

MARGINAL EFFECTS OF MERIT AID FOR LOW-INCOME STUDENTS*

JOSHUA ANGRIST
DAVID AUTOR
AMANDA PALLAIS

Financial aid from the Susan Thompson Buffett Foundation (STBF) provides comprehensive support to a student population similar to that served by a host of state aid programs. In conjunction with STBF, we randomly assigned aid awards to thousands of Nebraska high school graduates from low-income, minority, and first-generation college households. Randomly assigned STBF awards boost bachelor's (BA) degree completion for students targeting four-year schools by about 8 points. Degree gains are concentrated among four-year college applicants who would otherwise have been unlikely to pursue a four-year program. Degree effects are mediated by award-induced increases in credits earned toward a BA in the first year of college. The extent of initial four-year college engagement explains differences in impact by target campus and across covariate subgroups. The projected lifetime earnings effect of awards exceeds marginal educational spending for all of the subgroups examined in the study. Projected earnings gains exceed funder costs for urban students and for students with relatively weak academic preparation. *JEL Codes:* H52, I22, J24.

*This study was carried out under data-use agreements between MIT and the Susan Thompson Buffett Foundation (STBF) and between STBF and Nebraska's public colleges and universities. We are grateful to Sally Hudson for her contributions to this project. Noa Benveniste, Nick Gebbia, Raymond Han, Kenya Heard, Anran Li, and Julia Turner provided outstanding research assistance. Enrico Cantoni, Sydnee Caldwell, Brandon Enriquez, Tyler Hoppenfeld, Sookyo Jeong, Olivia Kim, Brendan Malone, Kemi Oyewole, Karen Scott, and Carolyn Stein were instrumental in the project's early stages. Our thanks also go to Eryn Heying, Jim Shen, and Anna Vallee for invaluable administrative support, and to the staff of the STBF for their expert assistance in implementing the evaluation. We thank the Provost's Office at the University of Nebraska, the Nebraska State College System, and Nebraska's community colleges for their support for this effort and for sharing their data. Raj Chetty, Amy Finkelstein, Nathan Hendren, Lisa Kahn, Lawrence Katz, Danielle Li, Ben Sprung-Keyser and seminar participants at AASLE, Amazon, Brookings, Boston University, Carleton College, Dartmouth, Harvard, IIES, J-PAL, MIT, NBER Summer Institute, Northwestern, Princeton, UC Berkeley, University of Chicago, University of Melbourne, University of Michigan, and Yale made many helpful comments and suggestions. We acknowledge financial support from the STBF and the MIT SEII seed fund. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research or the views of institutional study partners.

© The Author(s) 2021. Published by Oxford University Press on behalf of the President and Fellows of Harvard College. All rights reserved. For Permissions, please email: journals.permissions@oup.com
The Quarterly Journal of Economics (2022), 1039–1090. <https://doi.org/10.1093/qje/qjab050>.
Advance Access publication on December 20, 2021.

I. INTRODUCTION

U.S. governments and private organizations spent \$184 billion on financial aid to undergraduates in 2019. Government grant aid amounted to about \$3,250 per full-time undergraduate, while private and institutional grants came to almost \$5,600 per student.¹ Yet, the effect of this vast expenditure on college enrollment and degree completion remain unclear. Causal effects of aid are difficult to identify for at least two reasons. First, aid decisions are confounded with student characteristics like family background and ability. Second, naturally occurring variation in aid rules often changes aid packages by only a few hundred dollars. It's hard to say whether the response to such modest changes predict those of withdrawing or adding more substantial awards.

This article gauges the effects of grant aid on degree completion using a randomized field experiment that allocated scholarships to 3,700 high school seniors who graduated from 2012 to 2016. The experiment was conducted in partnership with the Susan Thompson Buffett Foundation (STBF), which funds about 11% of Nebraska high school seniors who go on to attend a Nebraska public college.² Characterized by modest merit cutoffs, a focus on applicants to public colleges, and strict family income eligibility caps, the STBF program targets an economically disadvantaged population judged capable of college-level work. Three-quarters of those in the experimental sample are eligible for need-based federal Pell grant aid, one-third are nonwhite, and fewer than one-third have a parent with a bachelor's degree (BA). STBF awards are unusually comprehensive, paying college costs for up to five years at any Nebraska public four-year college and up to three years at any Nebraska public two-year college. Because STBF grant aid can be applied to any part of a student's total cost of attendance—tuition, fees, books, room and board, personal expenses, and transportation—the awards are offset little by clawbacks or caps that affect other sorts of postsecondary aid.

For whom and by how much does STBF aid boost degree completion? Random assignment of STBF awards shows that aid boosts six-year BA completion rates for students targeting

1. These statistics are from <https://research.collegeboard.org/> (accessed May 2020). The federal government also loaned an average of \$4,090 per undergraduate in 2019.

2. Authors' calculations from data obtained from STBF and [Coordinating Commission for Postsecondary Education \(2013\)](#).

four-year schools by about 8 points (on a base of 64%). Degree gains are concentrated among four-year applicants who are unlikely to have otherwise enrolled in four-year programs and who have low predicted BA completion rates. This inverse relationship between baseline expected completion rates and the causal effect of aid on BAs is not a mechanical ceiling effect: even in the subgroups most likely to graduate, completion rates are no more than 80%. Aid to applicants targeting two-year schools does not increase associate degrees but may increase BAs. The latter effect is positive but not significantly different from zero.

Our analysis uses a simple causal model to explain degree gains among applicants targeting BA programs. Specifically, we show that degree effects can be explained by the effect of awards on credit units earned toward a BA in the first year of study. STBF aid is effective to the extent that it promotes early and deep engagement with a four-year college program. This early-engagement mediator accounts for heterogeneous effects by target campus (e.g., whether a student targets a University of Nebraska campus in Omaha or Lincoln) and across covariate subgroups defined by characteristics like race and ACT scores.³

We use an overidentification test to evaluate the hypothesis that early four-year engagement is the sole channel through which aid affects degree completion. Although other stories cannot be ruled out, the null hypothesis that attributes bachelor's degree gains to this single causal pathway fits remarkably well. The results reported here also show no significant difference in the impact of aid accompanied by academic support services (delivered through a program called Learning Communities) and the effects of financial awards alone. Results comparing recipients of aid plus academic support services with other award recipients should be seen as preliminary, however, because they rely on data for only two cohorts.⁴

The article concludes with a provisional comparison of program costs and anticipated earnings gains for STBF award

3. By “engagement,” we mean four-year college credits taken in the first year after high school. Other studies use this term to capture emotional, behavioral, and cognitive involvement in learning (Appleton, Christenson, and Furlong 2008). Cole et al. (2020), who study the STBF Learning Communities program, measure engagement by how frequently students ask questions and connect with peer mentors.

4. Larger samples, available in years to come, should generate more precise estimates of the causal effect of Learning Communities services.

recipients. This analysis highlights the gap between the private and social costs of marginal degrees. On average, scholarship awards to students targeting bachelor's degrees cost the funder a total of \$32,250 over six years, while raising direct costs of attendance (tuition plus books and supplies) by only \$2,390. Viewed through this lens, most funder spending is a transfer. At the same time, the estimated lifetime earnings gains generated by scholarship awards seem likely to exceed the sum of incremental educational costs and forgone earnings for each of the subgroups examined here (defined, for example, by demographic characteristics, academic preparation, and Pell eligibility). The comparison of expected gains with funder costs is more mixed, but gains are likely large enough to outweigh costs for award recipients whose degree attainment is most strongly affected by scholarship awards. This includes urban applicants, applicants who indicate they prefer a four-year college but are also considering two-year colleges, and applicants with weaker academic preparation. From the funder's point of view, award targeting increases program efficiency markedly.

II. BACKGROUND

II.A. The STBF Scholarship Program

STBF has been funding Nebraskan college students since 1965 and supported around 4,000 students in 2020. STBF is the largest private provider of postsecondary grant aid in Nebraska; more than half of Pell-eligible Nebraska seniors who apply for federal aid also apply for an STBF scholarship.⁵

STBF financial support is awarded on the basis of need and merit to Nebraska-resident high school seniors and Nebraska high school graduates. Both public and private school graduates are eligible, as are GED holders. Aid can be applied toward the cost of attendance (including tuition, fees, and room and board) at any public two-year or four-year college in Nebraska. Award amounts are campus-specific. Specifically, STBF sets a maximum award amount for each institution that is roughly equal to tuition and fees plus a \$500 book allotment. For example, 2013 awards provided \$8,500 per academic year for full-time students at the

5. Authors' calculations from data obtained from the Federal Student Aid office.

University of Nebraska's Lincoln campus, where tuition and fees amounted to \$8,060. Awards are prorated for part-time students. Recipients' total grant aid is capped at the federally recognized cost of attendance (COA) where they're enrolled. Conditional on good academic standing (award recipients are expected to maintain at least a 2.0 GPA), STBF awards are renewable for five years, three of which can be used at a two-year college.⁶

Scholarship eligibility is limited to applicants with a FAFSA-determined expected family contribution (EFC) below \$10,000 and a high school GPA above 2.5.⁷ Scholarship applicants complete an online application (typically due around February 1), submitting their FAFSA, high school transcript, an essay, and recommendation letters. Scholarship decisions are announced in mid-April. Applicants are asked to identify a first-choice target school at which they hope to use the scholarship (such as the University of Nebraska at Omaha). This is nonbinding, but predicts winner's college choices well. [Online Appendix A.1](#) details the application and scholarship renewal process further.

STBF aid has much in common with major public programs for postsecondary support. Like the federal government's Pell grant, STBF awards are based in part on financial need. Like many state aid programs, STBF considers both need and indicators of college readiness. STBF awards provide substantially more grant aid than Pell grants and are available to many applicants with EFCs above the Pell cutoff, but some state programs approach STBF levels of aid. These include the CalGrant program examined by [Kane \(2003\)](#) and [Bettinger et al. \(2019\)](#), and the Texas Longhorn Opportunity Scholarship and Century Scholars programs evaluated by [Andrews, Imberman, and Lovenheim \(2020\)](#). Combined with Pell, the Texas programs cover all tuition and fees at the University of Texas and Texas A&M. Like STBF awards, the Texas programs target low-income, college-bound high school students and provide a range of academic support services to recipients who enroll at a covered campus.

Many recipients of STBF awards (known as Buffett Scholars) attend the University of Nebraska, known locally as NU. Scholarship winners who attend one of NU's three main campuses—

6. STBF awards renew annually conditional on awarded students earning a GPA of at least 2.0 and at the foundation's discretion otherwise. Nebraska public colleges require a 2.0 cumulative GPA to graduate.

7. By way of comparison, the 2013 Pell-eligibility threshold was \$5,081. The 2012 EFC cutoff for STBF awards was \$15,000.

Lincoln (UNL), Omaha (UNO), or Kearney (UNK)—are required to participate in STBF-funded Learning Communities (LC) programs during their first and second years of college. These programs, detailed in [Kezar and Kitchen \(2020\)](#), incorporate a mix of college classes for STBF-funded students, social activities, peer mentoring, and academic advising. Many LC participants at UNK and UNL live in dedicated residence halls.⁸

II.B. Related Work

This study builds on decades of empirical work examining causal effects of postsecondary financial aid. Since the pioneering investigation by [Fuller, Manski, and Wise \(1983\)](#), economists have explored the hypothesis that college aid is mostly inframarginal, that is, primarily a transfer that leaves recipients' college outcomes unchanged.

[Online Appendix](#) Table A1 summarizes many published econometric analyses of grant aid.⁹ This table shows a wide range of estimated aid effects, even when computed for the same programs (as do the research summaries in [Dynarski and Scott-Clayton 2008](#); [Deming and Dynarski 2010](#); [Page and Scott-Clayton 2016](#)). Most relevant for our purposes are studies using experimental and quasi-experimental methods. In the latter category, econometric investigations of the effects of Pell grants typically exploit discontinuities in the Pell award formula via a regression discontinuity (RD) design. Recent RD estimates from [Scott-Clayton and Schudde \(2020\)](#) and [Denning, Marx, and Turner \(2019\)](#) suggest that Pell aid has a modest effect on persistence and degree completion. Early contributions by [Hansen \(1983\)](#) and [Kane \(1996\)](#), by contrast, show little effect of the introduction of the Pell program on student outcomes.

Regression discontinuity investigations are not limited to investigations of Pell grants. [Castleman and Long \(2016\)](#), for example, uses an RD design to examine the effect of Florida's Student

8. Some award recipients after 2013 were offered aid without required LC participation through a new award program described below. Impact evaluations of LC programs and LC-type services include [Bloom and Sommo \(2015\)](#), [Angrist et al. \(2009\)](#), [Bettinger and Baker \(2014\)](#), [Weiss et al. \(2015\)](#), and [Levin and Garcia \(2018\)](#).

9. A related literature looks at the effect of family income on college enrollment. For example, [Bulman et al. \(2021\)](#) finds that lottery windfalls increase college enrollment only if they are sufficiently large, while [Hilger \(2016\)](#) estimates small negative enrollment effects of parental job loss.

Access Grant. The resulting estimates show that grants increase college enrollment, particularly in four-year institutions, as well as increasing BA completion. [Bettinger et al. \(2019\)](#) finds that California's CalGrant significantly increases bachelor's degree completion but does not affect initial college enrollment.

Other studies use difference-in-differences style analyses of state aid program rollouts to identify causal aid effects. In an influential implementation of this approach, [Dynarski \(2000\)](#) finds that Georgia's HOPE Program increased both college enrollment and college completion. Applying a similar methodology, [Barr \(2019\)](#) reports modest positive post-9/11 GI Bill effects on college enrollment and graduation.

The wide range of results arising from observational studies is exemplified by [Cohodes and Goodman \(2014\)](#), which finds that Massachusetts' Adams Scholarship reduces BA completion. These negative effects appear to reflect diversion of scholarship recipients from institutions with higher graduation rates to less competitive (on average) public colleges.

Evidence on state merit aid since [Dynarski \(2000\)](#) is also mixed. [Fitzpatrick and Jones \(2016\)](#) and [Sjoquist and Winters \(2015\)](#), for example, find little or no effect of state merit scholarship programs on enrollment and completion. As we discuss at length below, a key channel for STBF's effect appears to operate through initial enrollment. Also suggestive of the importance of early college engagement, [Carruthers and Ozek \(2016\)](#) finds that the loss of financial aid leaves degree completion rates unchanged.

Consistent with our emphasis on the timing of award effects, programs that focus on academic performance and postenrollment progress have so far yielded modest and/or subgroup-specific graduation effects, if any. Interventions in this domain include West Virginia PROMISE scholarships evaluated in [Scott-Clayton \(2011\)](#) and [Scott-Clayton and Zafar \(2019\)](#) and the incentive schemes examined in [Angrist, Lang, and Oreopoulos \(2009\)](#) and [Angrist, Oreopoulos, and Williams \(2014\)](#). The incentive-heavy WV Promise program's six-year BA completion effects faded to zero 10 years after the award date.

Recent randomized evaluations provide an important point of comparison for our study. One of these examines the Wisconsin Scholars Grant (WSG), a program that offered \$3,500 per year to Pell-eligible Wisconsin residents enrolled as full-time freshmen at four-year colleges. WSG receipt leaves degree completion rates unchanged ([Anderson et al. 2019](#)). It is noteworthy, however, that

because WSG awards are made to already-enrolled first-year college students, they cannot affect first-year enrollment. Similarly, [Mayer, Patel, and Gutierrez \(2015\)](#) reports that aid to low-income parents enrolled at two-year schools and already receiving financial support accelerates degree completion but does not increase it. [Harris and Mills \(2021\)](#) reports results from a program offering financial aid to Milwaukee high school students enrolled at in-state colleges; this aid affected neither college enrollment nor bachelor's degree completion.

There have been few randomized evaluations of aid programs for two-year students. But one, the Accelerated Study in Associate Programs (ASAP), which targets already-enrolled community college students, appears to be highly effective at increasing degree completion and shortening time to degree in a randomized trial. ASAP is unusual in that its low-income recipients receive a wide array of support services, including some targeting nonacademic needs (see [Scrivener et al. 2015](#); [Miller et al. 2020](#)). [Deming and Walters \(2017\)](#) also finds large positive effects of college spending—broadly defined—on enrollment and degree completion.

How does the STBF program and our evaluation of it fit into this literature? First, STBF awards are unusually comprehensive, though some state programs offering aid at public institutions are almost as generous. STBF awards are also made early enough to change the entire postsecondary path for college-bound high school students. STBF awards include an incentive component (since students must meet minimum credit and GPA requirements) that may or may not be important. Finally, aid evaluations using random assignment are rare.

II.C. Research Design and Sample Construction

Among five cohorts of scholarship applicants aiming to enroll in fall 2012 through fall 2016, a subset of STBF awards were allocated by random assignment. Applications were scored based on applicants' college readiness, financial need, and other factors important to the foundation. The highest-scoring applicants (roughly 15% of the applicant pool) were guaranteed awards, while the lowest-scoring applicants (roughly 10%) were removed from consideration. The rest were subject to random assignment, with award rates determined by a variety of constraints on award counts at the target schools in each cohort. Because

award rates differ by application year and target school, regression estimates discussed below control for a full set of target-school by application-year dummies to reflect differing award rates. We refer to these controls as strata dummies.

In the 2013–16 cohorts (the second through fifth cohorts), treated applicants targeting NU campuses were offered one of two types of scholarships. The first, described to recipients as Susan T. Buffett Scholarships, combined financial aid with an obligation to participate in LCs. The second, College Opportunity Scholarships (COSs), consisted of financial aid only.¹⁰ This second arm of the study was designed to reveal any incremental treatment effects due to LC participation. In practice, awards with and without an LC component generated similar effects on college enrollment and degree completion. But, our ability to distinguish effects of the two types of awards is limited by the size of the COS treatment sample. Most of the analysis below therefore pools the two treatment groups.

The five cohorts involved in the randomized study include 3,699 treated applicants (applicants offered aid) and 4,491 controls. Among treatment and control applicants, 6,845 indicated a four-year college as their target school were they to be funded; the rest indicated that they would prefer a two-year school. A breakdown of the number of applicants in the treatment and control groups by application year and target campus appears in [Online Appendix Table A2](#). Of the 6,845 applicants targeting a four-year campus, 2,197 were offered STBF scholarships and 862 were offered COS awards (where STBF awards are defined here as those mandating LC participation among NU students). Of the 1,345 applicants targeting two-year schools, 640 were offered scholarships. We analyze scholarship effects separately by target school program length, referring to applicants targeting NU and other four-year colleges as comprising the four-year strata and applicants targeting community colleges as comprising the two-year strata. The primary analyses pool all five experimental cohorts, two of which have not yet completed the experiment—so that the number of cohorts differs according to whether we’re looking at degree completion four, five, or six years after random assignment.

10. Named scholarships may be more prestigious than the same amount of generic grant aid. The Buffett Scholars program is well known in Nebraska, while COSs were new in 2013 and not similarly publicized. COS awards might therefore be expected to have less of a motivating prestige effect.

[Online Appendix B](#) reports a set of comparable (albeit less precise) results computed using samples of balanced cohorts.

II.D. Data and Descriptive Statistics

Data for this study come from the STBF online application, linked with administrative records from Nebraska's public colleges and from the National Student Clearinghouse (NSC), which covers most U.S. postsecondary schools. Scholarship application records contain a rich set of baseline characteristics, including high school transcripts, ACT scores, and demographic and financial information from the FAFSA.¹¹ Over 90% of STBF applicants who ultimately enrolled in college attended a Nebraska public postsecondary school. These colleges and universities provided information on their students' enrollment, aid packages, and academic outcomes. To capture enrollment at private and out-of-state colleges, school-provided data on postsecondary outcomes were supplemented with information from the NSC. [Appendix A](#) provides additional information on data sources and data processing.

The first two columns of [Table I](#) compare eligible scholarship applicants with statewide samples of high school seniors.¹² STBF applicants are from households with an average income equal to only about half the average for the broader population of Nebraska high school seniors. Compared to the average Nebraska high school senior, STBF applicants are also more likely to be female and less likely to have a parent who attended college. ACT scores among STBF applicants are similar to those of other Nebraska ACT test-takers, though applicants are more likely to have taken the ACT.¹³

Consistent with the criteria used to evaluate applications, STBF's top-scoring applicants (those guaranteed awards) have academic credentials well above those of the smaller group of applicants that did not qualify for inclusion in the experimental sample. This can be seen in columns (3) and (4) in [Table I](#), which

11. Data on the race of 2012 and 2013 applicants come from the Nebraska Department of Motor Vehicles.

12. Data in column (1) come from the SEER (gender and race), the ACS (family income and parent education status), and an ACT National Profile Report ([ACT 2012](#)).

13. The high rate of ACT-taking in the sample is indicative of the fact that scholarship applicants are actively thinking about attending college. Although we believe the sample is broadly representative of students traditionally served by grant aid programs, it misses students who do not apply to college or for aid.

TABLE I
DESCRIPTIVE STATISTICS

	Nebraska HS seniors (1)	Eligible applicants (2)	Nonexperimental sample		Experimental sample	
			Guaranteed award (3)	No award (4)	All (5)	Treatment- control (6)
Female	0.49	0.62	0.68	0.54	0.62	0.02 (0.01)
White	0.75	0.63	0.54	0.57	0.66	0.00 (0.01)
Black	0.07	0.06	0.06	0.11	0.06	0.00 (0.01)
Hispanic	0.12	0.21	0.27	0.22	0.20	0.01 (0.01)
Asian	0.03	0.05	0.09	0.04	0.05	-0.01 (0.01)
Other race	0.02	0.04	0.04	0.05	0.04	0.00 (0.00)
Family income (\$)	87,567	44,774 [45,178]	37,503 [73,675]	44,073 [28,233]	46,353 [38,911]	-1,131 (1,226)
EFC (\$)	—	2,692 [3,063]	2,026 [2,682]	2,634 [3,271]	2,836 [3,087]	-89 (75)
Eligible for Pell grant	—	0.75	0.80	0.77	0.74	0.01 (0.01)
At least one parent attended college	0.70	0.66	0.57	0.64	0.68	0.01 (0.01)
At least one parent has a BA	0.44	0.31	0.27	0.28	0.32	0.00 (0.01)
Lives in Omaha	—	0.30	0.35	0.38	0.28	-0.01 (0.01)
Took ACT	0.85	0.94	0.94	0.90	0.94	0.00 (0.01)
Composite ACT score	21.61	21.87 [4.47]	22.67 [4.48]	20.18 [4.14]	21.94 [4.45]	-0.13 (0.10)
High school GPA	—	3.44 [0.43]	3.61 [0.36]	3.11 [0.40]	3.451 [0.416]	0.007 (0.010)
<i>F</i> -statistic					3.45	0.01
<i>p</i> -value					.42	.01
No. of applicants		11,009	1,667	1,152	8,190	

Notes. This table reports descriptive statistics for the experimental sample and, in column (1), a comparison group of Nebraska high school seniors. Data in column (1) come from the SEER (gender and race), the ACS (family income and parental education status), and the ACT National Profile Report (ACT 2012). Treatment-control differences in column (6) come from regressions that control for strata dummies (cohort by target college). The sample includes the 2012–16 applicant cohorts. Missing values for race (6%), family income (5%), and ACT (7%) are imputed from means within strata in the sample of eligible applicants. Standard deviations are reported in brackets. Robust standard errors for the differences in column (6) are reported in parentheses.

contain statistics for the top- and lowest-scoring applicants. Applicants guaranteed STBF awards without random assignment have lower family incomes and less-educated parents than do applicants in the experimental group, statistics for whom appear in column (5). The group guaranteed awards also includes a higher proportion of Hispanic applicants. At the other end of the distribution, applicants disqualified before random assignment have lower high school grades and ACT scores than those subject to random assignment.

Finally, the last column of [Table I](#), which reports strata-adjusted differences in characteristics by treatment status for applicants in the experimental group, suggests the set of applicants randomly selected for an award is closely comparable to the randomly selected control group. [Online Appendix Table A3](#) reports similar balance statistics computed within target-school strata.

III. GAUGING AWARD EFFECTS

STBF paid an average of \$8,200 toward the first year of study for treated students targeting a four-year program. The upper panel of [Figure I](#), shows that these awards boosted applicants' first-year financial aid packages from \$13,300 to \$19,200. Importantly, Panel B in the figure shows that while \$1 awarded raised total aid by only 52 cents, the gap between funder cost and amount received is due almost entirely to a reduction in loans. In fact, for every dollar awarded, grant aid rose 96 cents, with concomitant declines of 33 cents in loans and 5 cents in earnings through work-study programs. [Online Appendix Figure A1](#) reports award effects on aid for applicants in two-year strata. Consistent with the much lower cost of two-year programs, [Figure A1](#) shows average first-year award amounts of around \$3,800 for these applicants. Here, too, STBF awards increased grant aid substantially, in this case by one extra dollar for each dollar awarded.¹⁴

III.A. Effects on Enrollment and Degrees

The reduced-form analysis discussed in this section ignores considerations of initial award take-up. Because 93% of applicants who receive an award accept it, this is innocuous. The more

14. Award effects on loans are small among applicants in two-year strata because two-year students borrow relatively little.

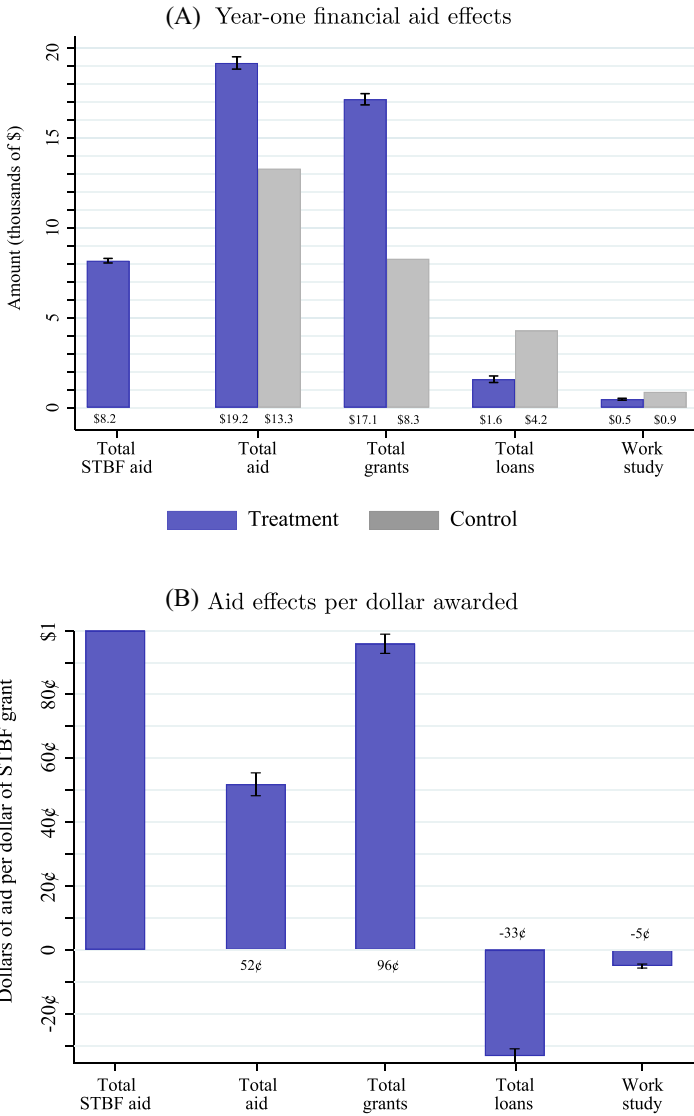


FIGURE I

Award Effects on Postsecondary Aid for Applicants in Four-Year Strata

This figure shows the effect of STBF award offers on aid of various kinds received in the year after scholarship application. The sample is restricted to students in the experimental sample who targeted four-year colleges and enrolled at a Nebraska public institution. Whiskers mark 95% confidence intervals for the treatment effect of an award offer. The regressions used to estimate treatment effects control for strata dummies.

structured analysis described in the next section uses randomized award offers to construct two-stage least squares (2SLS) estimates of the effect of mediating postsecondary choices, such as the type of college attended in the first year enrolled, on degree completion.

Reduced-form treatment effects on postsecondary outcomes, Y_i , are regression estimates of coefficient ρ in the equation:

$$(1) \quad Y_i = X_i'\delta + \rho A_i + \varepsilon_i,$$

where A_i indicates whether a scholarship was offered to applicant i . The covariate vector, X_i , includes saturated controls for application year and target institution, the strata variables that determine experimental award rates. Equation (1) is estimated using the 8,190 randomized applicants who applied between 2012 and 2016.

1. *College Enrollment.* Students applying to the STBF scholarship program are highly motivated to attend college. All but 4% of control-group applicants in four-year strata enrolled in college in the fall semester following their award application. Even so, as can be seen at the top of column (2) in Table II, STBF awards boosted any-college enrollment rates among four-year applicants by a statistically significant 2.3 percentage points. Moreover, while awards had only modest effects on any-college enrollment in the four-year strata, they appear to have increased enrollment in four-year programs by 10 points (on a base of 83%). Much of this gain is attributable to a 6.7-point decline in enrollment at two-year schools.

Like many state-funded financial aid schemes, the STBF program is meant to encourage in-state public college enrollment. The estimates in Table II, Panel B show that STBF awards increased Nebraska public college enrollment among four-year applicants by almost 7 points, a gain driven by an even larger effect on NU enrollment. Paralleling the award-induced decline in any two-year enrollment, awards induced a marked decline in Nebraska community college enrollment. The estimates in Panel B also show a modest award-induced drop in out-of-state and private college enrollment.¹⁵

15. Most STBF applicants who enrolled outside of Nebraska's public colleges and universities attended private, religiously affiliated schools in the Midwest, such as Nebraska Wesleyan University, Creighton University, and Hastings College.

TABLE II
INITIAL ENROLLMENT EFFECTS

	Four-year strata				Two-year strata	
	NU 2013–16				Control mean (1)	Award effect (2)
	Control mean (1)	Award effect (2)	Regular award (3)	COS award (4)		
Any college enrollment	0.964	0.023 (0.004)	0.021 (0.005)	0.024 (0.005)	0.899	0.058 (0.014)
Panel A: Program type						
Four-year	0.833	0.104 (0.008)	0.115 (0.010)	0.089 (0.012)	0.057	0.041 (0.015)
Two-year	0.095	-0.067 (0.006)	-0.078 (0.007)	-0.065 (0.009)	0.838	0.004 (0.020)
Dual enrollment	0.036	-0.014 (0.004)	-0.016 (0.005)	-0.001 (0.007)	0.004	0.013 (0.005)
Panel B: Sector and location						
Nebraska public	0.876	0.067 (0.007)	0.067 (0.009)	0.062 (0.011)	0.862	0.077 (0.016)
University of Nebraska	0.678	0.115 (0.009)	0.137 (0.012)	0.119 (0.014)	0.017	0.046 (0.011)
State college	0.108	0.014 (0.005)	0.001 (0.004)	0.004 (0.005)	0.017	0.012 (0.008)
Community college	0.121	-0.073 (0.007)	-0.084 (0.008)	-0.057 (0.011)	0.830	0.024 (0.020)
Out-of-state public	0.024	-0.016 (0.003)	-0.019 (0.004)	-0.014 (0.005)	0.017	-0.014 (0.005)
Private	0.064	-0.027 (0.005)	-0.028 (0.007)	-0.025 (0.008)	0.020	-0.005 (0.007)
No. of applicants	3,786	6,845	5,212		705	1,345

Notes. This table reports scholarship award effects on postsecondary enrollment measured in the year after scholarship application. Columns (1) and (2) show estimates for four-year strata from all experimental cohorts. Estimates in columns (3) and (4) show estimates for NU applicants from the 2013–16 cohorts. These were computed by replacing A_i in [equation \(1\)](#) with dummies for each version of the NU treatment (regular or COS, where the latter drops the obligation to participate in LCs). Columns (5) and (6) show estimates for two-year strata from all experimental cohorts. Outcomes in each panel are mutually exclusive. Students simultaneously enrolled at both Nebraska public colleges and universities and non-*Buffett* eligible campuses are coded as being in Nebraska public schools only. The regressions used to estimate treatment effects control for strata dummies. Dependent variable construction is detailed in [Appendix A](#). Robust standard errors appear in parentheses.

[Table II](#), columns (3) and (4) report estimates of the effect of regular awards (with mandatory LC participation) and COS awards (without mandated LCs) for applicants in the 2013–16 cohorts who targeted an NU campus. (Only students in these cohorts were eligible for COS awards.) These estimates are com-

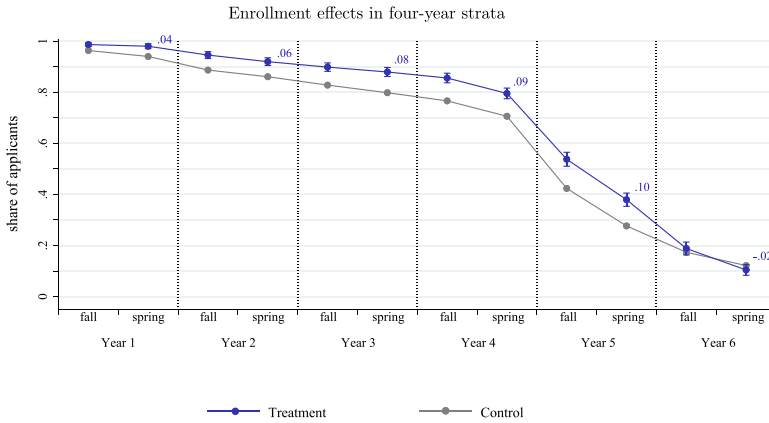


FIGURE II

Enrollment Effects in Four-Year Strata

This figure plots enrollment rates by treatment status for the four-year strata among applicants who had not completed a four-year degree as of the reported semester and year. Light gray lines plot completion rates for control applicants; dark gray/blue lines plot the sum of control means and strata-adjusted treatment effects (color version available online). Whiskers mark 95% confidence intervals. Samples differ by year. Regressions control for strata dummies.

puted by replacing A_i in [equation \(1\)](#) with dummies for each version of the NU treatment. Because regular award recipients are exposed to LC participation only once enrolled, it seems reasonable to expect the two award schemes to affect initial enrollment similarly. Initial enrollment effects of COS and regular awards are indeed similar.

The initial enrollment gains generated by award offers made to applicants in four-year strata led to a persistent increase in college enrollment. This is apparent in [Figure II](#), which plots treatment and control enrollment rates for each semester after random assignment.¹⁶ The sample used to compute each point omits applicants who had completed a college degree by the time the enrollment outcome was recorded. Conditional on not having earned a degree, college enrollment in the treated group is sharply higher than college enrollment in the control group two to five years after random assignment. The figure therefore suggests that awards reduced college dropout rates.

16. [Online Appendix Figure A2](#) plots treatment and control enrollment rates for students in two-year strata.

STBF award offers boosted college enrollment rates more for applicants in two-year strata than for applicants in four-year strata. In particular, the estimate at the top of [Table II](#), column (6) shows a gain of 5.8 points in any-college enrollment for the two-year group, compared with a control mean of 90%, reported in column (5). Four-year enrollment gains are much smaller, however, for applicants in two-year strata: awards increase the probability that a two-year targeting applicant enrolls in a four-year program by only 4 points. The estimates in Panel B also show that awards generated a marked gain in Nebraska public college enrollment for applicants in two-year strata, due mostly to a shift toward NU. Perhaps surprisingly, increased enrollment at NU appears to be mostly a net gain in college enrollment rather than a move away from two-year schools. Our working paper ([Angrist et al. 2016](#)) presents additional estimates of award effects on college enrollment and persistence.

2. *Degree Completion.* STBF awards boosted six-year BA completion rates by 8.1 percentage points for applicants in four-year strata, a substantial gain relative to the control mean of 64%. Estimated degree completion effects for the 2012–14 cohorts (those for which six-year follow-up is now available) appear in [Table III](#), column (2). The overall completion effect is estimated precisely, with a standard error of 0.016.

Columns (3) and (4) juxtapose estimates of the effect of COS and regular STBF awards on degree completion, estimated for the cohort of 2013–14 applicants targeting NU (the subsample eligible for the COS treatment and for which we have data on degrees). In contrast with effects on initial enrollment, here, we might expect effects to differ since COS awards do not include Learning Communities services. As it turns out, estimated COS effects (in column (4)) are close to the regular-award effects (in column (3)), though the COS estimates are somewhat less precise. Estimates of award effects by type are also close to the estimates for all four-year strata in column (2).

The award-induced increase in BAs is due partly to a shift from two-year to four-year programs. STBF awards reduced associate degree completion by 3 points for applicants in four-year strata, with similar drops seen for the 2013/14-only NU sample and among COS award winners. Most of the 8.1-point gain in BA completion, however, is due to a 5.2-point decline in the likelihood that applicants earn no degree (degree outcomes in [Table III](#) are not mutually exclusive).

TABLE III
DEGREE COMPLETION EFFECTS

	Four-year strata				Two-year strata	
	NU 2013–14					
	Control mean (1)	Award effect (2)	Regular award (3)	COS award (4)	Control mean (5)	Award effect (6)
Bachelor's degree earned	0.636	0.081 (0.016)	0.089 (0.022)	0.080 (0.026)	0.240	0.055 (0.034)
Associate degree earned	0.076	-0.032 (0.008)	-0.030 (0.010)	-0.038 (0.011)	0.531	-0.001 (0.038)
Enrolled at four-year	0.015	-0.006 (0.004)	-0.009 (0.005)	-0.009 (0.005)	0.046	0.009 (0.017)
No degree earned	0.307	-0.052 (0.015)	-0.058 (0.021)	-0.055 (0.025)	0.395	-0.046 (0.037)
Enrolled at four-year	0.051	-0.003 (0.007)	-0.009 (0.010)	-0.016 (0.012)	0.014	0.005 (0.009)
Total years of schooling	3.93	0.360 (0.041)	0.366 (0.056)	0.249 (0.068)	3.06	0.393 (0.121)
Time in four-year	3.17	0.592 (0.051)	0.622 (0.070)	0.465 (0.083)	0.751	0.429 (0.108)
Time in two-year	0.487	-0.219 (0.031)	-0.239 (0.042)	-0.212 (0.049)	2.20	-0.077 (0.098)
Dual enrollment	0.278	-0.012 (0.019)	-0.018 (0.025)	-0.004 (0.033)	0.102	0.042 (0.029)
No. of applicants	1,924	3,639	2,383		367	666

Notes. This table reports scholarship award effects on degree completion and years of schooling measured at the end of year six. Columns (1) and (2) show estimates for four-year strata in the 2012–14 cohorts. Estimates in columns (3) and (4) are for NU applicants from the 2013 and 2014 cohorts. These estimates were computed by replacing A_i in equation (1) with dummies for each version of the NU treatment (regular or COS, where the latter drops the obligation to participate in LCs). Columns (5) and (6) show estimates for two-year strata in the 2012–14 cohorts. Regressions used to estimate treatment effects control for strata dummies. Dependent variable construction is detailed in Appendix A. Robust standard errors appear in parentheses.

As can be seen in Table III, column (6), awards do not appear to have increased associate degree completion among applicants in two-year strata. Estimates in this column show a nontrivial positive award effect on BAs in two-year strata of 5.5 points. This estimated gain is not significantly different from zero, though precision may rise as the two remaining cohorts complete the experiment. It seems especially noteworthy that awards made to applicants in two-year strata—comprised of applicants who indicated a desire to attend two-year programs—generated no discernible rise in two-year degree completion.

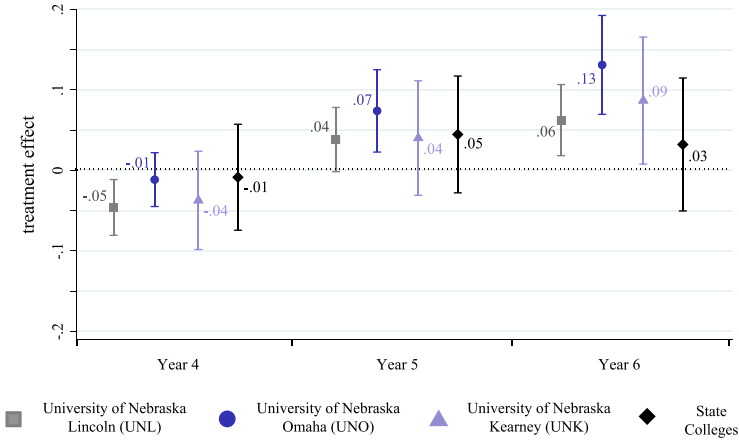


FIGURE III

BA Effects by Target Campus

This figure plots STBF award effects on BA completion for applicants in four-year strata. Samples differ by year. The regressions used to compute these estimates control for strata dummies. Whiskers mark 95% confidence intervals.

Figure III plots award effects on BA completion rates in postassignment years four through six, estimated separately by target campus for applicants in four-year strata. STBF awards appear to have increased time to completion for some. This delay is visible in a statistically significant 5-point decline in completion rates four years out for applicants targeting UNL (and an imprecisely estimated 4-point drop for applicants targeting UNK). Five years after random assignment, however, completion effects turn positive. Award offers boost completion rates most clearly for applicants targeting UNO, by 7 points five years out and 13 points six years out. Estimated effects for applicants targeting other NU campuses are smaller, though (state colleges excepted) they are close to the pooled estimate of 8 percentage points in year six. Estimated five- and six-year completion effects for applicants targeting state colleges are positive, but less precise than the corresponding estimates for applicants targeting NU and not significantly different from zero.

The large degree gains seen for UNO applicants play a leading role in our account of the mechanism by which awards increase completion. UNO serves a mostly low-income, disproportionately nonwhite population, and UNO-targeting award winners are

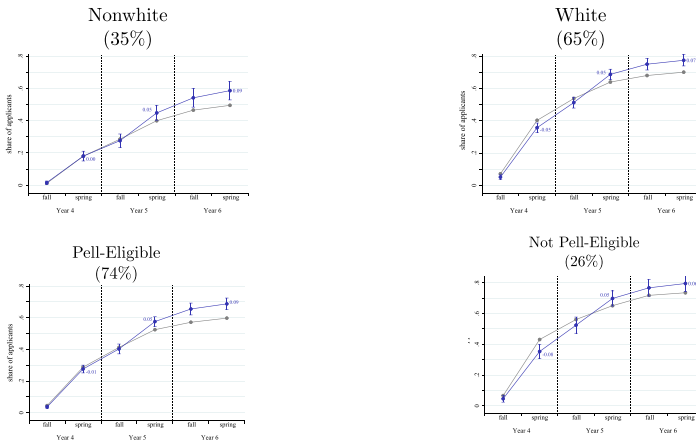
less likely to enroll in a four-year college in the absence of STBF support than are applicants targeting other campuses. Consistent with the pooled estimates in [Table III](#), a year-by-year analysis of treatment effects in four-year strata shows similar degree gains for award winners with and without mandatory participation in LCs. This is documented in [Online Appendix Figure A3](#), which plots yearly estimates of the two types of award effects. The analysis below therefore pools the LC and non-LC treatment groups when estimating effects in four-year strata.

III.B. Degree Effects by Subgroup

[Figure IV](#), Panel A contrasts award effects in sample splits by demographic subgroup. We see, for example, degree gains of 9 points for treated nonwhite applicants, with a corresponding gain of 7 points for whites. Award effects are larger for Pell-eligible applicants than for applicants with family incomes above the Pell threshold. These conditional effects align with the pattern of larger effects for UNO-targeters seen in the previous figure: nonwhite and Pell-eligible Nebraskans are overrepresented in Omaha and therefore disproportionately likely to target UNO. [Online Appendix Figure A4](#), which reports degree effects in additional subgroup splits, shows larger award effects for Omaha residents and for students without college-educated parents, but little difference in effects by gender.

Degree gains are larger for applicant subgroups likely to be less prepared for college, a pattern documented in [Figure IV](#), Panel B. These plots show award-induced BA gains of 12 points among applicants with a high school GPA below the Nebraska median, but only a 4-point gain for above-median applicants. This difference in impact is especially striking in light of the low control-group completion rate (of 42%) among applicants with below-median GPAs. Estimates by ACT score, reported in [Online Appendix Figure A4](#), show a similar pattern. A final split in [Figure IV](#) shows estimates conditional on whether applicants indicated they were likely to attend a two-year school in the absence of STBF support. Applicants indicating a two-year fallback might be seen as ambivalent about their readiness to commit to a four-year program. The estimated BA effect for those indicating a two-year fallback is almost twice as large as the estimate for applicants who considered only four-year colleges. As reported in the figure notes, each of these treatment effect contrasts (nonwhite vs. white,

(A) Demographic Subgroups



(B) Four-Year College Readiness Subgroups

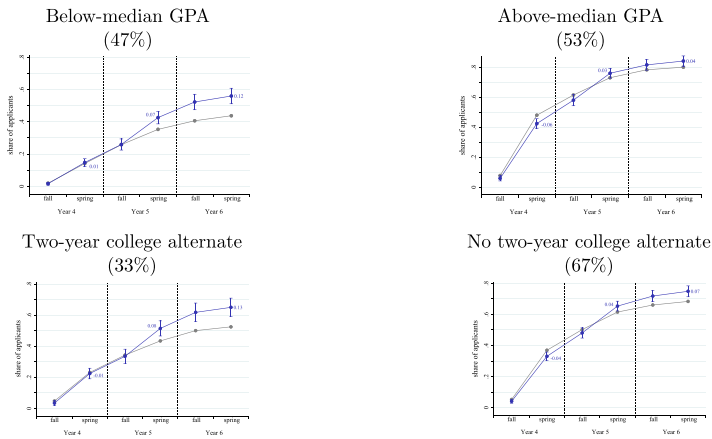


FIGURE IV

BA Effects in Demographic and College Readiness Subgroups

This figure plots mean degree completion rates by treatment status and subgroup for 2012–16 applicants in four-year strata. Light gray lines plot completion rates for control applicants; dark gray/blue lines plot the sum of control means and strata-adjusted treatment effects. Whiskers mark 95% confidence intervals. Samples differ by year. Percentages in each panel are for all experimental cohorts. The median high school GPA in Panel B is 3.49. STBF award applicants were asked to indicate their first choice (target school) and rank alternatives. “Two-year college alternate” indicates that a student ranked a two-year college among their alternative target schools on the STBF application. The differences in treatment effects in year six for each subgroup split are as follows (standard errors are given in parentheses): race: 0.018 (0.005), Pell-eligibility: 0.027 (0.006), GPA: 0.081 (0.005), two-year alternate: 0.060 (0.006).

Pell eligible vs. ineligible, below- vs. above-median GPA, two-year alternate vs. no two-year alternate) is statistically significant.

[Online Appendix Figure A5](#) shows that the subgroup differences in [Figures III, IV](#), and [A4](#) are driven by more than just the outsized treatment effects for applicants targeting UNO. In a split between UNO targeters and all remaining four-year applicants, effects are larger in the former group, but still significantly different from zero in the latter. A final subgroup analysis appears in [Online Appendix Figure A6](#). This figure reports results for a sample split determined by above- and below-median predicted BA completion, where completion is predicted using the covariates generating [Figure IV](#) and [Online Appendix Figure A4](#). Award-induced BA gains are estimated to be 12 points for those with below-median predicted completion but only 4 points for those with high predicted completion rates.

IV. EXPLAINING AWARD EFFECTS

The variation in strata and subgroup effects seen in [Figures III, IV](#), and [Online Appendix Figure A4](#) is explained here by a causal mediation story that hinges on the type of campus at which applicants first enroll. Specifically, we argue that an award-induced shift toward early, strong engagement with a four-year college is the primary channel by which STBF aid generates additional bachelor's degrees. Variation in the strength of award-induced shifts into four-year programs provides a consistent account of the reduced-form treatment effect variation seen in the figures.

IV.A. College Targets and Destinies

Most award recipients in four-year strata started their college careers on a four-year campus. But many applicants not selected for an award also embarked on a four-year program. How did awards change the likelihood of four-year college enrollment? For applicants in four-year strata, effects on initial four-year enrollment are strongest when awards facilitate enrollment at an applicant's target campus and when the alternative to target-campus enrollment is not a four-year program. We therefore quantify award-induced changes in initial college enrollment in two steps: first, by estimating award effects on target campus enrollment; second, by computing four-year enrollment rates among

target-enrollment compliers when these applicants do not receive an award.

The effects of STBF awards on target campus enrollment largely mirror award effects on BA completion, a pattern documented in [Figure V](#), Panel A (where bar height shows effects on target enrollment and dots mark effects on BA completion). We see, for example, that among four-year applicants, target enrollment effects are especially high for applicants targeting UNO, for Omaha residents, and for nonwhite applicants. On the other hand, target enrollment effects are similar for men and women, while BA effects also differ little by sex. With one exception (the split by Pell eligibility), subgroup differences in target enrollment effects are consistent with the direction of differences in group-specific BA effects.

Effects on target enrollment by measures of college readiness likewise parallel the differences in degree gains seen across college-readiness subgroups. As noted already, [Figure IV](#) and [Online Appendix Figure A4](#) show especially large degree gains for applicants with below-median ACT scores and below-median high school GPAs, as well as for applicants in four-year strata who considered a two-year alternative. Differences in target campus enrollment effects across these splits are also noteworthy, with larger effects in groups that appear less prepared for BA programs.

In the causal framework outlined by [Angrist, Imbens, and Rubin \(1996\)](#), award effects on target campus enrollment can be interpreted as a target-enrollment compliance rate. To make this idea precise, let T_{ji} denote potential target enrollment when $A_i = j; j = 0, 1$. Observed target enrollment, T_i , is determined by potential target enrollment according to:

$$T_i = T_{0i} + (T_{1i} - T_{0i})A_i.$$

Target compliers are defined as applicants for whom $T_{1i} = 1$ and $T_{0i} = 0$, that is, they enroll at their target campus when offered an award but not otherwise. Target compliers have $T_{1i} > T_{0i}$ and award effects on T_i equal the probability of this event.

By definition, target-enrollment compliers in four-year strata enroll in a four-year program when $A_i = 1$ (because applicants in four-year strata have a four-year target). We're interested in the likelihood that target compliers enroll in four-year programs when assigned to the control group. This is measured by

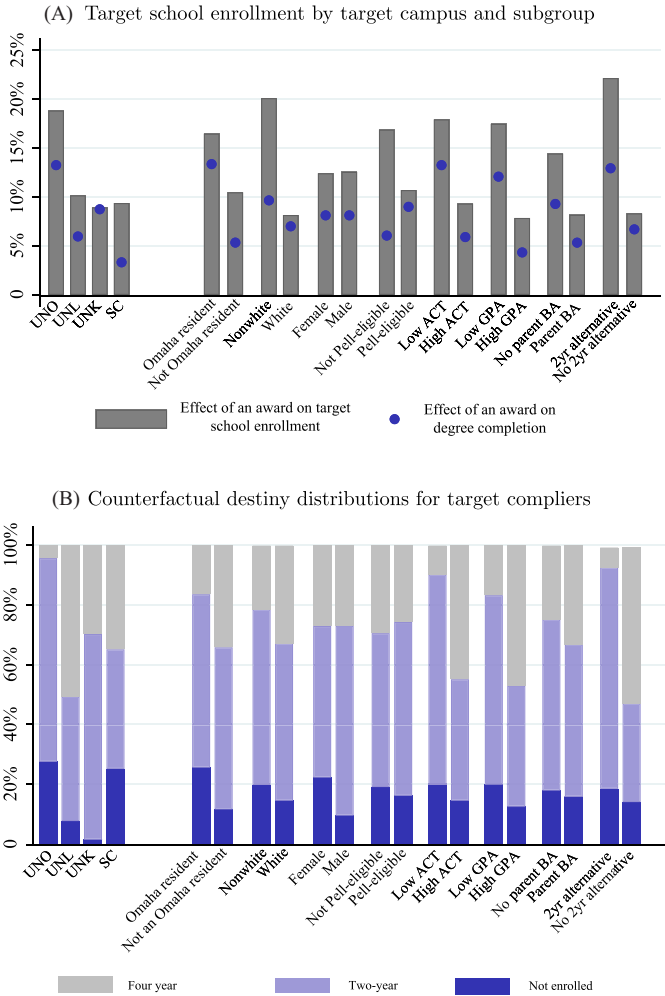


FIGURE V

First-Stage Estimates and Counterfactual Destinies for Target-School Compliers in Four-Year Strata

Bar height in Panel A measures the share of four-year applicant strata and subgroups who are target-school compliers; target-school compliers are defined as the set of applicants who enroll in their target school when awarded scholarships but not otherwise. Dots in Panel A indicate BA completion effects in each group. Panel B shows the distribution of enrollment by school type for target-school compliers when compliers are untreated (that is, the distribution of counterfactual destinies). Enrollment status is computed using first-year data only. Groups in the figure are the union of those used for Figure IV and Online Appendix Figure A4.

computing the share of target compliers enrolled in four-year programs, the share enrolled in two-year programs, and the share unenrolled—in the event they fail to receive an award. As in [Abdulkadiroglu et al. \(2017\)](#), we refer to these shares as the distribution of counterfactual destinies. Following [Abdulkadiroglu, Angrist, and Pathak \(2014\)](#), destinies are estimated by 2SLS.¹⁷

Figure V, Panel B plots estimated destiny distributions for target compliers in four-year strata, separately by target campus and subgroup. An important finding here is the substantial heterogeneity in the fraction of compliers who enroll in four-year programs without STBF aid. In the breakdown by target campus, for example, compliers targeting UNO are least likely to find their way to a four-year program absent an STBF award. This fact, in combination with a relatively high target-campus compliance rate in the UNO group, contributes to outsized award-induced degree gains for applicants targeting UNO. Similarly, across demographic and college-readiness subgroups, degree gains are most pronounced for applicants whose counterfactual destinies are least likely to include a four-year program.

IV.B. Measuring Mediation

The target compliance rates and college enrollment destinies exhibited in [Figure V](#) motivate a parsimonious mediation hypothesis that specifies early engagement with four-year programs as a key causal channel for STBF award effects. To make this hypothesis concrete, let f_{1i} denote the fraction of a full-time four-year course load an applicant completes in the school year immediately following random assignment (STBF defines a full load as 24 credit units per year). The mediation hypothesis is captured

17. Briefly let $W_i = c$ for $c \in \{4, 2, 0\}$ encode whether an STBF applicant is in a four-year program, two-year program, or unenrolled. In this case, the three-point destiny distribution, ω_c , is given by:

$$\omega_c = \frac{E[(1 - T_i)1\{W_i = c\}|A_i = 1] - E[(1 - T_i)1\{W_i = c\}|A_i = 0]}{E[(1 - T_i)|A_i = 1] - E[(1 - T_i)|A_i = 0]},$$

computed separately for each c . This formula, an IV estimand, is derived using the fact that $W_i = (1 - T_i)1\{W_{0i} = c\} + T_i1\{W_{1i} = c\}$, where W_{0i} and W_{1i} denote potential enrollment indexed against T_i , and the fact that the denominator is the negative of the target compliance rate. [Abadie \(2002\)](#) establishes identification of marginal potential outcome distributions in an extension of the LATE theorem ([Imbens and Angrist 1994](#)). As in earlier work, the 2SLS version of ω_c generalizes the formula above to allow for covariates.

by a model in which awards boost f_{1i} , which in turn increases BA completion, Y_i . This can be written:

$$(2) \quad Y_i = \beta'_1 X_i + \mu_1 f_{1i} + \varepsilon_{1i}$$

$$(3) \quad f_{1i} = \pi'_{10} X_i + \pi_{11} A_i + (\pi'_{12} X_i) A_i + \eta_{1i},$$

where ε_{1i} in equation (2) is the random part of potential degree completion in the absence of treatment, and μ_1 is the causal effect of interest. Equation (3) is the first stage for a 2SLS procedure that uses A_i to instrument f_{1i} . The first-stage residual, denoted η_{1i} in equation (3), is uncorrelated with A_i and X_i by construction.

Equation (3) allows the first-stage effect of award offers on f_{1i} to vary with covariates. We can write these covariate-specific first-stage coefficients as:

$$\pi(X_i) = \pi_{11} + \pi'_{12} X_i.$$

Importantly, the causal relationship of interest, described by equation (2), omits interactions between f_{1i} and X_i . The reduced form implied by equations (2) and (3) therefore satisfies:

$$(4) \quad \rho(X_i) \equiv E[f_{1i}|X_i, A_i = 1] - E[f_{1i}|X_i, A_i = 0] = \pi(X_i)\mu_1,$$

for each value of X_i . In other words, the assumptions behind equations (2) and (3) imply that all heterogeneity in reduced-form award effects by strata and subgroup is explained by differences in the extent to which scholarship offers change early four-year engagement. It bears emphasizing that equation (4) says more than that first-year course completion is correlated with college completion (as it surely is). Instead, it says that scholarship offers increase college completion only to the extent that they increase first-year course completion and it restricts the impact of first-year courses on college completion to be the same across subgroups.

Figure VI presents a visual instrumental variables (IV) representation of equation (4). This figure plots covariate-specific reduced-form estimates for degree outcomes against the corresponding first-stage estimates. The sample used to compute these estimates includes the 2012–14 cohorts in two-year and four-year strata. The covariate vector X_i includes dummies indicating four-year target campuses (UNO, UNL, UNK, and state colleges), a

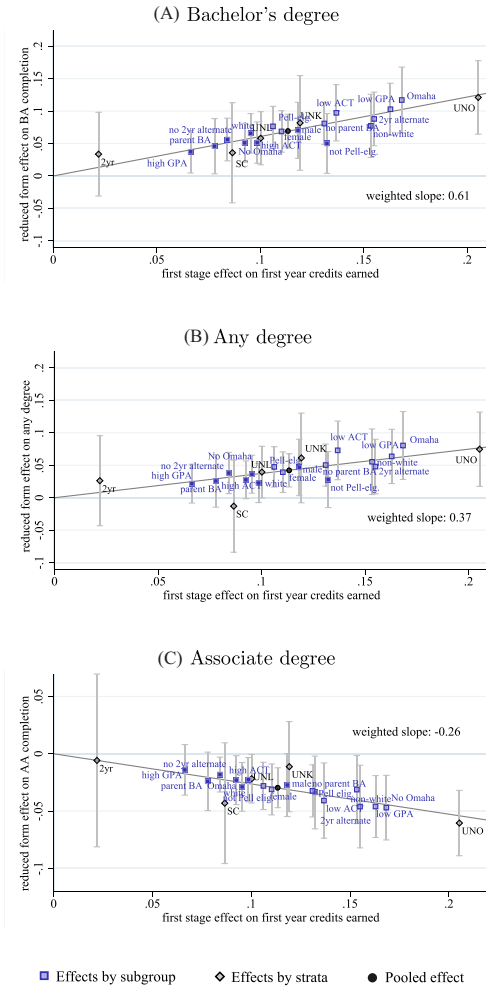


FIGURE VI

Visual IV Estimates of the Effect of Award-Induced Four-Year Credit Completion on Degrees

This figure plots reduced-form offer effects against first-stage offer effects, estimated as detailed in Section IV.B. The x-axis shows effects on credit-hours earned at any four-year institution in the first postapplication school year. Credit-hours are scaled by 24, the STBF standard for full-time enrollment. The y-axes show effects on degree completion. Regression lines in each panel are constrained to run through the origin and estimated using data weighted by strata and subgroup sample sizes. Estimates are for 2012–14 applicant cohorts in two- and four-year strata. All models control for strata and subgroup main effects. Whiskers mark 95% confidence intervals for each reduced-form estimate.

dummy for those targeting two-year schools, and dummies for the demographic and college-readiness subgroups seen in Figures IV and A4. Specifically, the figure plots easily interpreted sample average values of estimated $\hat{\rho}(X_i)$ and $\hat{\pi}(X_i)$ for all groups of interest. For example, one point in the figure has coordinates $(\hat{E}[\hat{\pi}(X_i)|F_i = 1], \hat{E}[\hat{\rho}(X_i)|F_i = 1])$ where F_i indicates female applicants and $\hat{E}[\cdot|F_i = 1]$ denotes a sample average. Appendix B details the calculations behind this figure further and shows that the slope of the line through the points plotted therein is a 2SLS estimate of μ_1 identified by instrumenting f_{1i} in equation (2) using A_i and the set of interactions between X_i and A_i as instruments. The figure also plots the pooled effect, the point determined by first-stage and reduced-form estimates for an IV model without interactions.¹⁸

The fitted line in Panel A of the figure, computed for award effects on BA completion, has a slope of 0.61 when estimated with no intercept, a proportionality restriction implied by equation (4). The relationship between first-year college success and degree completion that this estimate reflects is partly mechanical. Yet, while success in the first year of college is necessary for degree completion, it's not sufficient. Likewise, STBF awards need not boost degree completion only to the extent that they improve first-year outcomes. The overidentification statistic associated with 2SLS provides a formal test of the hypothesis that all variation in $\rho(X_i)$ is explained by variation in $\pi(X_i)$, leaving no room for other effects of A_i on degree completion. This test statistic is essentially a scaled version of the R^2 for the lines plotted in Figure VI (see, section 2.2.2 of Angrist and Pischke 2009). The addition of a data point for two-year strata reveals whether low degree effects for applicants targeting two-year schools are explained by small award effects on f_{1i} in these strata.

Overidentification test results, along with the associated 2SLS estimates and first-stage F -statistics, appear in the first three columns of Table IV, for alternative specifications of X_i .

18. The interaction terms underlying the figure are estimated jointly (the interaction of offer with low ACT, for example, is estimated in a model with other interactions, including that for low GPA). The figure plots fitted values from a group-size weighted regression of group-specific average reduced forms on the corresponding group-specific average first stage, omitting the estimate without interactions since this point is implied by the group-specific estimates. The estimates plotted in Figure VI and reported in Table IV (discussed below) are from reduced-form and first-stage equations that include the full vector of X_i as controls.

TABLE IV
 IV ESTIMATES OF THE EFFECT OF INITIAL FOUR-YEAR CREDITS COMPLETED ON
 DEGREES

	2SLS				
	Strata and subgroup interactions	Subgroup interactions	Strata interactions	Just- identified	OLS
	(1)	(2)	(3)	(4)	(5)
Panel A: Bachelor's degree					
Four-year credits earned	0.55 (0.09)	0.58 (0.10)	0.59 (0.10)	0.61 (0.11)	0.57 (0.02)
First stage					
Any award				0.11 (0.01)	
<i>F</i> -stat	11.20	14.55	25.09		
Overidentification test	7.75	6.69	0.71	–	
Degrees of freedom	12	8	4		
<i>p</i> -value	.80	.57	.95		
Panel B: Any degree					
Four-year credits earned	0.32 (0.09)	0.34 (0.10)	0.36 (0.10)	0.37 (0.11)	0.43 (0.02)
Overidentification test	8.25	5.64	2.23	–	
<i>p</i> -value	.77	.69	.69		
Panel C: Associate degree					
Four-year credits earned	– 0.28 (0.05)	– 0.27 (0.07)	– 0.27 (0.05)	– 0.26 (0.08)	– 0.20 (0.01)
Overidentification test	3.75	1.72	2.09	–	
<i>p</i> -value	.99	.99	.72		
<i>N</i>	4,305	4,305	4,305	4,305	

Notes. This table reports 2SLS estimates and overidentification test statistics for models where the outcome is degree completion and the endogenous variable is initial four-year engagement as defined in Figure VI. The just-identified estimate in column (4) uses a single offer dummy as the instrument. Estimates in columns (1)–(3) are from overidentified models with instrument sets constructed by interacting award offers with sets of dummies indicated in column headings. Instruments include an any-award dummy plus interactions with strata dummies (for UNL, UNO, UNK, SC, and two-year colleges) and subgroup dummies (for Omaha residence, nonwhite, male, Pell-eligible, below-median ACT, below-median GPA, first-generation, and listing a two-year college as an alternate). Strata and subgroups plotted are not mutually exclusive. Online Appendix Figure A8 plots a version of VIV using mutually exclusive groups. Estimates are for 2012–14 applicant cohorts in two- and four-year strata. All models control for strata and subgroup main effects. Robust standard errors appear in parentheses.

These test results accord with the impression that the VIV line fits well. The large *p*-values associated with the overidentification test statistics suggest that—across all strata and subgroups—deviations between sample moments and the proportionality hypothesis expressed by equation (4) can be attributed to sampling variance. The first-stage estimate for female applicants, for example, shows STBF offers boost f_{1i} by about 0.11. This in turn boosts BA completion by about 0.069, so the implied IV estimate

for this group is 0.62, close to the slope of the line in Panel A of the VIV figure. The point for two-year strata also lands near the line and (consistent with modest degree gains for this group) appears in the southwest corner of the figure.¹⁹

Combining all strata- and subgroup-specific instruments leads to the overidentified 2SLS estimate of 0.55 reported in the first column of Table IV (overidentified 2SLS estimates differ from the corresponding VIV slope estimate due to differences in weighting and because the set of covariate interactions in the instrument list is not saturated). The first-stage F -statistic for this heavily overidentified model is only around 11. In view of the risk of finite-sample bias in this scenario, it's noteworthy that 2SLS estimates computed using smaller instrument sets are similar. In particular, column (2) reports a 2SLS estimate of 0.58 when using subgroup interactions only; column (3) shows an estimate of 0.59 using strata interactions only; and column (4) reports a just-identified IV estimate computed using only an award dummy as an instrument. The first-stage relationship is notably stronger in these models, while the estimated effect of f_{1i} on degree completion changes little.

As a point of comparison, the OLS estimate generated by regressing a BA completion dummy on f_{1i} , controlling for X_i , appears in the last column of Table IV. At 0.57, this estimate is close to the corresponding IV estimates. The similarity between OLS and 2SLS estimates of the effect of f_{1i} on degree completion suggests, perhaps surprisingly, that there's little selection bias in the OLS estimates. Finally, other panels in Figure VI and Table IV repeat the analyses of Panel A with different dependent variables. The VIV and 2SLS estimates in Panel B of these exhibits suggest f_{1i} boosts overall degree attainment by only around 0.37, a gain well below the estimated increase in BAs. As can be seen in Panel C, the gap between BA and overall degree gains is accounted for by the fact that early engagement with four-year colleges decreases associate degrees. The VIV slope for f_{1i} effects on associate degree completion is -0.26 (almost identical to the 2SLS estimates in Table IV, Panel C). OLS estimates of the effect of f_{1i} on any degree and associate degree completion differ markedly from the corresponding 2SLS estimates, with evidence of positive selection bias in the former.

19. [Online Appendix](#) Figure A7 shows that VIV proportionality restrictions fit equally well in the sample of applicants not targeting UNO.

1. *Shifting College Credits.* STBF awards push some applicants from nonenrollment all the way to full-time four-year college enrollment. At the same time, for applicants likely to attend a four-year program without an award, award receipt may boost the number of four-year credits earned. How much does the intensity of four-year college engagement contribute to the causal mediation story suggested by Figure VI and Table IV? Figure VII measures intensity changes in two ways. Panel A plots the histograms of four-year credits earned in the first posttreatment year, separately for treatment and control applicants in four-year strata (these are distributions of f_{1i} measured in terms of units earned rather than share of a full-time load). The figure documents a large decline in the likelihood of having earned zero four-year credits, from around 13% in the control group to around 4% in the treated group, a statistically significant decline. The histograms also show clear, treatment-induced increases in the probability of earning 24–28 four-year credits. This finding is important because 24 credits marks a full-time load.

Figure VII, Panel B provides another view of the award-induced credit shift. This panel plots scaled treatment-control differences in the probability an applicant earns at least s credits, for each value of $s \in [1, 40]$. This plot is motivated by Angrist and Imbens (1995), which shows that in causal models with an ordered treatment, an IV estimator using a dummy instrument identifies a weighted average of single-unit causal effects (called an average causal response, or ACR). In particular, the ACR averages causal effects of increasing credits from $s - 1$ to s , for each s . Single-unit effects are specific to applicants who were induced by awards to move from fewer than s to at least s credits. ACR weights are given by the control-minus-treatment difference in (one minus) the cumulative distribution function of credits earned in each group, divided by the corresponding first-stage effect of the instrument on the ordered treatment. These weights can be interpreted as the probability that awards cause applicants to go from fewer than s credits earned to at least s credits earned. More formally, let $f_{1i}(0)$ denote potential credits earned in the absence of treatment and let $f_{1i}(1)$ denote potential credits earned when treated. The ACR weighting function is proportional to $P[f_{1i}(0) < s \leq f_{1i}(1)]$.

In a scenario where awards move some applicants from 0 four-year credits earned to 24 or more credits earned, with no one affected otherwise, the ACR weighting function is flat for $s \in [1, 24]$. To see this, note that if $f_{1i}(0) = 0$ and $f_{1i}(1) \geq t$ for all affected

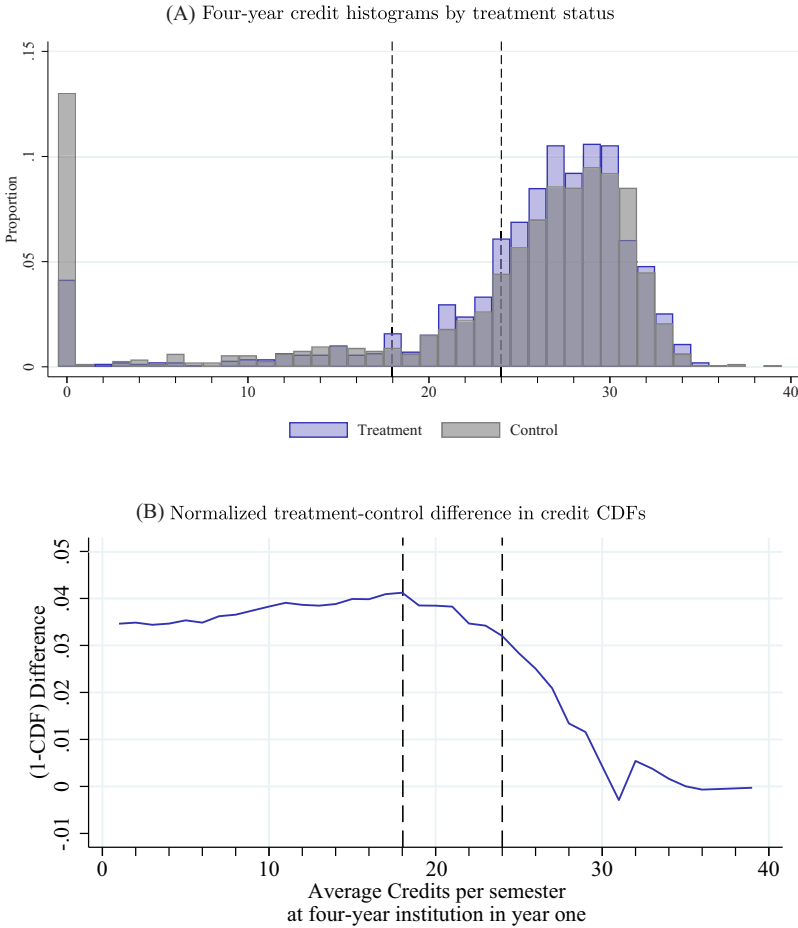


FIGURE VII

The Distribution of Four-Year Credits by Treatment Status

Panel A plots the histogram of four-year credits earned in the first postapplication year, separately by treatment status. Panel B plots the difference in one minus the CDF of four-year credits earned by treatment status, normalized to generate the weighting function described in the text. The x -axis in Panel B measures the likelihood that an award shifts applicants from completing fewer than s credit(s) to completing at least s credit(s). Cutoffs for $\frac{3}{4}$ - and full-time enrollment are marked on the x -axis. Students must be enrolled at least $\frac{3}{4}$ -time to qualify for STBF support. Estimates are for 2012–14 applicant cohorts in four-year strata.

applicants, the probability $f_{1i}(0) < s \leq f_{1i}(1)$ is the same for all $0 < s \leq t$. Figure VII, Panel B is largely consistent with this, showing a reasonably flat weighting function from $s = 1$ through $s = 24$, with a modest rise in the probability of completing 14–22 credits that's also visible in the histograms in Panel A (the vertical hash marks denote $\frac{3}{4}$ -time and full-time enrollment; students must be enrolled at least $\frac{3}{4}$ -time to qualify for STBF support). This pattern suggests that most applicants for whom awards boost four-year engagement move from attempting no four-year credits to full-time study. Some, however, move to more intensive but still part-time study. The fact that the weighting function declines steeply for $s > 24$ suggests awards push few students beyond the threshold for full-time enrollment.

2. *Dynamic Exclusion.* Early engagement with a four-year program appears to be an important channel through which STBF awards increase BA completion. But this claim raises the question of why we should focus on first-year engagement and not, say, sophomore or junior-year measures of four-year college credits earned. Is engagement in the first year of college the key step on the path to BA completion? Defining f_{ti} as the fraction of a full credit load earned in year t , it seems reasonable to imagine that awards boost f_{ti} for $t > 1$ as well as boosting f_{1i} . These gains, in turn, may contribute to degree completion. We show here that award-induced changes in downstream f_{ti} , as well as the consequences of these changes for BA completion, can be explained by award effects on f_{1i} . Because this model attributes all causal effects of f_{ti} to effects on f_{1i} , we say that it imposes dynamic exclusion restrictions.

Dynamic exclusion is captured by a causal model of sequential credit completion. This model is:

$$(5) \quad f_{ti} = \alpha'_t X_i + \psi_t f_{1i} + \xi_{ti}; \quad t = 2, 3, 4,$$

where ψ_t is the causal effect of f_{1i} on f_{ti} and ξ_{ti} is a residual assumed to be uncorrelated with A_i , conditional on covariates, X_i . Equation (5) is complemented by a causal model for the effect of f_{ti} on degree completion that can be written:

$$(6) \quad Y_i = \beta'_t X_i + \mu_t f_{ti} + \varepsilon_{ti}; \quad t = 2, 3, 4,$$

where awards and award-covariate interactions are likewise assumed to be uncorrelated with ε_{ti} . Dynamic exclusion is the claim that awards and award-covariate interactions are valid instruments for f_{ti} in equations (5) and (6). In other words, STBF awards boost credits earned in year t solely by virtue of boosting credits in year one. Effects of later credit completion on degrees are explained by this fact.

The orthogonality assumptions that identify equations (5) and (6) imply an illuminating cross-equation restriction. In particular, using equation (5) to substitute for f_{ti} in equation (6) reveals that the coefficient on f_{1i} in equation (2) satisfies:

$$(7) \quad \mu_1 = \psi_t \mu_t.$$

This substitution also shows the residual in equation (2) to be $\varepsilon_{1i} = \varepsilon_{ti} + \mu_t \xi_{ti}$. Dynamic exclusion therefore rationalizes the exclusion restrictions tested in Table IV.

It's worth asking whether equation (7) offers a further set of restrictions worth testing, beyond those examined in Table IV. The answer is that a Wald-type test computed by replacing parameters in equation (7) with the corresponding 2SLS estimates is the same as the overidentification test statistic associated with 2SLS estimation of equation (5).²⁰ This is distinct from the test examined in Table IV.

Table V reports 2SLS estimates of μ_t and ψ_t , along with their product, computed for different instrument sets and values of t . The instruments here are an award dummy, A_i , interacted with the same four-year strata and subgroup dummies used to compute the estimates in Table IV. In this case, the sample is limited to applicants in four-year strata since degree gains are concentrated in this group. Estimates of μ_t show strong effects of college credits

20. Let \hat{f}_{ti}^* denote fitted values from a regression of f_{ti} on instruments and covariates, with covariates then partialled out. Let $\hat{\psi}_t$ denote a 2SLS estimate of ψ_t computed using the same instruments, covariates, and sample. Instrument-error orthogonality in equation (5) implies that in large samples $\hat{\kappa}_{ti} = \hat{f}_{ti}^* - \hat{\psi}_t \hat{f}_{1i}^* \approx 0$, with an asymptotic mean-zero normal distribution; overidentification tests for equation (5) are derived from this distribution. It then follows that the quantity

$$E_n[Y_i \hat{\kappa}_{ti}] = E_n[Y_i \hat{f}_{ti}^*] - \hat{\psi}_t E_n[Y_i \hat{f}_{1i}^*],$$

where $E_n[\cdot]$ denotes sample averages in a sample of size n , converges to zero. Dividing $E_n[Y_i \hat{\kappa}_{ti}]$ by the sample variance of \hat{f}_{ti}^* and again using the fact that $\hat{\kappa}_{ti} \approx 0$ yields the sample analog of equation (7).

TABLE V
DYNAMIC EXCLUSION PARAMETER ESTIMATES AND SPECIFICATION TESTS

	Strata interactions			Subgroup interactions			Strata and subgroup interactions		
	Year 2 (1)	Year 3 (2)	Year 4 (3)	Year 2 (4)	Year 3 (5)	Year 4 (6)	Year 2 (7)	Year 3 (8)	Year 4 (9)
μ_t	0.54 (0.08)	0.61 (0.08)	0.60 (0.08)	0.55 (0.08)	0.65 (0.08)	0.65 (0.07)	0.53 (0.07)	0.63 (0.08)	0.63 (0.07)
ψ_t	1.08 (0.08)	0.95 (0.09)	0.94 (0.11)	1.01 (0.07)	0.86 (0.09)	0.85 (0.09)	1.02 (0.07)	0.87 (0.08)	0.85 (0.09)
$\psi_t\mu_t$	0.58	0.58	0.57	0.55	0.56	0.55	0.55	0.55	0.54
Overid test	4.00	0.72	3.91	9.52	8.39	7.06	14.32	9.24	10.62
<i>p</i> -value	.262	.869	.271	.300	.397	.530	.216	.600	.475

Notes. This table reports 2SLS estimates of μ_t in equation (6) and ψ_t in equation (5). The product of these two should equal μ_1 in equation (2). The overidentification test associated for 2SLS estimation of equation (7) tests this restriction. Instrument sets are indicated above column headings. Robust standard errors appear in parentheses.

earned in years two through four on degree completion, while the estimated ψ_t indicate increases in f_{1i} yield large gains in four-year credits earned down the road. The latter effects range from 0.85 to 1.08.

The product of the estimated μ_t and ψ_t suggest these parameters indeed reflect the effect of credits earned in the first year of college on later academic progress. In particular, the estimated $\mu_t\psi_t$ are remarkably close to the corresponding estimates of μ_1 shown at the top of Table IV (all around 0.58). Moreover, the overidentification test statistics associated with 2SLS estimates of equation (5) are consistent with the claim that STBF awards affect four-year credits earned in later years solely by increasing f_{1i} . This finding notwithstanding, it may be the guarantee of financial support for five years that induces otherwise hesitant prospective four-year students to fully dive in upon initial enrollment. In future research, we hope to be able to investigate whether front-loading aid is a cost-effective way to enhance aid effectiveness.

V. COST-BENEFIT PERSPECTIVES

The causal effects of STBF scholarship awards on adult employment, earnings, and financial security will not be known for at least a decade. In the meantime, this section provides a prospective cost-benefit analysis that compares predicted award-induced

increases in lifetime earnings with measures of program cost overall and by demographic subgroup.

V.A. *Estimating Costs*

Funder spending on awards is easily measured. While a funder's award costs may affect program viability, the economic cost of an award is a distinct concept: economic costs correspond to program-induced spending net of transfers. Scholarships may increase overall educational spending by increasing time spent in school and by moving students into more expensive programs. We therefore use the experimental framework to measure the incremental spending induced by awards, while also reporting per capita funder spending.

To determine the effect of award offers on funder spending, we put aid disbursements, D_i , on the left-hand side of the reduced-form model for treatment effects (equation (1)). No aid is disbursed to control group applicants, so the effect of STBF offers on D_i captures average funder spending on treated applicants adjusted for strata differences.

To quantify the extent of marginal educational spending—that is, spending induced by awards—we replace the funder cost variable, D_i , on the left side of equation (1) with a measure of the cost of college attendance. We use this award-induced cost of attendance later in our cost-benefit analysis in Section V.C. This variable, denoted COA_i , is proxied by the federally determined cost of attendance as reported in the Institutional Characteristics File of the publicly available Integrated Postsecondary Education Data System (IPEDS, U.S. Department of Education 2019). The imputed COA_i variable covers tuition, fees, and an allowance for books and supplies. We compute COA_i for all ever-enrolled applicants, including those who attend private colleges or non-Nebraska public colleges.²¹

The statistics for D_i and COA_i reported in Table VI, Panel A highlight the difference between STBF disbursements and marginal educational spending. Average COA_i is roughly \$30,940 among treated applicants in the four-year strata, close to aver-

21. This calculation omits housing and transportation costs and uses the smaller of credit-based costs or full-time tuition. Cost data are missing for one applicant. Costs of books and supplies for 8% of applicants are imputed using averages for two- and four-year schools. We discount funder cost and cost of attendance back to the first postapplication year at a 3% rate.

TABLE VI
COLLEGE COSTS AND MARGINAL SPENDING BY TARGET CAMPUS

	NU target campuses				State colleges (5)
	Four-year strata (1)	UNL (2)	UNO (3)	UNK (4)	
Panel A: College costs (\$1,000s)					
Treated					
Funder cost	32.25	33.09	33.05	32.97	26.77
COA	30.94	32.75	30.87	30.63	25.49
Years of schooling	4.30	4.31	4.39	4.32	3.98
Control					
COA	28.55	31.07	26.03	26.55	25.78
Years of schooling	3.93	4.01	3.91	3.88	3.78
No. of applicants	3,639	1,632	1,009	500	498
Panel B: Decomposition of marginal spending					
Award effects on:					
(1) Log cost of attendance	0.16 (0.02)	0.10 (0.03)	0.27 (0.04)	0.20 (0.05)	0.08 (0.06)
(2) Log years of college	0.11 (0.01)	0.09 (0.02)	0.13 (0.03)	0.13 (0.04)	0.07 (0.04)
(3) Log cost per year of college	0.05 (0.01)	0.01 (0.02)	0.14 (0.03)	0.07 (0.03)	0.01 (0.04)
Share of marginal spending due to increased years of college $\frac{(2)}{(1)}$	0.66	0.91	0.49	0.65	0.88
No. of applicants	3,593	1,616	990	495	492

Notes. This table reports award effects on degree costs. Panel A shows statistics including students who have zero years of college and thus zero cost of attendance; Panel B excludes these students. Panel A reports mean cost and years of college attendance for control students and treatment students. The first three rows in Panel B report results from regressions of log COA, log years of college, and log cost per year on a dummy for being offered a scholarship in the given sample. These regressions include strata dummies. Estimates are for the 2012–14 cohorts in four-year strata. Robust standard errors appear in parentheses. Funder cost and COA are discounted back to the first postapplication year at 3%. Dollar values are reported in thousands.

age program disbursements in this group (\$32,250). On the other hand, while mean D_i is zero for controls, average control COA_i is around \$28,550, only modestly below the average cost of attendance in the treated group.

Table VI, Panel B allocates award effects on COA_i to a component that reflects increased time in school and a component that reflects a shift toward more expensive programs. We refer to the latter as cost upgrading. To gauge the relative importance of these components, let COA_{1i} denote college costs incurred when applicant i is treated and let COA_{0i} denote costs incurred otherwise. Because $\{COA_{ji}; j = 0, 1\}$ is the product of years enrolled (denoted

S_{ji}) and cost per year (denoted F_{ji}), we can write:

$$\begin{aligned} \log(COA_{1i}) - \log(COA_{0i}) &= \log(S_{1i}F_{1i}) - \log(S_{0i}F_{0i}) \\ &= \underbrace{\log(S_{1i}) - \log(S_{0i})}_{\text{extra years}} + \underbrace{\log(F_{1i}) - \log(F_{0i})}_{\text{extra cost per year}}. \end{aligned}$$

The first term on the expression's second line captures incremental costs generated by more time in school, and the second term captures cost upgrading, both measured in proportional terms. The average of each piece is obtained by putting observed time in college and per semester spending, respectively, on the left side of equation (1).

Awards increased COA_i by 16 log points on average, as shown in the first row of Panel B. The pattern of spending increases across target strata mostly parallels differences in treatment effects on BA completion and years of schooling by strata. The increase in education spending is largest for UNO-targeting applicants (27 log points), not surprisingly, because this group sees an especially strong award-induced shift towards four-year college enrollment.

The remaining entries in Panel B show that over two-thirds of marginal spending is attributable to additional years of college, with the remainder due to cost upgrading (increased COA per year enrolled). UNO-targeting applicants are the only group for whom cost upgrading makes almost as large a contribution to marginal spending as does additional years enrolled (13 and 14 log points, respectively).²² For applicants targeting UNL and state colleges, by contrast, estimated cost-upgrading effects are not significantly different from zero.

V.B. Projecting Lifetime Earnings Gains

We forecast the expected lifetime earnings impact of grant aid using an earnings equation fit to cross-sectional 2008–19 American Community Survey (ACS) data for Nebraska-born residents aged 18–65 with at least a high school degree (not including GED holders) and at most a bachelor's degree. Returns to schooling are estimated using a Poisson regression model on earnings data that

22. Log COA per year of schooling increases more than the yearly COA level partly because awards boost the share of students enrolling full-time at target campuses, thereby lowering the variance of COA. Due to Jensen's inequality, mean log COA is declining in the variance of COA.

includes zeros. Annual earnings are calculated from the ACS, inflated to current dollars using the chained Consumer Price Index for all urban consumers, and are regressed on dummies for the highest level of schooling completed (some-college-no-degree, AA degree, and BA degree, with high school degree as the reference category) and a quartic in imputed potential experience. We use estimates on time in school from [Park \(1994\)](#) to calculate potential experience separately by gender and race (white/nonwhite) subgroups. [Online Appendix C](#) reports the underlying regression estimates and contains additional details related to imputation.

With a 3% discount rate, BA completion is estimated to boost the PDV of lifetime earnings by \$470,000 on average. This is in line with estimates from [Avery and Turner \(2012\)](#). Also consistent with [Avery and Turner \(2012\)](#), the estimated return to BA attainment is larger for men than for women. Estimated earnings gains differ little by race (white/nonwhite).

These regression results are combined with the scholarship's treatment effects to determine the expected lifetime earnings impact of grant aid. To calculate control group earnings, we use means of degree attainment and imputed time in school from our ACS sample as point estimates in our estimated earnings function. Expected earnings are calculated separately for gender-by-race subgroups and then averaged using as weights the subgroups' prevalence in the control group. By adding treatment effects on degree attainment and time in school calculated by [equation \(1\)](#) to the ACS means, we create expected treatment group earnings. Overall, the STBF scholarship is estimated to increase discounted lifetime earnings by \$21,150 for each treated applicant. These estimates ignore award-induced changes in postgraduate schooling.²³ This gain exceeds the award's average impact on educational spending (\$2,390), but falls below the funder's average cost per awardee of \$32,250.

V.C. Picturing Costs and Benefits

[Figure VIII](#) puts the cost-benefit pieces together for each subgroup considered in [Section IV](#). The top marker of each interval in the figure indicates funder costs while the bottom indicates marginal educational spending (effects on *COA*). Predicted

23. A more detailed description of this procedure can be found in [Online Appendix C](#).

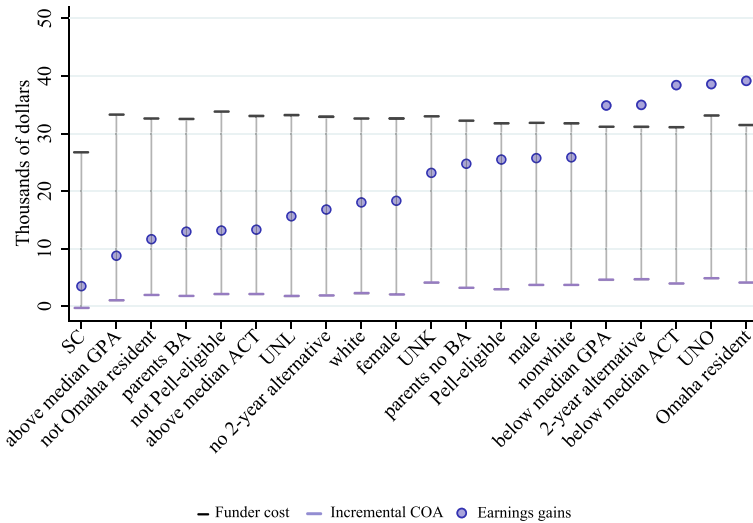


FIGURE VIII

Earnings Gains Compared with Program Costs

This figure compares program costs with estimates of the lifetime earnings generated by award receipt, where the latter are measured by the returns to schooling levels. Estimation details can be found in [Online Appendix C](#). Costs are measured two ways: the lower tick mark indicates the increase in educational spending (COA) generated by awards, while the upper tick mark indicates average funder cost. Estimates are for the 2012–14 cohorts in the four-year strata.

lifetime earnings gains are estimated using a similar parametric approach to the award effects in [Online Appendix Table C2, Panel C](#). As in [Avery and Turner \(2012\)](#), these are computed using a three percent discount rate.

For all groups, predicted earnings gains fall between funder costs and marginal COA, suggesting that STBF awards generate a positive social return on average and for all demographic subgroups. These estimates also imply that funder costs exceed award-induced earnings gains for most subgroups. However, estimated earnings gains exceed both marginal COA and funder costs for the subset of applicants with below-median grades, those who chose a community college as an alternative target, those with below-median ACT scores, those who indicated UNO as a target, and Omaha residents.

As a benchmark, we compare the cost-effectiveness of STBF aid with that of similar public sector scholarship programs in

a hypothetical scenario where the STBF program were publicly funded. Following [Hendren and Sprung-Keyser \(2020\)](#), this comparison uses the marginal value of public funds (MVPF), defined as the ratio of program benefits among policy beneficiaries to net government costs. For STBF beneficiaries, program benefits include a transfer of \$32,250 (the transfer made from the funder to the student, seen in [Table VI](#)) plus the award-induced increase in the PDV of lifetime earnings. The latter quantity is taken to be \$21,150 ([Online Appendix Table C2, Panel C](#)). Assuming that incremental earnings are taxed at 20% reduces the government's cost of operating the program and the private program benefits by \$4,230.

The ratio of private benefits ($\$32,250 + \$21,150 - \$4,230 = \$49,170$) to public costs ($\$32,250 - \$4,230 = \$28,020$) yields an MVPF of 1.75, implying that \$1 of public spending on the STBF program generates \$1.75 of private benefits. An MVPF of 1.75 puts the STBF program near the median of estimated MVPFs of other cost-effective grant aid programs examined in [Hendren and Sprung-Keyser \(2020\)](#). STBF ranks especially highly among programs targeting college-bound high school students. Relevant comparisons include the Massachusetts Adams scholarship, with an MVPF of 0.72, and the Wisconsin Scholars Grant program, with an MVPF of 1.43.²⁴

Based as they are on a predictive model of lifetime earnings, these cost-benefit comparisons are provisional. They seem likely to be conservative for a number of reasons. First, they omit nonpecuniary benefits of schooling related to health, social intelligence, and marriage (documented in [Oreopoulos and Salvanes 2011](#)). Our estimated earnings gains also ignore any scholarship-induced increases in post-BA schooling and possible economic returns to reductions in college debt. Finally, the overall returns to schooling estimated here may also fall below the economic returns to education for students whose school decisions are sensitive to financial constraints (a possibility suggested by [Card 2001](#); [Zimmerman 2014](#)).

24. With a 5% discount rate, the estimated MVPF for STBF aid falls to 1.42. Other comparably structured grant aid programs covered by [Hendren and Sprung-Keyser \(2020\)](#) include Kalamazoo Promise and Tennessee HOPE.

VI. SUMMARY AND CONCLUSIONS

Randomized evaluation of the comprehensive STBF aid program yields results that are both encouraging and cautionary. On one hand, scholarship awards increase four-year degree attainment substantially. On the other, the bulk of award spending is a transfer flowing to applicants whose schooling behavior is unchanged by awards. Aid boosts BA completion most sharply for applicants who aspire to a BA but are unlikely to embark on a four-year program in the absence of aid. Those who benefit most include groups of applicants with below-median grades and test scores, those seeking to enroll at the urban UNO campus, and those considering two-year colleges.

We explain the pattern of degree effects with a parsimonious model that makes the main mediator of award impact a credit-based measure of initial engagement with four-year college. Estimates of this model support the notion that awards induce degree completion primarily by prompting and deepening early engagement with four-year college programs. This finding suggests there may be a large payoff to less costly interventions that act to enhance early engagement. Examples of inexpensive service-oriented early engagement interventions include precollege advising and mentoring (as in [Bettinger and Evans 2019](#); [Carrell and Sacerdote 2017](#)) and efforts to boost SAT- and ACT-taking (as in [Bulman 2015](#); [Goodman, Gurantz, and Smith 2020](#)).

To put the early-engagement hypothesis in context, it's worth noting that almost all STBF applicants start college somewhere regardless of whether they are awarded a scholarship. Yet many are no longer enrolled two and three years out (as shown in [Angrist et al. 2016](#)). This leaves scope for STBF awards to boost four-year degree attainment by increasing persistence in college for those likely to start a four-year program even without STBF aid. The results reported here, however, weigh against the importance of persistence effects beyond those engendered by early four-year credit completion.

Similarly, because STBF awards provide incentives for students to remain in good academic standing, we might expect award incentives to have incremental effects in each academic year, even for applicants destined to start a four-year program anyway. Our findings also weigh against the importance of financial incentives to remain in good academic standing. Once aid recipients have responded to awards in year one by choosing to start and stick with

a four-year school, academic performance incentives and other downstream forces appear to matter little. This conclusion should be qualified, however, with the observation that results for a motivated, college-bound population of STBF applicants need not predict aid effects in other populations and circumstances.

A provisional cost-benefit analysis highlights the fact that most STBF aid spending is a transfer to applicants likely to earn degrees even without an award. The flip side of high transfer costs, however, is the fact that the marginal educational spending induced by STBF awards is low. For each subgroup considered here, the projected net earnings gains from scholarship-induced schooling outweigh the corresponding marginal educational cost. Moreover, although most award money is inframarginal, the projected earnings gains for high-benefit groups (with especially low counterfactual enrollment in a four-year program) also exceeds the corresponding funder cost.

The findings reported here strongly suggest that increased targeting of financial aid awards is likely to enhance aid impact, thereby boosting MVPF. Given that STBF award effects can be explained by the effect of scholarships on full-time four-year enrollment in year one, a fruitful question for subsequent research is whether front-loading financial aid might increase program effectiveness while reducing aid costs. Our results suggest that programs that encourage many students who would not do so otherwise to enroll at a four-year college are especially likely to increase BA attainment. That said, the promise of continuous aid may be necessary to induce initial four-year engagement. The question of the optimal timing of aid flows should be high priority for future work. Finally, [Scott-Clayton and Zafar's \(2019\)](#) estimates showing that aid effects on degrees tend to fade over time highlight the importance of continued follow-up and an investigation of effects on outcomes such as student debt and earnings.

APPENDIX A: DATA

A.A. Application Data

The STBF scholarship application collects detailed information on applicants' baseline characteristics. Academic measures such as GPA are gathered primarily from high school transcripts. We standardize GPAs to a 4.0 scale using the grade conversion formula provided by UNL. We also consider students' ACT

score. Since not all high schools report students' ACT scores on transcripts, transcript data are supplemented with self-reported scores from the application survey for 54% of the experimental sample.²⁵

Most of the financial and demographic data come from applicants' Student Aid Reports (SARs). These reports are available for all STBF applicants who filed the Free Application for Federal Student Aid (FAFSA). SARs contain responses to more than 100 questions regarding students' financial resources and family structure, including family income, parents' marital status, and parents' education. Roughly 3% of scholarship applicants are undocumented immigrants, who are ineligible for federal financial aid and therefore cannot file the FAFSA. STBF permits these students to submit an alternate form called the College Funding Estimator (CFE). The CFE is published by the EducationQuest Foundation, a nonprofit organization in Nebraska, and gathers a similar, though less detailed, set of information.

Neither SARs nor CFEs report students' race, and the scholarship application did not collect this variable until the 2014 cohort. Supplemental data on race were obtained from the Nebraska Department of Motor Vehicles. Over 85% of the randomization sample was successfully matched to driver's license records.

A.B. Financial Aid Data

Nebraska's public colleges and universities provided detailed information on their students' financial aid packages. These data report costs of attendance, grants, loans, and Federal Work Study aid. While all schools report federal loans, most do not report private loans, which may be obtained directly from lenders without involving financial aid officers. We therefore exclude private loans from our analysis. For most STBF applicants, federal loans offer the lowest available interest rate and therefore account for the vast majority of borrowing. [Figure I](#) reports various kinds of aid distributed in the first academic year following scholarship application.

1. *Cost of Attendance.* Publicly available IPEDS institutional characteristics data were used to estimate a sticker price of

25. In Nebraska, the majority of students take the ACT rather than the SAT. In 2012–13, 70% of Nebraska high school students took the ACT, compared with the national average of 52%.

college for every student in the experimental sample. The sticker price calculation includes in-state tuition, fees, and a books and supplies stipend. The yearly institutional characteristics dataset from IPEDS has nearly full coverage of tuition and fees for schools attended by students in the experimental sample. There is only one school for which we do not have tuition and fees—an out-of-state certificate school. This school's cost of attendance varies greatly based on certificate program, so we drop the student from the sample.

The IPEDS data are missing a books and supplies cost value for 8% of the sample. In these cases, we use the mean books and supplies cost for students enrolled in the same calendar year and college type (four-year or two-year and for-profit or nonprofit).

We calculate each student's sticker price by matching credits attempted per term to per credit costs at the school attended in that year. Importantly, we use credits attempted, as opposed to credits earned, because a student is charged for attempted credits. IPEDS has nearly full coverage of cost per credit for schools attended by the experimental sample. Every school that reports tuition also reports cost per credit. We calculate the total cost based on credits attempted for each student at each school. When this credit-based cost exceeds the school's reported tuition, the cost variable is assigned the full-time tuition value. Each student's sticker price is then estimated by summing credits-based cost per term, a books and supplies stipend, and the school-reported fees in each academic year.

A.C. Education Outcome Variables

Over 90% of experimental subjects enrolled in a Nebraska public college or university. We match STBF applicants to administrative data provided by these schools using names, dates of birth, and the last four digits of Social Security Numbers (SSNs). To measure enrollment at out-of-state and private institutions, we match applicants to National Student Clearinghouse (NSC) data using names and dates of birth. Though the NSC captures more than 91% of enrollment nationwide (and more than 99% at four-year public institutions), its name-based match has limitations, as [Dynarski, Hemelt, and Hyman \(2015\)](#) detail. Roughly 4% of experimental applicants have enrollment at Nebraska's public colleges and universities that does not appear in

the NSC-matched sample. These students are disproportionately nonwhite.

1. *Enrollment Measures.* The enrollment outcomes used for this article are dummy variables indicating type of institution enrolled. Table II, for example, reports effects on the probability of enrollment in the first postapplication year for two- and four-year schools and schools in various sectors. We define follow-up windows to match the start and end dates of each academic year based on individually published academic calendars at each school. This year covers the period from the beginning of the fall term to the end of the last summer term of an applicant's school in the year following the application. We use similar timing conventions from the NSC. In each window, we force binary enrollment outcomes to be mutually exclusive. Students who enroll at both two- and four-year institutions are coded as having "any four-year" enrollment. Likewise, those who enroll at in-state public colleges do not contribute to the out-of-state or private categories.

We also track cumulative credit completion. Most credit data come from Nebraska's public colleges and universities. Credits for the 7% of applicants who attend out-of-state or private colleges are imputed using the NSC's coarse enrollment status variable: an indicator for whether students were enrolled full-time, half-time, or less than half-time. Imputed credits is the predicted value from a regression of credits on enrollment status, degree program, academic term, and cohort. Fewer than 2% of applicants attend out-of-state or private schools that do not report the full-time enrollment indicator to the NSC. These students are coded as enrolled full-time when the full-time enrollment share at their chosen school is at least 85%, as reported by IPEDS.

Annual enrollment is coded as follows. A student is coded as enrolled in the first postapplication year if they completed credits at some point during this year, either in the fall, spring, or summer term. To be coded as enrolled in year 2+, a student must be coded as enrolled in fall, spring, or summer of the academic year beginning 2+ years after their STBF application year. If a student is enrolled in year 2+, there is no requirement to be enrolled in year one. Figures that plot term-wise enrollment show enrollment in either a fall or spring term, where the fall term includes both fall and winter

terms and the spring term includes both spring and summer terms.

2. *Years of Schooling Data.* Years of schooling variables are term counts derived from term-wise enrollment status as reported by Nebraska's public colleges and universities, or in the NSC when the former are not available. These indicate "attempted enrollment" at an institution (as opposed to measuring credits completed). Using data from the NSC-matched sample, students are coded as enrolled in a given term if the NSC records them as enrolled at any level in any institution in a particular term.

3. *Degree Data.* Degree completion indicators come from Nebraska's public colleges and universities, or the NSC when the former are not available. NSC and the colleges report completion of associate degrees and bachelor's degrees for each student and the year and term in which degree requirements were met. Figures show degree completion by year and term, while tables report treatment effects on year six completion. Degree completion dates are likewise coded from term-wise information on completion. A student is coded as having completed a degree in year six if they earned a degree in either the fall, spring, or summer term of that academic year.

APPENDIX B: CONSTRUCTION OF FIGURE VI

Points plotted in [Figure VI](#) are the average reduced-form and first-stage coefficients associated with [equations \(2\)](#) and [\(3\)](#). The setup used to compute these allows each element of X_i to interact with A_i in the instrument list, but higher-order terms (such as an interaction between strata, GPA, and A_i) are omitted. Because the reference groups for dummy variables need not be of intrinsic interest, the figure plots sample average values of $\hat{\rho}(X_i)$ and $\hat{\pi}(X_i)$, conditioning on membership in the groups for which degree effects are plotted in [Figures III, IV](#), and [Online Appendix Figure A4](#). Interaction terms appear together in the instrument list, but the averages in the figure are plotted one covariate at a time.

A simplified example illuminates the nature of these average effects. Suppose there are three strata, coded $S_i \in \{1, 2, 3\}$ and a single Bernoulli covariate, F_i . The corresponding covariate vector is $X_i = [S_i^1 S_i^2 F_i]'$ where $S_i^j = 1[S_i = j]$. So the reference group for S_i is 3.

The reduced form in this case can be written:

$$\begin{aligned}
 (8) \quad Y_i &= X_i \delta + \rho_0 A_i + \theta_1 S_i^1 A_i + \theta_2 S_i^2 A_i + \phi F_i A_i + \varepsilon_i \\
 &= X_i \delta + A_i [\rho_0 + \theta_1 S_i^1 + \theta_2 S_i^2 + \phi F_i] + \varepsilon_i \\
 &= X_i \delta + A_i \rho(X_i) + \varepsilon_i.
 \end{aligned}$$

This model implies

$$\begin{aligned}
 (9) \quad E[\rho(X_i)|S_i = 1] &= \rho_0 + \theta_1 + \phi E[F_i|S_i = 1] \\
 E[\rho(X_i)|S_i = 2] &= \rho_0 + \theta_2 + \phi E[F_i|S_i = 2] \\
 E[\rho(X_i)|S_i = 3] &= \rho_0 + \phi E[F_i|S_i = 3]
 \end{aligned}$$

and

$$\begin{aligned}
 (10) \quad E[\rho(X_i)|F_i = 1] &= \rho_0 + \theta_1 E[S_i^1|F_i = 1] + \theta_2 E[S_i^2|F_i = 1] + \phi \\
 E[\rho(X_i)|F_i = 0] &= \rho_0 + \theta_1 E[S_i^1|F_i = 0] + \theta_2 E[S_i^2|F_i = 0].
 \end{aligned}$$

Note that reference groups for each categorical conditioning variable have different effects. Specifically,

$$(11) \quad E[\rho(X_i)|S_i = 3] \neq E[\rho(X_i)|F_i = 0].$$

Neither of these equal the award main effect, ρ_0 .

In this example, 2SLS estimates are identified by exclusion of the four-instrument set $Z_i = \{A_i S_i^1 A_i S_i^2 A_i F_i A_i\}$ from [equation \(2\)](#). Substituting [equation \(3\)](#) in [equation \(2\)](#) shows, for example, that the marginal sample mean reduced form and first stage satisfy:

$$(12) \quad \hat{E}[\rho(X_i)|S_i] = \hat{E}[\pi(X_i)|S_i] \mu_1,$$

with a similar proportionality relation obtained by conditioning on F_i . So, a version of [Figure VI](#) for this example has five points, three for the values of S_i and two for the values of F_i .

MASSACHUSETTS INSTITUTE OF TECHNOLOGY AND NATIONAL BUREAU
OF ECONOMIC RESEARCH, UNITED STATES
MASSACHUSETTS INSTITUTE OF TECHNOLOGY AND NATIONAL BUREAU
OF ECONOMIC RESEARCH, UNITED STATES
HARVARD UNIVERSITY AND NATIONAL BUREAU OF ECONOMIC RE-
SEARCH, UNITED STATES

SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

DATA AVAILABILITY

Code replicating the tables and figures in this article can be found in Angrist, Autor, and Pallais (2022) in the Harvard Dataverse, <https://doi.org/10.7910/DVN/DCTHNS>.

REFERENCES

- Abadie, Alberto, "Bootstrap Tests for Distributional Treatment Effects in Instrumental Variable Models," *Journal of the American Statistical Association*, 97 (2002), 284–292.
- Abdulkadiroglu, Atila, Joshua Angrist, and Parag A. Pathak, "The Elite Illusion: Achievement Effects at Boston and New York Exam Schools," *Econometrica*, 82 (2014), 137–196.
- Abdulkadiroglu, Atila, Joshua D. Angrist, Yusuke Narita, and Parag A. Pathak, "Research Design Meets Market Design: Using Centralized Assignment for Impact Evaluation," *Econometrica*, 85 (2017), 1373–1432.
- ACT, "ACT National Profile Reports 2012–2016," ACT Technical report, 2012.
- Anderson, Drew M., Katharine M. Broton, Sara Goldrick-Rab, and Robert Kelchen, "Experimental Evidence on the Impacts of Need-Based Financial Aid: Longitudinal Assessment of the Wisconsin Scholars Grant," *Journal of Policy Analysis and Management*, 39 (2019), 720–739.
- Andrews, Rodney J., Scott A. Imberman, and Michael F. Lovenheim, "Recruiting and Supporting Low-Income, High-Achieving Students at Flagship Universities," *Economics of Education Review*, 74 (2020), 101923.
- Angrist, Josh, David Autor, Sally Hudson, and Amanda Pallais, "Evaluating Post-Secondary Aid: Enrollment, Persistence, and Projected Completion," NBER Working Paper no. 23015, 2016.
- Angrist, Joshua, David Autor, and Amanda Pallais, "Replication Data for: 'Marginal Effects of Merit Aid for Low-Income Students,'" (2022), Harvard Dataverse, <https://doi.org/10.7910/DVN/DCTHNS>.
- Angrist, Joshua D., and Guido W. Imbens, "Two-Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity," *Journal of the American Statistical Association*, 90 (1995), 431.
- Angrist, Joshua D., Guido W. Imbens, and Donald B. Rubin, "Identification of Causal Effects Using Instrumental Variables," *Journal of the American Statistical Association*, 91 (1996), 444–455.

- Angrist, Joshua D., Daniel Lang, and Philip Oreopoulos, "Incentives and Services for College Achievement: Evidence from a Randomized Trial," *American Economic Journal: Applied Economics*, 1 (2009), 136–163.
- Angrist, Joshua D., Philip Oreopoulos, and Tyler Williams, "When Opportunity Knocks, Who Answers? New Evidence on College Achievement Awards," *Journal of Human Resources*, 49 (2014), 572–610.
- Angrist, Joshua D., and Jörn-Steffen Pischke, *Mostly Harmless Econometrics: An Empiricist's Companion* (Princeton, NJ: Princeton University Press, 2009).
- Appleton, James J., Sandra L. Christenson, and Michael J. Furlong, "Student Engagement with School: Critical Conceptual and Methodological Issues of the Construct," *Psychology in the Schools*, 45 (2008), 369–386.
- Avery, Christopher, and Sarah Turner, "Student Loans: Do College Students Borrow Too Much—Or Not Enough?," *Journal of Economic Perspectives*, 26 (2012), 167–192.
- Barr, Andrew, "Fighting for Education: Veterans and Financial Aid," *Journal of Labor Economics*, 37 (2019), 509–544.
- Bettinger, Eric, Oded Gurantz, Laura Kawano, Bruce Sacerdote, and Michael Stevens, "The Long-Run Impacts of Financial Aid: Evidence from California's Cal Grant," *American Economic Journal: Economic Policy*, 11 (2019), 64–94.
- Bettinger, Eric P., and Rachel Baker, "The Effects of Student Coaching in College: An Evaluation of a Randomized Experiment in Student Mentoring," *Educational Evaluation and Policy Analysis*, 36 (2014), 3–19.
- Bettinger, Eric P., and Brent J. Evans, "College Guidance for All: A Randomized Experiment in PreCollege Advising," *Journal of Policy Analysis and Management*, 38 (2019), 579–599.
- Bloom, Dan, and Colleen Sommo, "Building Learning Communities: Early Results from the Opening Doors Demonstration at Kingsborough Community College," *Educational Evaluation and Policy Analysis*, 37 (2015), 520–541.
- Bulman, George, "The Effect of Access to College Assessments on Enrollment and Attainment," *Journal of Applied Econometrics*, 7 (2015), 1–36.
- Bulman, George, Robert W. Fairlie, Sarena Goodman, and Adam Isen, "Parental Resources and College Attendance: Evidence from Lottery Wins," *American Economic Review*, 111 (2021), 1201–1240.
- Card, David, "Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems," *Econometrica*, 69 (2001), 1127–1160.
- Carrell, Scott, and Bruce Sacerdote, "Why Do College-Going Interventions Work?" *American Economic Journal: Applied Economics*, 9 (2017), 124–151.
- Carruthers, Celeste K., and Umut Ozek, "Losing HOPE: Financial Aid and the Line between College and Work," *Economics of Education Review*, 53 (2016), 1–15.
- Castleman, Benjamin L., and Bridget Terry Long, "Looking beyond Enrollment: The Causal Effect of Need-Based Grants on College Access, Persistence, and Graduation," *Journal of Labor Economics*, 34 (2016), 1023–1073.
- Cohodes, Sarah R., and Joshua S. Goodman, "Merit Aid, College Quality, and College Completion: Massachusetts' Adams Scholarship as an In-Kind Subsidy," *American Economic Journal: Applied Economics*, 6 (2014), 251–285.
- Cole, Darnell, K. C. Culver, Joseph Kitchen, Gwendelyn Rivera, and Elise Swanson, "How and Why a Comprehensive College Transition Program Works: Promoting At-Promise Students' Success in the Thompson Scholars Learning Communities," Pullias Center for Higher Education report, 2020.
- Coordinating Commission for Postsecondary Education, "Nebraska Higher Education Progress Report," Coordinating Commission for Postsecondary Education technical report, 2013.
- Deming, David J., and Susan M. Dynarski, "Into College, Out of Poverty? Policies to Increase the Postsecondary Attainment of the Poor," in *Targeting Investments in Children: Fighting Poverty When Resources Are Limited*, Phillip B. Levine and David J. Zimmerman, eds. (Chicago: University of Chicago Press, 2010), 283–302.

- Deming, David J., and Christopher R. Walters, "The Impact of Price Caps and Spending Cuts on U.S. Postsecondary Attainment," NBER Working Paper no. 23736, 2017.
- Denning, Jeffrey T., Benjamin M. Marx, and Lesley J. Turner, "ProPelled: The Effects of Grants on Graduation, Earnings, and Welfare," *American Economic Journal: Applied Economics*, 11 (2019), 193–224.
- Dynarski, Susan M., "Hope for Whom? Financial Aid for the Middle Class and Its Impact on College Attendance," *National Tax Journal*, 53 (2000), 629–662.
- Dynarski, Susan M., Steven W. Hemelt, and Joshua M. Hyman, "The Missing Manual: Using National Student Clearinghouse Data to Track Postsecondary Outcomes," *Educational Evaluation and Policy Analysis*, 37 (2015), 53S–79S.
- Dynarski, Susan M., and Judith Scott-Clayton, "Complexity and Targeting in Federal Student Aid: A Quantitative Analysis," in *Tax Policy and the Economy*, James Poterba, ed. (Chicago: University of Chicago Press, 2008), 109–150.
- Fitzpatrick, Maria D., and Damon Jones, "Higher Education, Merit-Based Scholarships and Post-Baccalaureate Migration," *Economics of Education Review*, 54 (2016), 155–172.
- Fuller, Win, Charles F. Manski, and David A. Wise, "Enrollment Effects of the BEOG Program," in *College Choice in America*, Charles F. Manski and David A. Wise, eds. (Cambridge, MA: Harvard University Press, 1983), 118–128.
- Goodman, Joshua, Oded Gurantz, and Jonathan Smith, "Take Two! SAT Retaking and College Enrollment Gaps," *American Economic Journal: Economic Policy*, 12 (2020), 115–158.
- Hansen, W. Lee, "Impact of Student Financial Aid on Access," *Proceedings of the Academy of Political Science*, 35 (1983), 84–96.
- Harris, Douglas N., and Jonathan Mills, "Optimal College Financial Aid: Theory and Evidence on Free College, Early Commitment, and Merit Aid from an Eight-Year Randomized Control Trial." Ed Working Paper no. 21-393, 2021.
- Hendren, Nathaniel, and Ben Sprung-Keyser, "A Unified Welfare Analysis of Government Policies," *Quarterly Journal of Economics*, 135 (2020), 1209–1318.
- Hilger, Nathaniel G., "Parental Job Loss and Children's Long-Term Outcomes: Evidence from 7 Million Fathers' Layoffs," *American Economic Journal: Applied Economics*, 8 (2016), 247–283.
- Imbens, Guido W., and Joshua D. Angrist, "Identification and Estimation of Local Average Treatment Effects," *Econometrica*, 62 (1994), 467–475.
- Kane, Thomas J., "Lessons from the Largest School Voucher Program Ever: Two Decades of Experience with Pell Grants," in *Who Chooses? Who Loses? Culture, Institutions, and the Unequal Effects of School Choice*, Bruce Fuller, Richard Elmore, and Gary Orfield, eds. (New York: Teachers College Press, 1996).
- , "A Quasi-Experimental Estimate of the Impact of Financial Aid on College-Going," NBER Working Paper, 2003.
- Kezar, Adrianna, and Joseph A. Kitchen, "Supporting First-Generation, Low-Income, and Underrepresented Students' Transitions to College through Comprehensive and Integrated Programs," *American Behavioral Scientist*, 64 (2020), 223–229.
- Levin, Henry M., and Emma García, "Accelerating Community College Graduation Rates: A Benefit–Cost Analysis," *Journal of Higher Education*, 89 (2018), 1–27.
- Mayer, Alexander K., Reshma Patel, and Melvin Gutierrez, "Four-Year Effects on Degree Receipt and Employment Outcomes from a Performance-Based Scholarship Program in Ohio," MDRC report, 2015.
- Miller, Cynthia, Camielle Headlam, Michelle Manno, and Dan Cullinan, "Increasing Community College Graduation Rates with a Proven Model: Three-Year Results from the Accelerated Study in Associate Programs (ASAP) Ohio Demonstration," MDRC report, 2020.
- Oreopoulos, Philip, and Kjell G. Salvanes, "Priceless: The Nonpecuniary Benefits of Schooling," *Journal of Economic Perspectives*, 25 (2011), 159–184.

- Page, Lindsay C., and Judith Scott-Clayton, "Improving College Access in the United States: Barriers and Policy Responses," *Economics of Education Review*, 51 (2016), 4–22.
- Park, Jin Huem, "Estimation of Sheepskin Effects and Returns to Schooling Using the Old and the New CPS Measures of Educational Attainment," Princeton University, Department of Economics, Industrial Relations Section Working Papers 717, 1994.
- Scott-Clayton, Judith, "On Money and Motivation: A Quasi-Experimental Analysis of Financial Incentives for College Achievement," *Journal of Human Resources*, 46 (2011), 614–646.
- Scott-Clayton, Judith, and Lauren Schudde, "The Consequences of Performance Standards in Need Based Aid: Evidence from Community Colleges," *Journal of Human Resources*, 55 (2020), 1105–1136.
- Scott-Clayton, Judith, and Basit Zafar, "Financial Aid, Debt Management, and Socioeconomic Outcomes: Post-College Effects of Merit-Based Aid," *Journal of Public Economics*, 170 (2019), 68–82.
- Scrivener, Susan, Michael J. Weiss, Alyssa Ratledge, Timothy Rudd, Colleen Sommo, and Hannah Fresques, "Doubling Graduation Rates: Three-Year Effects of CUNY's Accelerated Study in Associate Programs (ASAP) for Developmental Education Students," MDRC report, 2015.
- Sjoquist, David, and John Winters, "State Merit Aid Programs and College Major: A Focus on STEM," *Journal of Labor Economics*, 33 (2015), 973–1003.
- U.S. Department of Education, "Integrated Postsecondary Education Data System: Institutional Characteristics File," 2019.
- Weiss, Michael J., Mary G. Visher, Evan Weissman, and Heather D. Wathington, "The Impact of Learning Communities for Students in Developmental Education: A Synthesis of Findings from Randomized Trials at Six Community Colleges," *Education Evaluation and Policy Analysis*, 37 (2015), 520–541.
- Zimmerman, Seth D., "The Returns to College Admission for Academically Marginal Students," *Journal of Labor Economics*, 32 (2014), 711–754.