the distribution. These relative increases are not present to the same extent in the matriculation patterns of nonresident students, as seen in Tables 6 and 7. For the neediest among nonresident students, there is little discernable change in the distance traveled to innovating institutions across the distribution of distances travelled. As nonresidents are not treated by these programs directly, this is consistent with our expectations. Panel B of Fig. 2 also illustrates this pattern across all quantiles. For the relatively less needy (i.e.,

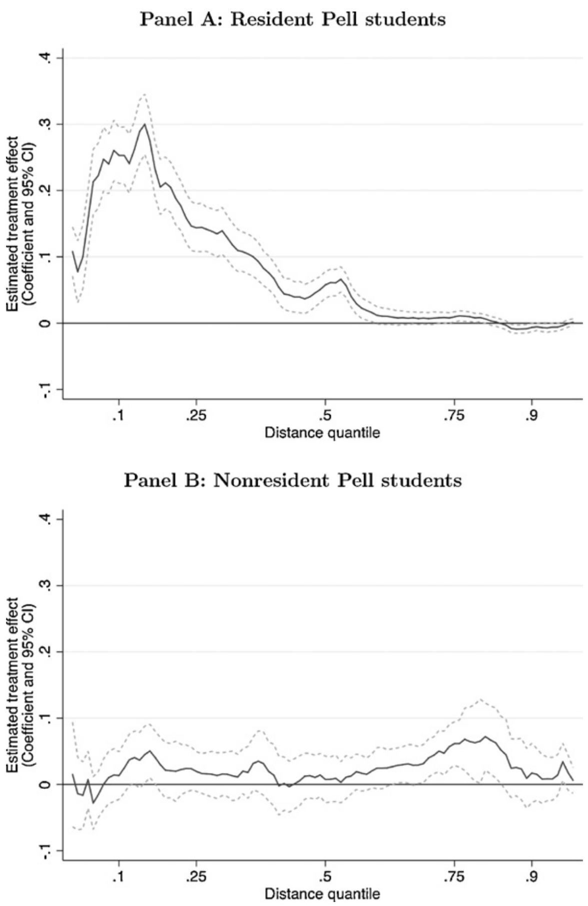

Fig. 2. Response to policy initiative by distance quantile.

[17] As in earlier specifications, the identifying assumption here is again that low-income students in treatment and control states trend similarly prior to the introduction of treatment. Finding no evidence of differential trending in the data, we can confirm the validity of this assumption.

[18] While proportional measures of treatment response suggest a monotonically decreasing effect throughout the range of distances, note that the absolute distance implied by the estimates is inverted-U shaped.

Table 6
Nonresident proximity-response to the adoption of income-targeted aid at flagship public institutions, by proximity quantiles.

	10th (1)	25th (2)	Median (3)	75th (4)	90th (5)
Treatment period	0.005 (0.014)	0.023 ** (0.011)	0.015 (0.033)	0.050 *** (0.016)	0.018 (0.018)
ln(Cost of attendance)	0.022 * (0.012)	−0.003 (0.011)	−0.031 ** (0.014)	−0.034 ** (0.015)	−0.071 *** (0.019)
ln(Resident Pell population)	−0.005 (0.033)	−0.044 (0.038)	−0.064 (0.042)	−0.051 (0.041)	0.065 (0.051)
ln(Nonresident Pell population)	0.787 (1.839)	−1.187 (1.495)	−2.556 (2.664)	−2.585 (2.233)	2.410 * (1.238)
Institution controls	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes
Observations	88,221	88,221	88,221	88,221	88,221
Distance at quantile (km)	65.4	175.5	445.0	1114.3	2902.6

The dependent variable is equal to the log of the *X*th distance quantile between home addresses and institutions', where *X* is given by the column heading. All specifications include a full set of institution effects and a full set of time effects, over annual institution-level observations. Bootstrapped standard errors in parentheses.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table 7
Nonresident proximity-response to the adoption of income-targeted aid at flagship public institutions, by proximity quantiles and EFC.

	10th (1)	25th (2)	Median (3)	75th (4)	90th (5)
Panel A: *EFC* = 0					
Treatment period	−0.012 (0.038)	0.002 (0.025)	−0.011 (0.044)	−0.007 (0.056)	0.000 (0.046)
Institution controls	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes
Observations	24,325	24,325	24,325	24,325	24,325
Distance at quantile (km)	62.8	177.1	458.2	1146.2	4162.5
Panel B: *EFC* > 0					
Treatment period	0.024 (0.021)	0.044 ** (0.021)	0.041 (0.040)	0.046 ** (0.019)	0.021 (0.026)
Institution controls	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes
Observations	63,896	63,896	63,896	63,896	63,896
Distance at quantile (km)	66.4	175.0	441.4	1100.6	2823.5

The dependent variable is equal to the log of the *X*th distance quantile between home addresses and institutions', where *X* is given by the column heading. All specifications include a full set of institution effects and a full set of time effects, over annual institution-level observations. Bootstrapped standard errors in parentheses. All specifications include control variables as in Table 6. *$p < 0.1$; ***$p < 0.01$.
** $p < 0.05$.

with *EFC* > 0), the middle of the distribution appears to respond positively to targeted aid, whereas the tails of the distribution do not vary significantly with aid.

Before we conclude, note that Table 6 reveals an interesting pattern across the distribution of proximity in the underlying behavior of low-income students around changes in *COA* — the price effect (across distance quartiles) tends toward negative as one considers more-distant locations. One interpretation of this pattern is that the more geographically distant is a student, the greater are the student's individual costs to attending and the greater their incentive to determine quality independent of price. As the confounding of price and quality are often suggested as a reason for the positive correlation of price and enrollment, this can be viewed as a challenge to the often-implied prior that students closer to the institution are better informed about the true quality.

7. Conclusion

We use unique data on all Pell students between 1999 and 2007, that constitutes the most comprehensive representation of poor students entering higher education over this period, to examine the access effects of the introduction of targeted, need-based aid programs available to in-state students at several 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 for in-state students experienced any systematic increase in the number of (first-time, first-year, dependent) Pell students, the financial need of matriculating Pell students or their proximity to campus.

Our analysis strongly suggests that targeted-aid programs influence the behavior of needy students, but in a subtle way. Specifically, enrollment models generate negative enrollment gains from adopting a targeted need-based aid program. At the same time, models of *EFC* and proximity (as measured by the distance between the institution and their official residence prior to matriculation) indicate that institutions that introduce these programs subsequently enroll more-financially-needy and more-geographically-distant students. Quantile regressions also indicate that these aid programs generate a non-linear (i.e., an inverted-U) effect on proximity with maximum effect well below the mean distance. Overall, our results suggests that need is not being met on the extensive margin and that enrollment levels actually fall with the introduction of targeted aid. Enrollments of the relatively needy fall less, however, and that matriculating Pell students display more need after treatment suggests that along the intensive margin income-targeted aid may still be benefiting the most needy. We also find that institutions that introduce income-targeted aid subsequently enroll geographically more-distant students, suggestive of improved matching.

Where the higher-ability population has been considered (i.e., Linsenmeier et al., 2006), no significant enrollment effect has been associated with similar policy changes. Yet, at flagship public institutions, where low-income students are much more prevalent and more representative of the wider population of needy students, we find that enrollments have actually declined. One might consider that active capacity and/or budget constraints imply that "improved" access in a matching sense may actually displace some needy students in favor of others. Collectively, our findings are consistent with Rothstein and Rouse (2007) insofar as increased basins of attraction are suggestive that need-based aid programs can reduce credit constraints. Our findings are also consistent with the Avery et al. (2006) hypothesis that these programs permit students to better match within the hierarchy of institutions.

While it is possible that improved access and consequent displacement could work towards a need-blind process where needy students are appropriately matched within the hierarchy of institutions, further work must be done to understand the full general-equilibrium enrollment and access effects of such programs. For example, Linsenmeier et al. (2006), analyzing student responses to one of the earliest adopters of targeted need-based aid in the country, finds statistically significant enrollment gains in low-income minority populations. However, it is unclear whether such programs would have similar efficacy in light of the adoption of similar programs at comparable institutions. Given our data, we are unable to test for differential effects across race. However, future work should consider samples of institutions with competing need-based aid programs in order to tease out the general-equilibrium effects of need-based aid and their implications for low-income and minority populations across the full distribution of ability.

Our findings are important as they suggest that institutions can influence access with income-targeted aid and that Pell students are responsive to financial incentives. They are also somewhat provocative, however, insofar as they suggest that measurable influences are somewhat more complex than may have been anticipated. One explanation for this complexity may be that institutions themselves are likely to optimize around the new aid regimes, particularly when budget constraints are not relaxed with innovations in aid.

Acknowledgements

We thank Tim Bramby and seminar participants at University College Dublin, University of Edinburgh, University of Dundee, University of Aberdeen, and University of Guelph for comments on earlier versions of this manuscript. We also thank Peter Stiffler for helpful research assistance. Any and all errors remain our responsibility.

References

Abraham, K. G., & Clark, M. W. (2006). Financial aid and students' college decisions: Evidence from the District of Columbia Tuition Assistance Grant Program. *Journal of Human Resources*, *41*(3), 578–610.

Angrist, J. D. (1993). The effect of veterans benefits on education and earnings. *Industrial and Labor Relations Review*, *46*(4), 637–652.

Avery, C., Hoxby, C., Jackson, C., Burek, K., Pope, G., & Raman, M. (2006). *Cost should be no barrier: An evaluation of the first year of Harvard's Financial Aid Initiative* (NBER Working Papers 12029). National Bureau of Economic Research.

Bound, J., & Turner, S. (2002). Going to war and going to college: Did World War II and the GI Bill increase educational attainment for returning veterans? *Journal of Labor Economics*, *20*, 784–815.

Brewer, D., Eide, E., & Ehrenberg, R. (1999). Does it pay to attend an elite private college? Cross cohort evidence on the effects of college type on earnings. *Journal of Human Resources*, *34*(1), 104–123.

Cornwell, C., Mustard, D. B., & Sridhar, D. J. (2006). The enrollment effects of merit-based financial aid: Evidence from Georgia's HOPE program. *Journal of Labor Economics*, *24*(4), 761–786.

Curs, B. R., & Singell, L. D. (2002). An analysis of the application and enrollment processes for in-state and out-of-state students at a large public university. *Economics of Education Review*, *21*, 111–124.

Curs, B. R., Singell, L. D., & Waddell, G. R. (2007). The Pell program at thirty years. In J. Smart (Ed.), *Higher education handbook of theory and research. Vol. XXII*. New York: Springer.

Dynarski, S. M. (2000). Hope for whom? Financial aid for the middle class and its impact on college attendance. *National Tax Journal*, *53*(3), 629–661.

Dynarski, S. M. (2003). Does aid matter? Measuring the effect of student aid on college attendance and completion. *American Economic Review*, *93*(1), 279–288.

Dynarski, S. M. (2004a). The new merit aid. In C. Hoxby (Ed.), *College choices: The economics of where to go, when to go and how to pay for it* (pp. 63–100). Chicago: University of Chicago Press.

Dynarski, S. M. (2004b). Who benefits from the education saving incentives? Income, educational expectations and the value of the 529 and Coverdell. *National Tax Journal*, *57*(2), 359–383.

Ellwood, D., & Kane, T. (2000). Who is getting a college education: Family background and the growing gaps in enrollment. In S. Danzinger, & J. Waldfogel (Eds.), *Securing the future*. New York: Russell Sage Foundation.

Hansen, L. (1983). Impact of student financial aid on access. In J. Froomkin (Ed.), *Crisis in higher education* (pp. 84–96). New York: Academy of Political Science.

Havemen, R., & Wilson, K. (2007). Access, matriculation, and graduation. In S. Dickert-Conlin, & R. Rubenstein (Eds.), *Economic inequality and higher education: Access, persistence, and success* (pp. 17–43). Russell Sage Foundation.

Hoxby, C. M. (1997). *How the changing market structure of U.S. higher education explains college tuition* (NBER Working Papers 6323). National Bureau of Economic Research.

Kane, T. J. (1994). College entry by Blacks since 1970: The role of college costs, family background, and the returns to education. *The Journal of Political Economy*, *102*, 878–911.

Kane, T. J. (2003). *A quasi-experimental estimate of the impact of financial aid on college-going* (NBER Working Papers 9703). National Bureau of Economic Research.

Kane, T. J. (2007). Evaluating the impact of the D. C. Tuition Assistance Grant Program. *Journal of Human Resources*, *42*(3), 555–582.

Leslie, L. L., & Brinkman, P. T. (1987). Student price response in higher education: The student demand studies. *Journal of Higher Education*, *58*(2), 181–204.

Linsenmeier, D. M., Rosen, H. S., & Rouse, C. E. (2006). Financial aid packages and college enrollment decisions: An econometric case study. *Review of Economics and Statistics*, *88*(1), 126–145.

Long, B. T. (2004). How do financial aid policies affect colleges? The institutional impact of the Georgia HOPE Scholarship. *Journal of Human Resources*, *39*, 1045–1066.

McPherson, M. S., & Schapiro, M. O. (1998). *The student aid game*. Princeton University Press.

Mishel, L., Bernstein, J., & Allegretto, S. (2007). *The state of working America 2006/2007*. Cornell University Press.

Rothschild, M., & White, L. J. (1995). The analytics of the pricing of higher education and other services in which customers are inputs. *The Journal of Political Economy*, *103*, 573–586.

Rothstein, J., & Rouse, C. (2007). *Constrained after college: Student loans and early career occupational choices* (NBER Working Papers 13117). National Bureau of Economic Research.

Seftor, N., & Turner, S. E. (2002). Back to school: Federal student aid policy and adult college enrollment. *Journal of Human Resources*, *37*(2), 336–352.

Singell, L. D., Waddell, G. R., & Curs, B. R. (2006). HOPE for the Pell? Institutional effects in the intersection of merit-based and need-based aid. *Southern Economic Journal*, *73*(1), 79–99.

Snyder, T. D., Tan, A. G., & Hoffman, C. M. (2006). *Digest of education statistics, 2005*. Washington, D.C.: National Center for Education Statistics.

van der Klaauw, W. (2002). Estimating the effect of financial aid offers on college enrollment: A regression-discontinuity approach. *International Economic Review*, *43*, 1249–1287.

Wetzel, J., O'Toole, D., & Peterson, S. (1998). An analysis of student enrollment demand. *Economics of Education Review*, *17*(1), 47–54.

Winston, G. C. (1999). Subsidies, hierarchy and peers: The awkward economics of higher education. *The Journal of Economic Perspectives*, *13*(1), 13–36.

Zhang, L. (2007). Nonresident enrollment demand in public higher education: An analysis at national, state, and institutional levels. *Journal of Higher Education*, *31*(1), 1–25.