NBER WORKING PAPER SERIES

MULTIFACETED AID FOR LOW-INCOME STUDENTS AND COLLEGE OUTCOMES: EVIDENCE FROM NORTH CAROLINA

Charles T. Clotfelter
Steven W. Hemelt
Helen F. Ladd

Working Paper 22217
http://www.nber.org/papers/w22217

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2016

The research reported here was supported by the National Center for Analysis of Longitudinal Data in Education Research (CALDER) and the Gates Foundation. We are very grateful to Shirley Ort, Associate Provost and Director of Scholarships and Student Aid, and Lynn Williford, Assistant Provost for Institutional Research and Assessment, for sharing generously of their expertise and deep institutional knowledge with the research team – and for answering many questions about data and programmatic elements of the Carolina Covenant. We benefited from suggestions and comments from seminar participants at Stanford University, University of Michigan, and the fall 2015 meetings of the Association for Public Policy Analysis and Management (APPAM) in Miami, FL. We are grateful to Dan Cohen-Vogel and Eric Zwieg for many helpful conversations about data. We thank Yang Zhou, Cindy Wu, and Ying Shi for excellent research assistance. Clotfelter can be reached at charles.clotfelter@duke.edu; Hemelt (contact author) at hemelt@email.unc.edu; and Ladd at hladd@duke.edu. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

Multifaceted Aid for Low-Income Students and College Outcomes: Evidence from North Carolina

Charles T. Clotfelter, Steven W. Hemelt, and Helen F. Ladd

NBER Working Paper No. 22217
May 2016
JEL No. I22,I23

ABSTRACT

Launched in 2004, the Carolina Covenant combines grant-heavy financial aid with an array of non-financial supports for low-income students at an elite public university. We find that the program increased four-year graduation rates by about 8 percentage points for eligible students in the cohorts who experienced the fully developed program. For these cohorts, we also find suggestive effects on persistence to the fourth year of college, cumulative earned credits, and academic performance. We conclude that aid programs targeting low-income, high-ability students are most successful when they couple grant aid with strong non-financial supports.

Charles T. Clotfelter
Sanford School of Public Policy
Duke University
Rubenstein Hall Room 178
201 Science Drive
Box 90312
Durham, NC 27708
and NBER
charles.clotfelter@duke.edu

Steven W. Hemelt
Department of Public Policy
University of North Carolina - Chapel Hill
Abernethy Hall, Campus Box 3435
Chapel Hill, NC 27599
hemelt@email.unc.edu

Helen F. Ladd
Duke University
201 Science Drive
Box 90245
Durham, NC 27708
helen.ladd@duke.edu

I. Introduction

In addition to being less likely than their more affluent peers to enter college, low-income students are also less likely to graduate after they enroll. Moreover, these income-based gaps in college completion have grown larger in recent decades (Bailey & Dynarski, 2011). This widening occurred despite attempts to improve postsecondary access and success for low-income students. The number of students receiving need-based federal Pell grants and the average, per-student value of those awards steadily increased from the mid-1990s to 2011 (Dynarski & Scott-Clayton, 2013). In addition, states have doubled expenditures on grant aid to students since 1980 (Dynarski & Scott-Clayton, 2013), but an increasing share of that aid, is not need-based.[1] Recent policy discussions have centered on the genesis and evolution of income-based gaps in college enrollment and completion, as well as on ways postsecondary institutions can improve outcomes for the low-income students they enroll.

In this paper, we study a multifaceted aid program for low-income college-enrollees implemented by a leading public flagship institution. In the fall of 2003, the University of North Carolina at Chapel Hill (UNC-CH) announced a new aid program for low-income students who had gained admission through the standard process. This program was specifically designed to promote their successful progress through college and ultimate graduation. Beginning with students entering UNC-CH in the fall of 2004, this aid program, dubbed the "Carolina Covenant®," would cover the financial costs of college attendance – without loans – and provide the "Covenant scholars" with additional support services, such as mentoring by faculty and peers, career advice, professional development opportunities, and social events. We examine the effects of the Carolina Covenant on various markers of college success: earned credits, grades,

[1] As of 2012-2013, about 39% of all state-based grant aid dispersed to undergraduates included a merit component (NASSGAP, 2014).

and graduation. Our results document the potential for need-based aid from the university itself to improve the success of low-income college students. In addition, our work contributes to ongoing debates about the effectiveness of financial aid relative to other types of support for student success in college.

The next section situates our paper and its contributions within the literature that explores the effects of college costs, information, and other supports on students' postsecondary outcomes. We focus particularly on studies that examine outcomes for enrolled students. Section III describes the components and implementation of the Carolina Covenant. Section IV describes the data and our approach to estimating causal effects of the program. Section V presents our main findings; section VI examines the costs of the program; and section VI concludes with a discussion of policy implications.

II. Existing Literature

Previous research has explored the effects of need-based aid on the postsecondary attendance choices of students (e.g., Dynarski, 2003; Deming & Dynarski, 2010), the influence of tuition costs on college enrollment (e.g., Hemelt & Marcotte, 2011; Heller, 1996; Kane, 1995), and the effects of merit-based scholarships on postsecondary enrollment and choice (e.g., Bruce & Carruthers, 2014; Scott-Clayton, 2011). Much less attention has been paid to the influence of such factors (and other supports) on outcomes beyond college enrollment such as short-run persistence, academic performance, and college completion.[2] We summarize here first the studies that examine such effects of financial support, second those that focus on the effects of non-financial supports, and third the few that study programs combining both types of support. The Carolina Covenant program includes financial and non-financial assistance.

[2] For an excellent synthesis of recent research that explores ways to improve college access in the United States, see Page and Scott-Clayton (2015).

A. Need- and Merit-Based Aid: Effects on Postsecondary Persistence and Completion

Examining the effects of merit aid on college completion, Dynarski (2008) found that merit-aid programs in Arkansas and Georgia increased the share of young people with a college degree by 3 percentage points, with the largest effects accruing to women. She notes, however, that her reduced-form approach cannot "separately identify the effect of aid on entry and persistence conditional on entry" (p. 579). Further, she highlights the fact that her findings suggest that "scholarships alone will not keep the bulk of dropouts from leaving college" (p. 579). Even with generous merit aid, a sizeable share of entering college students fail to persist to completion.[3]

Scott-Clayton (2011) and Bruce and Carruthers (2014) studied the effects of merit-based, statewide aid programs in West Virginia and Tennessee, respectively, on measures of college success. In both cases, the authors exploited a set of transparent eligibility criteria based on ACT scores and GPAs to estimate scholarship effects. The West Virginia program fully covered tuition and fees for up to four years of college for first-time, full-time college attendees at public in-state institutions (or an equivalent amount at in-state privates). The Tennessee scholarship awarded up to three-fourths of the cost of tuition and fees at in-state public institutions, a quarter of the analogous cost at in-state private institutions.

Scott-Clayton (2011) focused on college enrollees and explored effects on credit accumulation and persistence, whereas Bruce and Carruthers (2014) focused on high school graduates and explored effects along the extensive margin, that is, where students enrolled. Scott-Clayton (2011) found that eligibility for the West Virginia scholarship increased the share of individuals in the state with a bachelor's degree by 1.8 to 2.3 percentage points (i.e., between

[3] Sjoquist and Winters (2012) reevaluate different samples of the 2000 Census used by Dynarski (2008) and find little overall impact of merit aid on college completion. This finding further highlights the need to explore impacts of multifaceted aid packages on postsecondary persistence and completion.

8 and 10 percent). In addition, she argued that the requirements for renewing the scholarship – a minimum GPA and number of credits – drove the main effects. That is, compared to their ineligible counterparts, students eligible for the scholarship were substantially more likely to take and complete 30 or more credits during each of their first three years of college. Such effects on key intermediate outcomes kept the treatment group on track to graduate.

These studies all pertain to merit-based aid programs. Whether we should expect similar effects for need-based aid programs is unclear given that merit- and need-based programs often differ by target population, amount of aid, delivery mechanisms, and incentives. To date, with a few notable exceptions, there is little evidence on how need-based aid affects outcomes beyond the college-entry margin. Using student-level data from Ohio, Bettinger (2004) exploited discontinuities in students' Pell grant awards (based on family size and number of children attending college) to estimate effects on persistence beyond the first year of college. He found that a $1,000 increase in Pell award led to roughly a 4 percentage point decrease in the likelihood that a student would drop out of college (p.28). In an experimental context, Goldrick-Rab et al. (2012) focused on first-year Pell grant recipients at Wisconsin public colleges and found that additional need-based aid increased the likelihood students completed a full-time credit load as well as persistence to the second year of college. Castleman and Long (2013) used data on high school seniors in Florida during the 2000-2001 academic year to examine effects of additional need-based aid (above the Pell grant) on college enrollment, persistence, credit accumulation, and graduation. Comparing students who received some aid (i.e., a Pell grant) to those who were eligible to receive additional aid on top of Pell, they found that an additional $1,000 of need-based aid (in 2000 dollars) increased continual enrollment through the spring semester of

freshman year and bachelor's degree completion within four years by 3.3 and 3.5 percentage points, respectively (pp. 27-28).

In contrast to Scott-Clayton (2011), Castleman and Long (2013) contend that their findings are driven "much more by the reduction in cost of attendance at public, four-year institutions than by possible incentive effects" (p. 29). Of course, the populations of students eligible for the types of aid studied in these two papers differ along important dimensions, such as academic preparation and severity of financial need.[4] Still, our reading of this set of evidence is that money and incentives both matter for students' progress and success in college – as does how they are combined in specific aid programs.

B. Non-Pecuniary Interventions to Improve Postsecondary Persistence and Completion

A second line of recent research has focused on non-pecuniary interventions, such as college counseling and information provision. At present, much of this work explores the ability of such interventions to influence students' choices about college-going – whether by improving student-institution matches, (Hoxby & Turner, 2014), encouraging students to apply for need-based financial aid (Bettinger et al., 2012), providing better information about the costs and payoffs of college (Oreopoulos & Dunn, 2012), or counseling students through the college transition process (Castleman, Page, & Schooley, 2014; Castleman & Page, 2015).

Two of these studies examine short-run persistence (Bettinger et al., 2012; Castleman & Page, 2014). Castleman and Page (2014) found that students who were offered summer counseling were 5 percentage points more likely to be continuously enrolled through three semesters of college. Bettinger and colleagues (2012) found that high school seniors whose parents were offered assistance with filing the Free Application for Federal Student Aid

[4] Recent evidence shows how additional financial aid affects non-traditional, older students. Barr (2014) exploits the expansion of financial aid for college via the Post-9/11 GI Bill and finds that additional aid increases the likelihood a veteran obtains a bachelor's degree within six years of enrollment by 6 percentage points (30 percent).

(FAFSA) were 8 percentage points more likely to have completed two years of college (during the first three years after the experiment). These findings highlight the capacity of interventions mainly targeted at improving access to college (or higher-quality postsecondary options) to encourage persistence.

C. Mixed Aid Programs and Postsecondary Success

A few emerging studies provide evidence about the importance of mixed financial and non-financial supports. Two focus on post-enrollment college outcomes (Scrivener et al., 2015; Angrist et al., 2009) and one begins with high school seniors to explore effects on postsecondary access and persistence (Page et al., 2016). MDRC conducted a randomized evaluation of a program fielded in New York called Accelerated Study in Associate Programs (ASAP), which included financial supports like tuition waivers, free metro cards, and free textbooks, along with non-financial supports such as advising, career services, and seminars. The authors found that ASAP increased persistence, credit accumulation, and graduation – with 40 percent of the treatment group earning an associate's degree by the end of the three-year period compared to just 22 percent of the control group (Scrivener et al., 2015, p. 51).

Angrist, Lang, and Oreopoulos (2009) tested the relative efficacy of financial incentives, non-financial supports, and a mix of both financial and non-financial assistance in the context of an experiment at a large Canadian university. The programs targeted first-time freshman in the bottom three-quarters of the achievement distribution (defined by high school GPA). One group was offered an array of non-financial supports, including peer mentoring and supplemental tutoring. A second group was offered financial awards equivalent to about one year's tuition that were tied to academic performance. A third group was offered access to both the non-financial supports and the financial incentives. All treatments were only available during the first year of

college. Angrist and colleagues (2009) explored the effects of these treatments on academic performance and credits earned during the first two years of college. They found that the combined treatment increased students' GPA and number of earned credits, relative to the control group, but detected little effect of either the financial or non-financial treatment in isolation. Notably, the positive effects were driven entirely by women.[5] By the second year of college, women in the treatment group earned about 0.5 more credits and had GPAs that were higher than their control-group counterparts by about 0.28 points (pp. 159-160). The authors do not look at effects on college completion.

Finally, within the U.S. context, Page, Castleman, and Sahadewo (2016) study the effects of the Dell Scholars Program which provides generous financial support and individualized, virtual advising to low-income, college-intending high school students. The authors find suggestive effects of scholarship receipt on postsecondary persistence and marked impacts on college completion (i.e., a 25 percent increase in four-year and six-year bachelor's degree attainment rates). Eligibility for the Dell scholarship is based on financial need, evidence of overcoming adversity, and modest academic requirements.

D. Contributions

Our research explores the effects of a need-based aid program that combines financial and non-financial supports on post-enrollment college outcomes, including completion. The Carolina Covenant is targeted at high-achieving, low-income students and the manner in which it is delivered allows us to look at the effects of mixed aid awarded after admission and enrollment on measures of postsecondary success. Further, we explore these questions at a highly selective

[5] The positive effects of the combined treatment on female students' GPAs and credit accumulation persisted into the second year of college, when the treatments were no longer in operation. Thus, this suggests that financial incentives in conjunction with non-financial supports led to lasting improvements in academic and study skills (Angrist et al., 2009, p. 160).

public flagship institution, a particularly important setting given the recent finding that earnings are higher for students who attend a state flagship university (Hoekstra, 2009).

Our study contributes to the literature in three ways. First, it examines the capacity of institutional aid and programmatic support to influence the likelihood that low-income students persist and complete college. Most of the existing research examines aid provided through federal or state aid programs, or through private philanthropic foundations. Although federal and state programs are important and have the potential to reach large numbers of students, aid programs awarded by particular institutions to low-income students also may contribute to student success in college. Second, because the Carolina Covenant includes both financial and non-pecuniary elements, our findings add to the nascent literature on the effects of aid programs that mix financial support with incentives and wrap-around services for low-income students. Finally, we are able to illustrate ways in which the choice of estimating approach affects the treatment-control contrast of interest and thereby the research question being asked. This comparison and discussion highlight the importance for researchers of paying careful attention to the choice of study design when multiple quasi-experimental approaches are possible.

III. The Carolina Covenant

The Carolina Covenant aid program at UNC-CH offers low-income students financial aid packages without loans. The program was announced in the fall of 2003, and the first cohort of awardees entered UNC-CH the following fall, making the University of North Carolina at Chapel Hill the first public institution in the country to award loan-free financial aid packages to low-income students (Fiske, 2010). Virginia and then a number of other public universities soon followed suit, with similar programs.[6]

[6] For a much more detailed exploration of the origins of the Carolina Covenant, its components, and evolution over time, see Fiske (2010).

A. Brief History of the Carolina Covenant

Drawing on research that highlighted low-income families' particular wariness of loans and general unfamiliarly with postsecondary aid systems, the designers of the Covenant specified that a) the aid packages contain no loans; and b) the eligibility criteria be simple and clear (Fiske, 2010, pp. 24-25). Though the loan-free financial aid award is the most visible part of the Covenant, the program was also designed to address other barriers to success in college faced by many high-ability students from low-income families, including "lack of informed guidance from parents unfamiliar with the ways of higher education" (Fiske, 2010, p. 18). We discuss these components of the program below.

The first cohort of Covenant scholars, those who matriculated in the fall of 2004, totaled 224 students. Over the ensuing years, this number grew, rising to 558 students in fall of 2010, the most recent cohort of scholars for whom we have data. In total, these recipients comprise about 10 percent of all undergraduates at UNC-CH and hail overwhelmingly from North Carolina.

B. Eligibility and Components of the Covenant

The Carolina Covenant is open to incoming freshman and transfer students who meet a basic set of enrollment and financial criteria: First, a student must be admitted to UNC-CH and be pursuing her first undergraduate degree in a full-time capacity. Second, the student must be dependent on parents whose adjusted gross income (AGI) is no higher than 200% of the federal poverty guidelines, an amount that varies by family size.[7] Third, the student's family must have limited assets and other resources to pay for college as well as have a low income.[8] For the 2015-

[7] For the first cohort of Covenant scholars (i.e., fall of 2004), the cutoff was 150% of the federal poverty line. In all subsequent years the cutoff was 200%. Eligibility is determined using family income from the prior tax year: For example, for incoming freshman in the 2015-2016 academic year, 2014 income records are used.

[8] The student must complete the Free Application for Federal Student Aid (FAFSA) and the College Board's PROFILE application. In fact, all applicants to UNC-CH are required to fill out the College Board's profile if they want to be considered for (any) financial aid.

2016 academic year, a student from a family of four with a 2014 income of $47,700 or less would be eligible to receive the Carolina Covenant.[9]

The majority of Covenant scholars find out about their award after accepting admission but prior to enrollment. Family income and asset claims are verified by the UNC Office of Scholarships and Student Aid after admission and before the Covenant is officially awarded. In practice, this means the total number of Covenant scholars fluctuates by a few students over the first several weeks of classes each year.[10] Each spring, Covenant recipients must renew their funding for the next year by submitting the required paperwork. Criteria for renewal are simple: the student must continue to meet the income requirement, be enrolled and making good progress toward a degree at UNC-CH, and be in compliance with federal financial aid standards. Covenant scholars can receive funding for up to a maximum of nine semesters (i.e., one semester beyond four years, full-time).[11]

The first and most salient component of the Carolina Covenant is the financial award. The program covers 100 percent of unmet need, that is, the difference between the financial-aid recognized costs of attending UNC-CH for an academic year and the amount the student's family is expected to contribute, through a mix of grant and work-study awards.[12] In addition to this

[9] Additional information about the Carolina Covenant can be found here: http://carolinacovenant.unc.edu/faqs/.
[10] In complementary analyses using data on 8th grade students in North Carolina public schools, we explored whether the Carolina Covenant appeared to affect recruitment and enrollment patterns. We found no consistent evidence that the existence of the program meaningfully altered the likelihood of enrollment in UNC-CH for disadvantaged students. Results are available from authors upon request.
[11] For students entering UNC-CH in fall 2007 or later, university policy dictates that students must appeal to enroll for a 9th semester. If a student is granted that appeal, and remains Covenant eligible, Covenant funding continues for the 9th semester. UNC-CH's policy is described here: http://advising.unc.edu/policies-and-procedures/additional-semester-appeal/.
[12] The Covenant does not replace the amount (if any) that a family is expected to contribute based on the FAFSA. While the Covenant award makes it possible for students to graduate without borrowing, some scholars elect to borrow. This can be for a number of reasons: a) some students wish to substitute a loan for the work-study component of their award; b) some borrow for study abroad or summer programs; or c) some students may cover any remaining "expected family contribution" with loans. In almost all cases, Covenant scholars who borrow originate federal (Stafford and Perkins) loans (Shirley Ort, Associate Provost, UNC-CH Office of Scholarships and Student Aid, Personal Communication, April 13, 2014).

financial support, Covenant scholars are offered a number of non-financial services, including mentoring by a faculty or staff member, peer mentoring by older Covenant scholars, academic workshops on topics such as time management, note taking, and subject-specific study techniques, career and personal development opportunities such as career workshops, financial literacy, an "etiquette dinner," and social events (Fiske, 2010; UNC-CH Office of Scholarships and Student Aid, Carolina Covenant, "Academic and Personal Support").

Several of these non-financial components were later additions to the suite of services received by Covenant scholars. Peer mentoring did not begin until the 2006-2007 academic year, and targeted summer support for struggling scholars began with the incoming cohort of 2007.[13] Academic and social programming for Covenant scholars has deepened and improved during the last few years. In addition, the fall 2007 cohort was the first exposed to the university-wide policy limiting students to eight full-time semesters in which to complete degree requirements. For this reason, and also because, as with any large program, there were logistical and programmatic kinks during the first few years, we view scholars entering in the fall of 2007 as the earliest cohort to experience the "full-fledged" Carolina Covenant.

IV. Data and Methods

We use student-level administrative data from the UNC system-wide Office of Institutional Research and Assessment. These de-identified data include demographic descriptors of students, such as gender and race and ethnicity, basic information about their pre-college preparation, such as high school GPA, SAT/ACT scores, detailed information on any and all financial aid awards, and the criteria on which need-based awards rest, namely family income

[13] Starting in the summer of 2008, Covenant scholars close to losing academic eligibility and in need of summer courses to remain eligible were provided with loans to take those courses. At the end of the summer, the loans turned into grants if students successfully completed the course(s). This agreement was specified in a contract (Fiske, 2010, pp. 40-41).

and household size. Finally, these data files contain our outcomes of interest: credit accumulation, GPA, and graduation.

We identify entering freshman cohorts of students, labeled by the fall of the academic year in which they entered UNC-CH, from 2003 through 2010.[14] We then restrict our analytic sample in a number of ways. First, we remove incoming transfer students from these cohorts, allowing us to focus on initial freshman enrollees.[15] Second, we drop independent students – since only dependent students are eligible to receive the Covenant. Third, among dependent students, we further restrict our sample to those who report parental income information, which is necessary to be considered for any aid award. Fourth and finally, since students must enroll full-time to be eligible for the Covenant, we use information on the number of attempted credits to restrict our sample to students attempting a full-time course load in their first semester of college (i.e., 12 credits or greater).

A. Identifying Covenant-Eligible Students

Although we can observe detailed information on the sources of financial aid students receive, the files we received from UNC do not identify which specific students were Covenant recipients.[16] Lacking that information, we were forced to predict eligibility for the aid program by combining eligibility rules with detailed, student-level data that capture the key components that determine eligibility: family size, parental income, and each year's federal poverty line.

[14] We attempted to acquire data for additional years prior to 2003, but unfortunately the UNC system did not begin to regularly and reliably retain information on income and family size from student applications for financial aid until the entering class of 2003.

[15] This restriction implies that we also drop students who enroll for the first time in UNC-CH in the summer or in the spring of an academic year. We do not observe any spring enrollees (among first-time students) during our sample's timeframe. Our results are similar if we re-include the 593 students we observe first entering UNC-CH in a summer term.

[16] Students who actually receive the Carolina Covenant are not identified anywhere in our data. Our request to merge on indicators flagging anonymized recipients was denied by the Provost's committee that handles internal data requests.

Hence our first approach was to flag eligible students solely based on the family income cutoff, which is a function of the federal poverty line in each year.

We were also able to use a more refined approach that incorporated information about loans and specific categories of aid funding. We learned that the few Covenant scholars who take out loans (for personal expenses) exclusively use federal Stafford or Perkins loans. In addition, we confirmed with the Office of Scholarships and Student Aid the set of specific aid categories in which Covenant need-based institutional aid dollars are categorized. We used these two pieces of information to refine our measure of Covenant eligibility. Specifically, we re-coded as ineligible any students who would have otherwise appeared eligible but who had loans other than Perkins or Stafford in their first-year aid packages. We also re-coded as ineligible seemingly eligible students who did not receive aid from at least one of the five specific aid categories from which Covenant program dollars are funded.[17]

In Table 1 we compare the number of Covenant-eligible students that we predict using each of these two approaches to actual totals reported by the UNC-CH Office of Scholarships and Student Aid. We generally underestimate the number of true Covenant scholars by between 7 and 20 percent. In-state students make up more than 80 percent of all recipients and over half of Covenant scholars are first-generation college students.

B. Choosing an Empirical Approach

The structure of the Carolina Covenant and the data available to us make possible two estimation approaches: difference-in-differences and regression discontinuity (RD). Each approach rests on different identifying assumptions that, in turn, rely on different comparison

[17] These aid-type restrictions are one way to proxy for non-income assets. We learned that the imposition of the wealth criterion generally reduces the number of Covenant-eligible students in a given cohort by about 10 percent. In Table 1, the reductions we see in the number of Covenant-eligible students as a consequence of applying the aid-type restrictions are equivalent to between 7 and 11 percent of the originally eligible population.

groups. In this section, we explore relevant differences between the two approaches, and settle on difference-in-differences as the preferred strategy in this case.

A difference-in-differences setup enables us to measure the effects of the aid program as the change in performance of eligible students over time relative to that of students not eligible for the program. The baseline for measuring these changes is the performance of students entering UNC-CH in the fall of 2003, the final year before the program was introduced. Thus, changes in the performance of ineligible students over time allow us to net out the effects of confounding factors on our outcomes of interest.

By contrast, if we were to employ an RD setup, we could exploit the income-based cutoff used to identify Covenant scholars. In this approach, the comparison group is students whose family income placed them just above the Covenant threshold, in each year of the program's existence. The underlying assumption is that students who are just above and below the cutoff are virtually identical in terms of both measurable characteristics such as academic preparation and financial need as well as unobservable characteristics, such as motivation.

Although the RD approach has the appealing characteristic of comparing students in treatment and control groups within the same cohort, it has the disadvantage of measuring treatment effects within a relatively narrow range of family income near the cutoff for eligibility. In many contexts, treatment effects measured in this way would be policy relevant and the RD approach would be quite suitable for estimating causal effects.[18] In our case, however, a number of factors reduce its appeal. First, because the magnitude of the grant aid under the Carolina Covenant program declines with the student's family income, we would expect the financial

[18] For example, in Hoekstra's (2009) analysis of the earnings effects of attending a state flagship public institution, the treatment-control contrast at the cutoff is a large jump in the probability of admission to the institution. Admission and subsequent enrollment in the flagship university constitute a complex, multi-pronged treatment, relative to the counterfactual of rejection.

component of the program to be the largest and to have its greatest effect on postsecondary outcomes for students well below the income eligibility cutoff. Second, over the past decade, UNC-CH has improved its institutional aid packages for all needy students, regardless of Covenant eligibility. This means that the student who falls just above the cutoff for the Covenant may receive an aid package that does not differ substantially from that of her counterpart who just barely qualifies for the program. Thus, we may not expect much effect on later outcomes of the "treatment" at the cutoff margin.[19] Finally, the difference-in-differences setup better accommodates our inability to identify actual Covenant recipients. The identifying variation in the RD context depends much more heavily on differentiating students on each side of the cutoff within a relatively narrow window of data.

In Table 2, we present analyses that allow us to characterize differences in the financial aspects of the treatment-control contrasts in the two approaches. We focus on several financial measures: whether a student had any loans in her aid package during the first year at UNC-CH, the amount of loans (in constant 2010 dollars), whether a student had any institutional, need-based aid in the first year of college, and the amount of institutional need-based aid (in 2010 dollars). The entries in the table are based on difference-in-difference models (in the top panel) and RD models (in the bottom panel). For comparability across approaches, the indicator for Covenant eligibility is based solely on income in both models.

The difference-in-difference results come from models of the following basic shape:

$$Y_{it} = \alpha + \beta_1 CC_{it} + \beta_2[CC_{it} * After_{it}] + \theta X_{it} + \delta_t + \varepsilon_{it} \quad (1)$$

[19] An alternative way to think about this argument is that the marginal benefit of being Covenant eligible increases the farther below the cutoff we move. If the marginal benefit is linear in the "running" variable of an RD setup, one could imagine a scenario in which passing through the cutoff yields little to no discontinuity in that benefit (given the global expansion of institution-based grant aid at UNC-CH for needy students).

Here, Y_{it} is the financial measure of interest (e.g., the inclusion of loans in the student's aid package during the first year of college) for student *i* in year *t;* CC_{it} indicates whether student *i* in year *t* was eligible to receive the Carolina Covenant; $After_{it}$ takes on the value of one for all students entering UNC-CH in the fall of 2004 and later, that is, after the Covenant program had begun. The coefficient, β_2 , on the interaction of $After_{it}$ and CC_{it} is the coefficient of interest and represents our estimate of how eligibility for the Carolina Covenant affects the particular financial outcome of interest. The vector X_{it} refers to student-level characteristics including gender, race and ethnicity, high school GPA, SAT/ACT score, real family income and is square; δ_t represents year effects, which set up the difference-in-differences framework and capture secular changes over time in the outcomes of interest, and ε_{it} is a stochastic error term. The estimation sample for the difference-in-differences results includes students in the cohorts of 2003 through 2010.

The RD results come from parametric models of the following basic form:

$$Y_{it} = \alpha + \beta_1 CC_{it} + f(PL\_pct_{it}) + \theta X_{it} + \delta_t + \varepsilon_{it} \qquad (2)$$

Here again, CC_{it} is an indicator equal to one if student *i* in year *t* was eligible to receive the Carolina Covenant, and $f(PL\_pct_{it})$ represents a flexible function of the distance from the cutoff (centered at zero) based on a student's family income expressed as a percentage of the poverty line in year *t*.[20] We control linearly for the running variable, but interact it with the treatment indicator to allow the relationship between family income and the outcome to differ on either side of the cutoff. [21] As in the difference-in-differences setup, X_{it} refers to student-level characteristics likely associated with the outcomes of interest: gender, race and ethnicity, high

[20] The poverty line is household-size specific.

[21] Introducing higher order polynomials of the running variable, in this case, simply adds noise to the estimates with little explanatory gain in that it fits a relationship between the running variable and the outcome more poorly than the linear control.

school GPA, and SAT/ACT score. Finally, δ_t is a vector of year indicators[22] and ε_{it} is a stochastic error term. Within this parametric setup, β_1 represents the intent-to-treat (ITT) effect of being eligible to receive the Carolina Covenant on the financial measure of interest. The estimation sample for the RD results includes students in the cohorts of 2004 through 2010.

The purpose of Table 2 is to illustrate average financial differences between the treated and untreated groups that characterize each of the two analytic methods. We present results for three sets of cohorts: the full set (2004-2010) and two subgroups (2004-2006 and 2007-2010). In subsequent analyses, we use the latter two subgroups to explore differences in estimated effects of the program on college success in the early period and the later period once the program was fully implemented. Given their relevance for the subsequent analysis in section V, we include the subgroups here in our analysis of differentials in financial treatment.

In Panel A of Table 2, we report estimated differences in financial measures based on the differences-in-difference setup. Given the resulting patterns, we see that eligible students were between 28 and 33 percentage points less likely to have loans as a part of their initial aid packages than their ineligible counterparts. Interestingly, while Covenant-eligible students in the early years of the Covenant program (i.e., 2004-2006) were about 16 percentage points more likely to have some institutional, need-based grant aid than their ineligible counterparts, the difference between eligible and ineligible students in later cohorts (i.e., 2007-2010) was only 9 percentage points. This decline is consistent with anecdotal evidence on the expansion of institutional, need-based aid to a wider range of financially needy students at UNC-CH in the late 2000s. In terms of amounts, on average Covenant-eligible students receive about $1,400 less in loans and $2,400 more in need-based, institutional grant aid than their ineligible counterparts.

[22] Year fixed effects are important in this setup since the cutoff for Carolina Covenant eligibility (in terms of where a family's income fell relative to the poverty line) was different in 2004 than in later years.

In the RD approach, for each financial measure we present results from two different windows of data around the income cut point, one slightly larger than the other.[23] Based on the RD estimates, we can characterize the financial treatment of the Covenant as follows: Eligible students were between 14 and 18 percentage points less likely to have a loan as part of their initial aid package (which is about half the magnitude of the corresponding difference-in-differences estimate), but equally likely to have some kind of institutional, need-based aid component. In terms of dollars, Covenant-eligible students received an average of $800 less in loans compared to their just-ineligible counterparts, and around $1,000 more in institutional, need-based grant aid. Thus, the financial treatment under the RD setup essentially replaced around $800 of loans with grants and added an additional $200 of institutional grant aid in a student's first year of college.

The financial treatment under the differences-in-difference setup is more substantial in magnitude relative to the treatment versus control contrast leveraged by the RD setup. Specifically, the financial treatment under the difference-in-differences approach is over double the size of the treatment within the RD framework: $2,400 of need-based grant aid in the first year of college versus about $1,000.

In sum, Table 2 makes clear that the treatment versus control contrasts that are implicit in the two alternative estimation strategies are substantively different. Since we are interested in how of the Covenant program affects all eligible students, including and especially those college-goers most likely to benefit from the program, we adopt a difference-in-differences approach to explore how the program affects measures of postsecondary success. An additional advantage of the difference-in-differences approach is that it easily accommodates our refined measure of

[23] Non-parametric estimates using local linear regressions of degree zero, a bandwidth of 50, and a triangular kernel resemble these parametric estimates that control linearly for the running variable within the specified windows of data.

Covenant eligibility. Accordingly, we use this refined eligibility indicator in the difference-in-differences models that produce results in the subsequent sections.[24]

V. Findings

We present descriptive statistics for the full sample and sub-samples of Covenant-eligible and Covenant-ineligible students in Table 3. The full sample is 62 percent female, 67 percent white, relatively high achieving (with an average SAT score of about 1290), and consists of many well-off families (with average real parental income of nearly $108,000). Relative to the whole sample, Covenant-eligible students are more likely to be minority and are slightly lower-achieving (with an average SAT score of about 1240), and they come from families with markedly lower parental income. The typical Covenant-eligible student comes from a family with parental income of a bit more than $26,000 (measured in constant 2010 dollars), which is far below the average parental income of the typical student not eligible for the Covenant – of nearly $122,000.

We have already documented the nature and magnitude of financial benefits received by eligible students relative to the other students. Recall that the Carolina Covenant also features a range of non-financial supports, including peer mentoring, summer course contracts, and substantially expanded academic and social programming, many of which were not fully implemented until 2007.[25] In the two following sections, we examine the extent to which the full

[24] Results based on the eligibility indicator that is solely a function of family income are very similar and are available from the authors upon request.

[25] Conversations with the Associate Provost and Director of the Office of Scholarship and Financial Aid confirm this distinction (Shirley Ort, Personal Communication, April 30, 2015). Examples of supports that were expanded for later cohorts include 1) paying for a student's orientation expenses (and that of one parent); 2) providing welcome receptions and introductions to faculty/staff/peer mentors in the fall; 3) holiday and end-of-year social receptions; 4) business networking workshops; 5) providing for "business clothing needs" through UNC-CH's "Styled for Success" program; 6) providing multiple opportunities to engage with other campus organizations; 7) encouraging self-formed Covenant Scholar organizations (like the Covenant Gives Back or the Covenant pre-Health Society); 8) doing triage for campus wellness and academic tutoring/support programs; 9) providing regular opportunities to dine with fellow scholars and administrators; 10) providing learning disabilities services; 11) providing funding for

program affected measures of college success, with attention to the differences between the students in the earlier cohorts and those in the more recent cohorts.

A. Effects on Persistence and College Completion

In Table 4 we present estimates from our preferred difference-in-differences model on the effect of Covenant eligibility on persistence through college. For each of the three persistence outcomes, we present cohort-specific effects in the first column, a pooled estimate in the second, and estimates for two groups of cohorts that experienced different incarnations of the Covenant program in the third column. Though we fail to detect any effect of Covenant eligibility on persistence through the first three years of college, we find suggestive evidence of a boost in persistence to the fourth year of college for the later cohorts of 2007 to 2010. Indeed, the estimate for these later cohorts is statistically different from the near-zero estimate for the early cohorts of 2004 to 2006 (column 9).

In Table 5 we turn to effects of Covenant eligibility on graduating from college in four and six years. We can measure four-year graduation rates for all of our cohorts. Cohorts through 2008 contribute to estimates of the effects of the Covenant on six-year completion rates. Thus, in columns 4 and 7 we present the effects of Covenant eligibility on four-year and six-year completion, respectively, for the subset of later cohorts for which can observe both four-year and six-year graduation (i.e., 2007 and 2008).

In column 1 we see positive coefficients for all cohorts – though the size and significance of these effects tend to rise as we move from earlier to more recent cohorts, with a notable uptick in 2007. In column 3, we illustrate differences in effects on graduation of being Covenant eligible for early cohorts of scholars compared to later cohorts. We find that the typical

---

medical insurance; 12) providing a rich array of low- or no-cost cultural and performing arts opportunities (Carolina Performing Arts, and Playmakers).

Covenant-eligible student (in cohorts 2007-2010) was 7.8 percentage points more likely to graduate in four years, compared to her ineligible counterpart. Column 1 illustrates that this result is mostly driven by large, positive, and statistically significant effects for cohorts 2008 and 2009. In addition, a test of the two coefficients in column 3 reveals that the effect for the 2007-2010 cohorts is statistically different from the noisy null effect for the earlier cohorts of 2004-2006 (p-value = 0.002). For these earlier cohorts, we see a positive, small, and statistically insignificant effect on four-year graduation.

With respect to effects of the Covenant on six-year graduation rates, we again see a coefficient of near zero for the earlier cohorts of 2004 to 2006 and a positive coefficient of near 6 percentage points for the later cohorts of 2007 and 2008. Though this estimate for the later cohorts does not rise to conventional levels of statistical significance, it is statistically different from the near-zero estimate for the early cohorts and is in the same ballpark as the corresponding estimate when four-year completion is the outcome (i.e., 0.067 from column 4).

Taken together, we interpret the results in Table 5 as illustrating a clear completion benefit of the Covenant for later cohorts. The 8 percentage point bump in the four-year graduation rate represents a 10 percent increase in the already high mean four-year completion rate (of 83 percent).[26] The fact that we find the largest and most significant effects for these later cohorts (2007-2010) when the non-financial supports were more numerous and well developed suggests that both monetary and non-monetary supports matter, and perhaps interact in mutually reinforcing ways.

[26] In additional results not presented, we see little heterogeneity in this main graduation effect (for later cohorts) across student subgroups defined by gender or race and ethnicity. Results are available from authors upon request.

B. Effects on Credit Accumulation and Academic Performance

We examine two measures of performance during college: credit accumulation (Table 6) and cumulative GPA (Table 7).[27] In Table 6 we see that Covenant-eligible students tend to earn fewer credits than their non-Covenant counterparts during the first two years of college, but more during their third year of college. Thus, by year four of college, Covenant-eligible students have accumulated similar numbers of earned credits as their non-Covenant counterparts. Estimates of the effect of Covenant eligibility on cumulative earned credits by year four are particularly large for the two later cohorts that drive our graduation findings: 2008 and 2009. Indeed, by year four of college, Covenant-eligible students in the 2009 cohort had earned about 4.4 credits more (i.e., roughly one class) than their ineligible counterparts in same cohort. In Panel B of Table 6, we see no differences in the number of non-cumulative credits earned across three summer terms[28] by Covenant-eligible versus ineligible students.

In terms of academic performance as measured by cumulative grade point averages (GPA), Table 7 suggests that Covenant-eligible students perform as well as (if not a bit better than) their ineligible counterparts throughout college. Once again, though, estimated coefficients are consistently larger for the later cohorts of 2007 to 2010. By the third year of college, we estimate the cumulative GPA of Covenant-eligible students in these later cohorts to be about 0.10 GPA-points higher than Covenant-ineligible students. Although effects on GPA for these later cohorts only rise to traditional levels of statistical significance in year three of college, the

[27] For both sets of cumulative outcomes (credits earned and GPA), we assign students who drop out or otherwise disappear from our data the last cumulative value on record for the variable of interest. Earned credits are reported at the beginning of the fall semester of each academic year and reflect credits earned to date upon entering that year of college. We use cumulative GPA measures reported at the start of the spring semester of each year, thereby enabling us to have a GPA outcome for the first year of college (which reflects GPA for the first, fall semester).

[28] The "first summer" corresponds to the summer between the first and second year of college.

differences between effects for the early cohorts of 2004 to 2006 and the later cohorts of 2007 to 2010 are statistically significant across all four years of college.

Combining our findings related to credits earned and GPA suggests Covenant-eligible students in the later cohorts of 2007-2010 are earning more credits than their ineligible counterparts and performing better in those courses during the second half of college. The fact that Covenant-eligible students earn fewer credits during the early years of college but still perform at least as well as their ineligible counterparts is consistent with the slate of well-developed non-financial supports at work such as advising, especially during the post-2007 period. In sum, we find suggestive effects of Covenant-eligibility on three candidate mediators that could underlie the increase in four-year graduation rates: persistence to the fourth year of college, accumulation of earned credits in years three and four of college, and improved academic performance during those later years.[29]

C. Results from an Alternative Estimation Approach: Regression Discontinuity

Although we prefer the difference-in-differences estimation approach for determining the effectiveness of the Covenant, we present results in Table 8 based on the regression-discontinuity approach (described earlier in section IV. B.). We focus on results from the narrowest data window around the cutoff (i.e., -150 to 150 in columns 3, 6, and 9). For the pooled sample of all cohorts (2004-2010), as well as for both earlier (2004-2006) and later (2007-2010) cohorts, we see little evidence of any effect of Covenant eligibility on four-year graduation rates.[30] Recall that under the RD setup, the financial treatment being tested at the cutoff margin is modest (i.e.,

[29] Covenant-eligible students could perform better in the later years of college if they differentially switch to majors with relatively high average GPAs. We test this explanation in Appendix Table A2. In this table, we estimate the effects of Covenant-eligibility on the likelihood a student's first major is in a STEM field and the likelihood her final declared major on record is in a STEM field. If Covenant-eligible students were switching away from STEM between the beginning and end of college, we would expect to see bigger negative coefficients when the outcome is the final major (relative to when the outcome is the first major). We detect no such progression and conclude that the improvements in academic performance attributable to Covenant eligibility do not stem from students moving away from majors with relatively low average GPAs.

a substitution of around $800 in loans for grants per year). Of course, treated students also have access to a variety of non-financial supports – with the availability, number, and breadth of such supports being greater for the later cohorts (2007-2010).

These RD results suggest that non-financial supports in the absence of substantial financial aid do little to affect postsecondary outcomes like graduation. The difference-in-differences results suggest that substantial additional need-based grant aid (i.e., substituting $1,400 in loans for grants per year plus an additional $1,000 in grant aid per year) may do little to affect long-run postsecondary outcomes in the absence of strong, well-developed non-financial supports. Taken together, these sets of results imply that it is the interaction of appreciable additional need-based aid and non-financial (academic and social) supports that can improve graduation rates and academic performance of low-income, high-ability students at highly selective public institutions.

D. Robustness of Main Findings

In this section we probe the robustness of our main findings to a few lingering threats to internal validity. Though our preferred difference-in-differences approach adjusts for many potentially confounding factors related to Covenant receipt and postsecondary success, our results could be biased by differential changes over time in the low-income student body if those changes were not captured by our rich set of covariates and are related to both Covenant eligibility and our outcomes of interest. We address this concern by estimating a triple-differences specification. To do so, we add cohorts of UNC-system enrollees from 2003 through 2010 at a few other branch campuses to our analytic data set: UNC-Charlotte, UNC-Greensboro,

[30] In Appendix A, we display the results of our RD analysis for the later cohorts of 2007 to 2010 in Figure A1 (for the four-year graduation outcome). Ocular, parametric, and nonparametric analyses reveal no detectable effect of Covenant eligibility on the likelihood of graduating from UNC-CH in four years. In Figure A2, we present results from the McCrary (2008) test for bunching at the cutoff in the density of the running variable. We find no such bunching and fail to reject the null hypothesis of no discontinuity in the density of our running variable.

and UNC-Asheville are all four-year universities in the UNC system that did not adopt similar aid programs to the Covenant during our timeframe of study.

We present results from this specification in Table 9, wherein we focus on our main completion outcome: four-year graduation. The triple-differences setup allows us to compare changes in an outcome of interest such as four-year graduation rates over time for Covenant-eligible students at UNC-CH relative to Covenant-ineligible students at UNC-CH net of changes for Covenant-eligible (i.e., low-income) students at other UNC-system campuses. Changes at these other branch campuses allow us to net out the influence of other factors that may have differentially affected the success of low-income college-going students across the state over this time period.

Our estimates in columns 1 to 3 of Table 9 can be compared to the coefficients in the first three columns of Table 5, which arise from the basic difference-in-differences setup. We see similar patterns of findings across the two sets of results – though estimates from the triple-differences setup are a bit more suggestive of possible effects on four-year graduation for some earlier cohorts. Still, the coefficient for the later cohorts of 2007 to 2010 is nearly twice the size of the coefficient for the earlier cohorts of 2004 to 2006.

In columns 4 through 6 of Table 9, we estimate the same triple-differences specification on a sample that trims the upper end of the sample by dropping students from families with income above 600 percent of the federal poverty line (which is analogous to having an income slightly below the 75th percentile of the real income distribution for students at UNC-CH). The point of this exercise is to ensure that our comparisons are not being swayed by students at the upper end of the income distribution, who are likely to differ in a variety of ways. We see little change to our key estimates. If anything, this restriction sharpens the contrast between effects for

the early cohorts of 2004 to 2006 and the later cohorts of 2007 to 2010. In this trimmed sample, the difference between these coefficients in column 6 (i.e., 0.031 and 0.076) is statistically significant.

Next, we test for differential changes over time in the level of pre-college preparation of incoming cohorts by using SAT scores in math and reading as outcomes in a basic version of our preferred difference-in-differences approach. (See Appendix Table A1.) We fail to detect any "effect" of Covenant-eligibility on SAT math or reading scores. These findings assuage concerns that measures of pre-college preparation changed differentially for Covenant-eligible students in ways that might bias our findings.

Finally, bias in our results could arise if the starting point for students in 2003 is not representative of pre-Covenant trends in our outcomes of interest. We have no reason to suspect that 2003 was an odd year in any institutional sense. As we noted earlier, however, the fact that the UNC system does not have full data for the earlier years means that we cannot test this assumption.

VI. Discussion

Given our conclusion that the Covenant program raises college completion and performance, a natural next question is whether the benefits of the program exceed its costs. We conduct a back-of-the-envelope benefit-cost calculation using a framework similar to Pallais (2009) and Page et al. (2016). The goal is to explore whether the increases in completion justify the costs of the Covenant program.

We focus on the later cohorts of 2007 to 2010 for which we find effects on college completion. The average number of first-year Covenant-eligible students across those cohorts is 394 students. Our findings imply that the Covenant program would encourage about 32 more

students (or 8 percent) to complete college within four to six years. Relying on our estimates of the implicit financial treatment from Table 2, we can calculate the cost of supplying additional, institutional need-based aid to Covenant-eligible students. In this case, the total cost for a typical cohort of 394 students would be about $976,700 (in 2010 dollars). Next, we obtained an estimate of the costs associated with supplying the non-financial component of the Covenant program from the Office of Scholarships and Student Aid at UNC-CH: $276,000 per year (in 2010 dollars). This figure includes salaries for faculty mentors and office administrative support as well as the costs of all special programming activities. Thus, in total, delivering the fully developed Covenant program costs about $3,200 per student and $39,000 per student induced to complete college.

We now turn to estimating the benefits associated with this higher level of educational attainment. We use recent estimates of median earnings and tax payments for full-time employees by education level to estimate the personal and social benefits of more education. The difference between median earnings and tax payments for those with a bachelor's degree and those with only "some college" and no degree was $16,100 in 2011 dollars (Baum, Ma, & Payea, 2013, p. 11). This earnings premium is purely observational and based on median wages. Since Covenant-eligible students induced to complete college will have graduated from a top, public flagship university, we scale up this completion premium to $19,320 based on Hoekstra's (2009) finding that attending a public flagship boosts earnings by about 20 percent. If we assume this differential remains constant, the benefits of the Covenant program would outstrip its costs after only two years of post-college employment. Since Hoekstra's (2009) findings apply to flagship enrollees, even if we assume that students who attend UNC-CH and do not complete a bachelor's degree benefit in a way that is proportional to those who graduate from the public flagship in

terms of future earnings, sticking with the $16,100 differential and a constant post-graduation earnings stream increases the break-even point only to three years of post-college earnings.

Acknowledging the simplifying assumptions we have made, we conclude there would be a positive rate of return for the Covenant program over a fairly short time horizon. Thus, though programs that combine need-based financial aid with an array of non-financial supports are usually more costly than initiatives that employ only one type of support, the investment in such mixed aid programs appears to be justified.

VII. Conclusions

We use rich administrative data on college enrollees at a highly selective public institution to study the effects of a need-based, multifaceted aid program on postsecondary achievement and completion. Because we were unable to identify the actual students who received the Carolina Covenant, our indicator for Covenant eligibility is measured with error. Given that such measurement error biases our estimated effects toward zero, our findings should be interpreted as lower-bound estimates of the effects of Covenant receipt on measures of postsecondary success.

We find larger effects of the Carolina Covenant on the post-matriculation behavior of later cohorts of students than on the early cohorts. Specifically, we find that the program raises four-year graduation rates by about 8 percentage points (i.e., a 10 percent increase) for the cohorts of 2007 to 2010. Figure 1 plots the income gradient in four-year college completion at UNC-CH. Our estimated effect represents a substantial shift along this gradient. The typical Covenant-eligible student comes from a family with parental income of about $26,000 (in 2010 dollars) and the average four-year graduation rate for such students is roughly 77 percent (follow the solid red line in Figure 1). An 8 percentage point increase would bring this rate up to a level

that is reflective of the typical four-year graduation rate for students from families with incomes near $125,000 (the dashed red line in Figure 1). Although the association between parental income and likelihood of timely college completion reflects a myriad of interrelated factors, benchmarking our estimated effect against this observed gradient highlights its practical significance.

Consistent with the increase in graduation rates, we also find suggestive evidence that Covenant-eligible students in the later cohorts of 2007 to 2010 are more likely to persist to the fourth year of college, earn fewer credits during the early years of college but more during the latter, and perform better than their Covenant-ineligible counterparts especially during years three and four of college.

The major difference between the early and late cohorts is that the later cohorts were exposed to a stronger and more comprehensive set of academic and social supports, including peer mentoring, learning disability services, help with paying for health insurance, and the provision of a rich array of low- or no-cost on-campus cultural experiences. Thus, our findings are consistent with the notion that expansions of need-based aid for low-income, high-ability students stand the best chance of affecting postsecondary outcomes such as graduation when coupled with strong non-financial supports. This interpretation is consistent with recent portrayals of barriers encountered by low-income, first-generation students at strong postsecondary institutions (e.g., Pappano, 2015). These anecdotes make the case that financial aid can get such students in the door, but once enrolled these students confront barriers beyond the financial. Such barriers are depicted as social, cultural, and complicated: “The bright children of janitors and nail salon workers, bus drivers and fast-food cooks may not have grown up with the edifying vacations, museum excursions, daily doses of NPR and prep schools that groom Ivy

applicants…" (Pappano, 2015). It is exactly these types of barriers the Carolina Covenant was designed to address. Our work suggests such supports are very important, but apparently only when layered on top of strong need-based financial aid that obviates the need for students to take out loans.

References

Angrist, J., Lang, D., & Oreopoulos, P. (2009). Incentives and services for college achievement: Evidence from a Randomized Trial. *American Economic Journal: Applied Economics, 1(1),* 136-163.

Bailey, M., & Dynarski, S. (2011). Inequality in postsecondary attainment. In Greg Duncan and Richard Murnane (Eds.), *Whither Opportunity: Rising Inequality, Schools, and Children's Life Chances*, pp. 117-132. New York, NY: Russell Sage Foundation.

Baum, S., Ma, J., & Payea, K. (2013). Education pays: The benefits of higher education for individuals and society. The College Board. Access: https://trends.collegeboard.org/sites/default/files/education-pays-2013-full-report.pdf

Bettinger, E. P., Long, B. T., Oreopoulos, P., & Sanbonmatsu, L. (2012). The role of application assistance and information in college decisions: Results from the H&R Block FAFSA experiment. *Quarterly Journal of Economics, 127(3),* 1205-1242.

Bettinger, E. P. (2004). How financial aid affects persistence. In C. M. Hoxby (Ed.), *College choices: The economics of where to go, when to go, and how to pay for it* (pp. 207-238). Cambridge, MA: National Bureau of Economic Research.

Bowen, W. G., Chingos, M. M., & McPerson, M. S. (2009). *Crossing the finish line: Completing college at America's public universities.* Princeton, NJ: Princeton University Press.

Bruce, D. J., & Carruthers, C. K. (2014). Jackpot? The impact of lottery scholarships on enrollment in Tennessee. *Journal of Urban Economics, 81,* 30-44.

Castleman, B. L., & Page, L. C. (2015). Freshman year financial aid nudges: An experiment to increase FAFSA renewal and college persistence. *Journal of Human Resources,* 51(2), forthcoming.

Castleman, B. L., & Long, B. T. (2013). Looking beyond enrollment: The causal effect of need-based grants on college access, persistence, and graduation. NBER Working Paper, No. 19306. Access: http://www.nber.org/papers/w19306

Castleman, B. L., & Page, L. C., & Schooley, K. (2014). The forgotten summer: Does the offer of college counseling after high school mitigate summer melt among college-intending, low-income high school graduates? *Journal of Policy Analysis and Management, 33(2),* 320-344.

Deming, D. & Dynarski, S. (2010). Into college, out of poverty? Policies to increase postsecondary attainment of the poor. In Phillip B. Levine & David J. Zimmerman (Eds.), *Targeting Investments in Children: Fighting Poverty When Resources are Limited*, pp. 382-402.

Dynarski, S., & Scott-Clayton, J. (2013). Financial aid policy: Lesson from research. *Future of Children, 23(1),* 67-91.

Dynarski, S. (2008). Building the stock of college-educated labor. *Journal of Human Resources, 43(3),* 576-610.

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

Fiske, E. B. (2010). "The Carolina Covenant." In R. D. Kahlenberg, ed., *Rewarding Strikvers: Helping Low-Income Students Succeed in College.* The Century Foundation Press.

Fitzpatrick, M., & Jones, D. (2012). Higher education, merit-based scholarships and post-baccalaureate migration. NBER Working Paper Series, No. 18530. Access: http://www.nber.org/papers/w18530

Heller, D. E. (1997). Student price response in higher education: An update to Leslie and Brinkman. *The Journal of Higher Education, 68(6),* 624-659.

Hemelt, S. W., & Marcotte, D. E. (2011). The impact of tuition increases on enrollment at public colleges and universities. *Educational Evaluation and Policy Analysis,* 33(4), 435-457.

Hoekstra, M. (2009). The effect of attending the flagship state university on earnings: a discontinuity-based approach. *Review of Economics and Statistics, 91(4),* 717-724.

Hoxby, C. & Turner, S. (2014). Expanding opportunities for high-achieving, low-income students. Stanford Institute for Economic Policy Research (SIEPR) Working Paper, No. 12-014. Access: http://siepr.stanford.edu/publicationsprofile/2555

Hoxby, C., & Avery, C. (2013). The missing "one-offs": The hidden supply of high-achieving, low-income students. Brookings Papers on Economic Activity, Spring 2013. Access: http://www.brookings.edu/~/media/projects/bpea/spring-2013/2013a_hoxby.pdf

Kane, T. J. (1995). Rising public college tuition and college entry: How well do public subsidies promote access to college? *NBER Working Paper Series,* No. 5164.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics, 142(2),* 398-714.

NASSGAP. (2014). 44th Annual Survey Report on State-Sponsored Financial Aid. National Association of State Student Grant Aid Programs. Access: https://www.nassgap.org/viewrepository.aspx?categoryID=3#

Pallais, A. (2015). Small differences that matter: Mistakes in applying to college. *Journal of Labor Economics, 33(2),* 493-520.

Page, L. C., Castleman, B. L., & Sahadewo, G. A. (2016). More than dollars for scholars: The impact of the Dell Scholars Program on college access, persistence and degree attainment. Working paper, access: http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2726320

Page, L., C., & Scott-Clayton, J. (2015). Improving college access in the United States: Barriers and policy responses. *NBER Working Paper Series*, No. 21781. Access: http://www.nber.org/papers/w21781

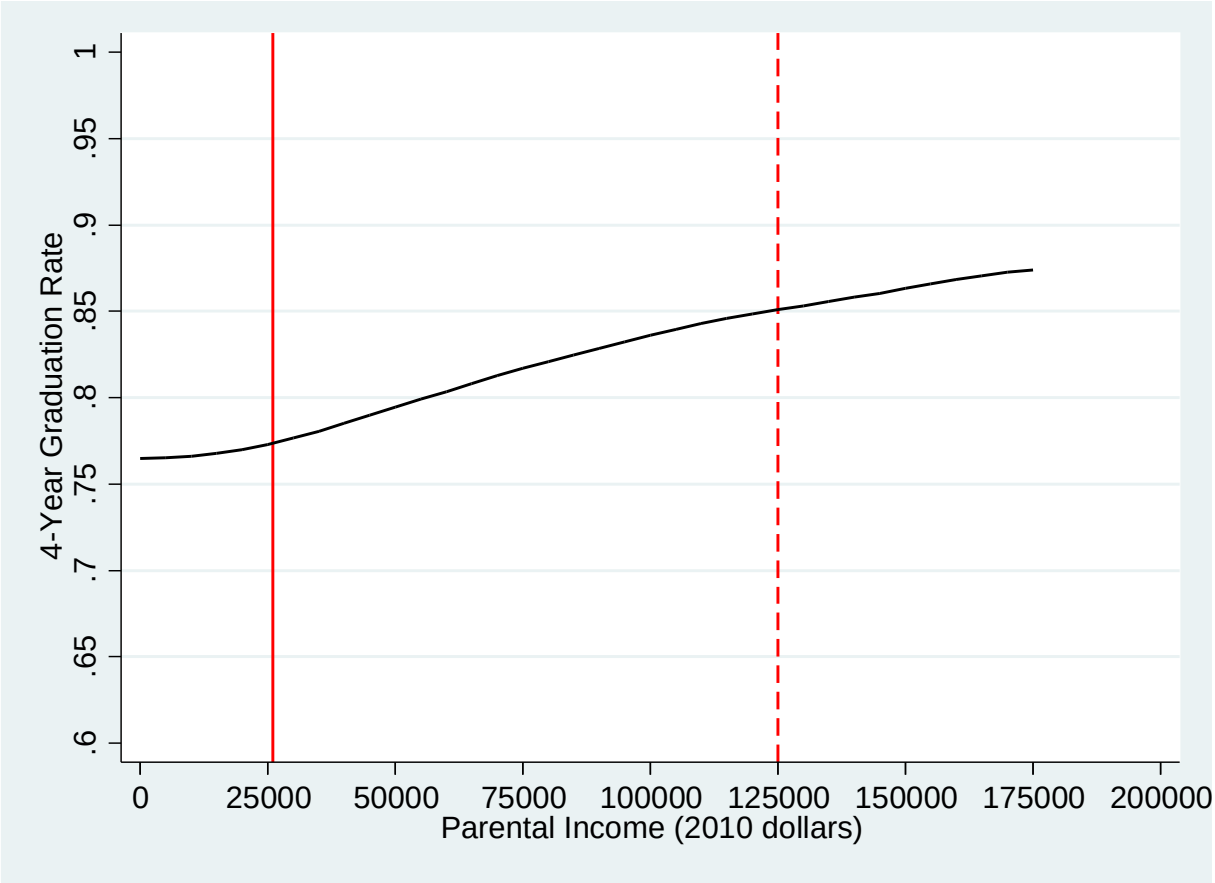
Pappano, L. (2015, April 8). First-generation college students unite. *The New York Times,* Education Life, p. ED18.

Scott-Clayton, J. (2010). On money and motivation: A quasi-experimental analysis of financial incentives for college achievement. *Journal of Human Resources, 46(3),* 614-646.

Scrivener, S., et. al. (2015). Doubling graduation rates: Three-year effects of CUNY's Accelerated Study in Associate Programs (ASAP) for developmental education students. MDRC Research Report, Access: http://www.mdrc.org/publication/doubling-graduation-rates

Sjoquist, D. L., & Winters, J. V. (2012). Building the stock of college-educated labor revisited. *Journal of Human Resources, 49,* 572-610.

Figure 1. Income Gradient in College Completion at UNC-CH



Notes: Sample includes cohorts of first-time, full-time freshman at UNC-CH from 2003 through 2010. A "lowess" line (i.e., a running-mean smooth) is fit to means calculated by $5,000 bins of family income. Observations above the 95th percentile of the distribution of real parental income (i.e., $180,000) are not included in the graph.

Table 1. Number of UNC-CH Students Eligible for Carolina Covenant: Estimated and Actual

	Estimated				*Actual*				
	Income Cutoff for Eligibility		Income Cutoff + Aid-Category Restrictions for Eligibility		All	First-Year Students	Transfers	Share First Generation	Share NC Residents
Year (Fall)	<150% Federal Poverty Level	< 200% Federal Poverty Level	<150% Federal Poverty Level	< 200% Federal Poverty Level					
2003	153		122		N/A	N/A	N/A	N/A	N/A
2004	183		163		224	224	0	0.54	0.88
2005		299		270	351	325	26	0.52	0.88
2006		305		271	413	340	73	0.57	0.84
2007		327		296	397	337	60	0.59	0.87
2008		354		322	409	349	60	0.60	0.85
2009		474		439	530	440	90	0.56	0.86
2010		510		474	558	450	108	0.55	0.80

Notes: Analytic sample captures first-time, fall enrollees in UNC-CH. The first cohort of Covenant scholars entered UNC-CH in the fall of 2004. Federal poverty level cutoffs take into account family size and are based on parents' prior year adjusted gross earnings. See text for details about eligibility.

Table 2. Characteristics of Financial Dimension of Treatment: Difference-in-Differences versus Regression Discontinuity

A. Difference-in-Differences Approach

	Any Loans in First Year		Amount of Loans in First Year (Dollars)		Any Institutional, Need-Based Aid in First Year		Amount of Institutional, Need-Based Aid in First Year (Dollars)	
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CC eligible	-0.018	-0.018	-849***	-846***	-0.147***	-0.147***	-1741***	-1742***
	(0.043)	(0.043)	(251.762)	(251.726)	(0.040)	(0.040)	(289.836)	(289.876)
CC eligible*2004-2010	-0.315***		-1378***		0.115***		2338***	
	(0.044)		(252.576)		(0.040)		(315.038)	
CC eligible*2004-2006		-0.281***		-1025***		0.162***		2066***
		(0.045)		(261.343)		(0.042)		(355.382)
CC eligible*2007-2010		-0.333***		-1562***		0.091**		2479***
		(0.044)		(258.265)		(0.041)		(337.594)
Outcome mean	0.402		$2,753		0.45		$2,641	
N	17659		17659		17659		17659	
R-squared	0.077		0.063		0.352		0.234	

Notes: All models include year effects and the following student covariates: gender, race and ethnicity, high school GPA, and SAT/ACT score. We control for students with missing covariate information with indicator variables. All models also control for household size, parental income, and the square of parental income. All financial measures are expressed in real, 2010 dollars (using the CPI-U). Loans include federal Perkins and Stafford, state, institutional, and other loans. Robust standard errors appear in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

B. RD Approach

	Any Loans in First Year		Amount of Loans in First Year (Dollars)		Any Institutional, Need-Based Aid in First Year		Amount of Institutional, Need-Based Aid in First Year (Dollars)	
	Sample: 2004-2010							
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CC eligible	-0.198***	-0.164***	-868***	-778***	-0.036**	-0.016	828**	1051***
	(0.021)	(0.024)	(116.927)	(128.822)	(0.018)	(0.020)	(322.258)	(363.069)
Outcome mean	0.38	0.36	$1,848	$1,640	0.78	0.80	$4,760	$5,005
N	6892	5547	6892	5547	6892	5547	6892	5547
R-squared	0.067	0.072	0.080	0.080	0.052	0.034	0.034	0.027
Data window	-200 to 200	-150 to 150	-200 to 200	-150 to 150	-200 to 200	-150 to 150	-200 to 200	-150 to 150
	Sample: 2004-2006							
Independent variable	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
CC eligible	-0.180***	-0.149***	-574***	-603***	-0.040	0.008	770	1202**
	(0.034)	(0.038)	(179.951)	(191.907)	(0.030)	(0.033)	(469.300)	(527.373)
Outcome mean	0.39	0.37	$1,840	$1,604	0.76	0.8	$4,119	$4,455
N	2687	2144	2687	2144	2687	2144	2687	2144
R-squared	0.060	0.066	0.075	0.069	0.046	0.019	0.020	0.009
Data window	-200 to 200	-150 to 150	-200 to 200	-150 to 150	-200 to 200	-150 to 150	-200 to 200	-150 to 150
	Sample: 2007-2010							
Independent variable	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
CC eligible	-0.210***	-0.176***	-1033***	-873***	-0.037*	-0.035	909**	960**
	(0.027)	(0.031)	(153.947)	(173.362)	(0.022)	(0.025)	(429.971)	(486.490)
Outcome mean	0.37	0.36	$1,853	$1,662	0.79	0.81	$5,170	$5,352
N	4205	3403	4205	3403	4205	3403	4205	3403
R-squared	0.071	0.075	0.083	0.086	0.056	0.045	0.030	0.029
Data window	-200 to 200	-150 to 150	-200 to 200	-150 to 150	-200 to 200	-150 to 150	-200 to 200	-150 to 150

Notes: Data windows are expressed in terms of running variable. The running variable is equal to family income minus the cutoff, both expressed as percentages of the federal poverty line. All models include the running variable, an interaction between the running variable and the treatment indicator, and year effects. All financial measures are expressed in real, 2010 dollars (using the CPI-U). Loans include federal Perkins and Stafford, state, institutional, and other loans. Robust standard errors appear in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Descriptive Statistics: Cohorts 2003-2010

	All Students (N = 17,659)		*Covenant-Eligible Students (N = 2,605)*		*Covenant-Ineligible Students (N = 15,054)*	
	Mean	SD	Mean	SD	Mean	SD
Demographics						
Female	0.62	0.49	0.64	0.48	0.62	0.49
Black	0.14	0.35	0.28	0.45	0.12	0.32
Hispanic	0.06	0.23	0.09	0.29	0.05	0.22
Asian	0.08	0.27	0.13	0.34	0.07	0.26
White	0.67	0.47	0.44	0.50	0.71	0.45
Other race/ethnicity	0.02	0.14	0.04	0.18	0.02	0.13
High School Achievement						
Final high school GPA	4.30	0.75	4.25	0.75	4.31	0.75
Missing high school GPA	0.02	0.15	0.02	0.14	0.02	0.15
SAT Math score	652	71	628	77	656	69
SAT Critical Reading score	640	76	610	80	645	74
Missing SAT score	0.02	0.13	0.02	0.15	0.02	0.13
ACT Composite	27.95	3.70	26.63	3.91	28.18	3.61
Missing ACT score	0.73	0.44	0.72	0.45	0.73	0.44
Family Background						
Income (in 2010 dollars)	$107,831	$82,021	$26,163	$12,411	$121,964	$80,691
Income (as % of poverty line)	503	367	122	50	569	358
Household size	4.05	1.11	4.02	1.40	4.05	1.05

Notes: SD = Standard deviation; Family income measure is expressed in real (2010) dollars using the Consumer Price Index for All Urban Consumers (CPI-U).

Table 4. Effects of Covenant Eligibility on Postsecondary Persistence

	Persist to 2nd Year of College			Persist to 3rd Year of College			Persist to 4th Year of College		
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CC eligible	0.001	0.001	0.001	-0.008	-0.008	-0.008	-0.023	-0.022	-0.023
	(0.019)	(0.019)	(0.019)	(0.027)	(0.027)	(0.027)	(0.032)	(0.032)	(0.032)
CC eligible*2004	-0.005			0.002			0.007		
	(0.023)			(0.034)			(0.041)		
CC eligible*2005	-0.013			-0.012			-0.020		
	(0.022)			(0.032)			(0.038)		
CC eligible*2006	0.011			0.018			0.000		
	(0.021)			(0.031)			(0.038)		
CC eligible*2007	-0.016			-0.014			0.010		
	(0.022)			(0.032)			(0.038)		
CC eligible*2008	-0.001			0.020			0.046		
	(0.022)			(0.030)			(0.036)		
CC eligible*2009	-0.001			0.012			0.029		
	(0.021)			(0.029)			(0.035)		
CC eligible*2010	-0.015			-0.002			0.010		
	(0.021)			(0.030)			(0.035)		
CC eligible*2004-2010		-0.006			0.004			0.013	
		(0.019)			(0.027)			(0.033)	
CC eligible*2004-2006			-0.002			0.003			-0.006
			(0.020)			(0.029)			(0.035)
CC eligible*2007-2010			-0.008			0.004			0.023
			(0.019)			(0.028)			(0.033)
H_0: (CC eligible*2004-2006) = (CC eligible*2007-2010):			p-value = 0.46			p-value = 0.92			p-value = 0.06
Outcome mean		0.97			0.94			0.92	
N	17659	17659	17659	17659	17659	17659	17659	17659	17659
R-squared	0.006	0.006	0.006	0.008	0.008	0.008	0.012	0.011	0.011

Notes: All models include year effects and the following student covariates: gender, race and ethnicity, high school GPA, and SAT/ACT score. We control for students with missing covariate information with indicator variables. All models also control for household size, parental income (measured in constant 2010 dollars), and the square of parental income. Robust standard errors appear in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Effects of Covenant Eligibility on College Completion

	Graduate College in 4 Years				Graduate College in 6 Years		
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CC eligible	-0.067	-0.067	-0.067	-0.061	-0.035	-0.035	-0.035
	(0.043)	(0.043)	(0.043)	(0.044)	(0.040)	(0.040)	(0.040)
CC eligible*2004	0.002				-0.005		
	(0.057)				(0.052)		
CC eligible*2005	0.020				-0.003		
	(0.051)				(0.046)		
CC eligible*2006	0.015				0.025		
	(0.051)				(0.046)		
CC eligible*2007	0.047				0.052		
	(0.050)				(0.045)		
CC eligible*2008	0.087*				0.064		
	(0.049)				(0.044)		
CC eligible*2009	0.102**						
	(0.047)						
CC eligible*2010	0.070						
	(0.048)						
CC eligible*2004-2010		0.056				0.030	
		(0.044)				(0.041)	
CC eligible*2004-2006			0.014	0.013			0.007
			(0.047)	(0.047)			(0.042)
CC eligible*2007-2010			0.078*				
			(0.044)				
CC eligible*2007-2008				0.067			0.058
				(0.047)			(0.042)
H_0: (CC eligible*2004-2006) = (CC eligible*2007-2010):			p-value = 0.002				
H_0: (CC eligible*2004-2006) = (CC eligible*2007-2008):				p-value = 0.03			p-value = 0.02
Outcome mean		0.83		0.82		0.88	
N	17659	17659	17659	13108	13108	13108	13108
R-squared	0.032	0.031	0.031	0.032	0.030	0.029	0.030

Notes: All models include year effects and the following student covariates: gender, race and ethnicity, high school GPA, and SAT/ACT score. We control for students with missing covariate information with indicator variables. All models also control for household size, parental income (measured in constant 2010 dollars), and the square of parental income. Robust standard errors appear in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Effects of Covenant Eligibility on Credit Accumulation

A. Cumulative Credits Earned

	Cumulative Credits Earned: Entering Year 2 of College			Cumulative Credits Earned: Entering Year 3 of College			Cumulative Credits Earned: Entering Year 4 of College		
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CC eligible	-0.322	-0.328	-0.325	3.262**	3.263**	3.258**	-2.054	-2.055	-2.061
	(0.663)	(0.663)	(0.663)	(1.459)	(1.458)	(1.458)	(2.303)	(2.302)	(2.302)
CC eligible*2004	3.931***			-4.552**			-0.259		
	(1.043)			(1.842)			(2.997)		
CC eligible*2005	-0.777			-5.450***			0.094		
	(0.786)			(1.701)			(2.715)		
CC eligible*2006	-0.442			-4.376***			1.939		
	(0.741)			(1.647)			(2.633)		
CC eligible*2007	-0.862			-5.833***			-0.642		
	(0.750)			(1.690)			(2.711)		
CC eligible*2008	-0.284			-3.229**			3.847		
	(0.746)			(1.618)			(2.574)		
CC eligible*2009	0.191			-2.702*			4.363*		
	(0.711)			(1.589)			(2.568)		
CC eligible*2010	-0.519			-3.816**			0.628		
	(0.718)			(1.692)			(2.575)		
CC eligible*2004-2010		-0.068			-4.125***			1.653	
		(0.672)			(1.482)			(2.341)	
CC eligible*2004-2006			0.462			-4.826***			0.717
			(0.719)			(1.541)			(2.449)
CC eligible*2007-2010			-0.335			-3.779**			2.119
			(0.675)			(1.501)			(2.369)
Outcome mean (continually enrolled students)		27.92			57.49			88.08	
N	17446	17446	17446	17286	17286	17286	17058	17058	17058
R-squared	0.290	0.287	0.287	0.114	0.113	0.113	0.020	0.019	0.020

B. Credits Earned During Summer

	Credits Earned: 1st Summer			Credits Earned: 2nd Summer			Credits Earned: 3rd Summer		
Independent variable	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
CC eligible	1.356*	1.354*	1.354*	0.267	0.267	0.267	0.496	0.494	0.496
	(0.756)	(0.755)	(0.756)	(0.354)	(0.354)	(0.354)	(0.368)	(0.368)	(0.368)
CC eligible*2004	-0.642			0.454			0.347		
	(0.814)			(0.475)			(0.491)		
CC eligible*2005	-1.073			0.329			-0.043		
	(0.792)			(0.442)			(0.444)		
CC eligible*2006	-1.182			0.234			0.540		
	(0.794)			(0.442)			(0.462)		
CC eligible*2007	-1.471*			0.275			-0.167		
	(0.785)			(0.437)			(0.448)		
CC eligible*2008	-0.812			0.419			-0.064		
	(0.788)			(0.427)			(0.433)		
CC eligible*2009	-0.608			0.646			0.053		
	(0.778)			(0.405)			(0.416)		
CC eligible*2010	-1.051			0.117			-0.030		
	(0.778)			(0.397)			(0.413)		
CC eligible*2004-2010		-0.976			0.350			0.064	
		(0.758)			(0.361)			(0.375)	
CC eligible*2004-2006			-1.012			0.323			0.275
			(0.768)			(0.386)			(0.400)
CC eligible*2007-2010			-0.958			0.364			-0.041
			(0.760)			(0.367)			(0.380)
Outcome mean		4.05			3.7			3.69	
N	17196	17196	17196	16883	16883	16883	16731	16731	16731
R-squared	0.128	0.127	0.127	0.058	0.058	0.058	0.064	0.064	0.064

Notes: All models include year effects and the following student covariates: gender, race and ethnicity, high school GPA, and SAT/ACT score. We control for students with missing covariate information with indicator variables. All models also control for household size, parental income (measured in constant 2010 dollars), and the square of parental income. Robust standard errors appear in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Effects of Covenant Eligibility on Academic Performance

	Cumulative GPA: Year 1 of College			Cumulative GPA: Year 2 of College			Cumulative GPA: Year 3 of College			Cumulative GPA: Year 4 of College		
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
CC eligible	-0.036	-0.036	-0.036	-0.036	-0.035	-0.036	-0.079	-0.078	-0.079	-0.058	-0.058	-0.058
	(0.060)	(0.060)	(0.060)	(0.049)	(0.049)	(0.049)	(0.054)	(0.054)	(0.054)	(0.054)	(0.054)	(0.054)
CC eligible*2004	-0.008			0.020			0.068			-0.009		
	(0.079)			(0.067)			(0.071)			(0.073)		
CC eligible*2005	-0.066			-0.066			-0.008			-0.019		
	(0.072)			(0.063)			(0.067)			(0.067)		
CC eligible*2006	0.017			0.006			0.044			-0.014		
	(0.069)			(0.058)			(0.062)			(0.064)		
CC eligible*2007	0.005			0.051			0.086			0.083		
	(0.071)			(0.059)			(0.063)			(0.064)		
CC eligible*2008	0.085			0.060			0.110*			0.090		
	(0.068)			(0.058)			(0.062)			(0.063)		
CC eligible*2009	0.026			0.036			0.090			0.072		
	(0.066)			(0.056)			(0.061)			(0.061)		
CC eligible*2010	0.063			0.042			0.097			0.059		
	(0.065)			(0.056)			(0.060)			(0.061)		
CC eligible*2004-2010		0.024			0.025			0.074			0.045	
		(0.061)			(0.050)			(0.055)			(0.055)	
CC eligible*2004-2006			-0.020			-0.018			0.029			-0.015
			(0.064)			(0.053)			(0.058)			(0.058)
CC eligible*2007-2010			0.046			0.046			0.096*			0.074
			(0.061)			(0.051)			(0.055)			(0.056)
H_0: (CC eligible*2004-2006) = (CC eligible*2007-2010):			p-value = 0.02			p-value = 0.02			p-value = 0.02			p-value = 0.002
Outcome mean		3.09			3.09			3.1			3.12	
N	17428	17428	17428	17088	17088	17088	17093	17093	17093	16428	16428	16428
R-squared	0.214	0.213	0.213	0.204	0.203	0.204	0.194	0.194	0.194	0.182	0.181	0.182

Notes: All models include year effects and the following student covariates: gender, race and ethnicity, high school GPA, and SAT/ACT score. We control for students with missing covariate information with indicator variables. All models also control for household size, parental income (measured in constant 2010 dollars), and the square of parental income. Robust standard errors appear in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Effects of Covenant Eligibility on College Completion: RD Results

	Sample: 2004-2010		
	Graduate College in 4 Years		
Independent variable	(1)	(2)	(3)
CC eligible	-0.030**	-0.003	0.008
	(0.015)	(0.019)	(0.021)
Outcome mean	0.83	0.8	0.79
N	15495	6892	5547
R-squared	0.026	0.028	0.026
Data window	All data	-200 to 200	-150 to 150

	Sample: 2004-2006		
	Graduate College in 4 Years		
Independent variable	(4)	(5)	(6)
CC eligible	-0.042	-0.006	0.005
	(0.028)	(0.033)	(0.037)
Outcome mean	0.82	0.77	0.76
N	6990	2687	2144
R-squared	0.029	0.025	0.023
Data window	All data	-200 to 200	-150 to 150

	Sample: 2007-2010		
	Graduate College in 4 Years		
Independent variable	(7)	(8)	(9)
CC eligible	-0.020	0.002	0.009
	(0.018)	(0.023)	(0.026)
Outcome mean	0.84	0.82	0.81
N	8505	4205	3403
R-squared	0.026	0.027	0.026
Data window	All data	-200 to 200	-150 to 150

Notes: Data windows are expressed in terms of running variable. The running variable is equal to family income minus the cutoff, both expressed as percentages of the federal poverty line. All models include the running variable, an interaction between the running variable and the treatment indicator, and the following student covariates: gender, race and ethnicity, high school GPA, and SAT/ACT score. We control for students with missing covariate information with indicator variables. All models also include year effects. Robust standard errors appear in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 9. Effects of Covenant Eligibility on College Completion: Robustness Checks

	Outcome = Graduate College in 4 Years					
	Comparison Institutions: UNC-C, UNC-G, UNC-A			Drop students from sample with parental income above 600 percent of the poverty line		
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
CC eligible	0.001	0.001	0.001	0.013	0.013	0.013
	(0.026)	(0.026)	(0.026)	(0.027)	(0.027)	(0.027)
CC eligible*UNC-CH	-0.051	-0.051	-0.051	-0.039	-0.039	-0.039
	(0.050)	(0.050)	(0.050)	(0.051)	(0.051)	(0.051)
CC eligible*UNC-CH*2004	0.008			-0.003		
	(0.067)			(0.067)		
CC eligible*UNC-CH*2005	0.063			0.037		
	(0.060)			(0.061)		
CC eligible*UNC-CH*2006	0.052			0.049		
	(0.061)			(0.062)		
CC eligible*UNC-CH*2007	0.053			0.041		
	(0.060)			(0.061)		
CC eligible*UNC-CH*2008	0.118**			0.123**		
	(0.058)			(0.059)		
CC eligible*UNC-CH*2009	0.113**			0.096*		
	(0.056)			(0.057)		
CC eligible*UNC-CH*2010	0.054			0.046		
	(0.056)			(0.057)		
CC eligible*UNC-CH*2004-2010		0.072			0.061	
		(0.051)			(0.052)	
CC eligible*UNC-CH*2004-2006			0.046			0.031
			(0.054)			(0.055)
CC eligible*UNC-CH*2007-2010			0.084			0.076
			(0.052)			(0.053)
H_0: (CC eligible*2004-2006) = (CC eligible*2007-2010):			p-value = 0.12			p-value = 0.09
Outcome mean		0.54			0.51	
N	46758	46758	46758	38185	38185	38185
R-squared	0.239	0.239	0.239	0.219	0.219	0.219

Notes: All models include year effects and the following student covariates: gender, race and ethnicity, high school GPA, and SAT/ACT score. We control for students with missing covariate information with indicator variables. All models also control for household size, parental income (measured in constant 2010 dollars), and the square of parental income. Robust standard errors appear in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A1. Effects of Covenant Eligibility on Pre-College Achievement

	SAT Math Score			SAT Verbal Score		
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
CC eligible	-9.610	-9.600	-9.615	-14.526*	-14.498*	-14.526*
	(6.689)	(6.688)	(6.688)	(7.558)	(7.557)	(7.557)
CC eligible*2004	2.137			-5.298		
	(9.114)			(10.363)		
CC eligible*2005	-2.110			0.412		
	(8.112)			(8.813)		
CC eligible*2006	-4.714			-5.185		
	(8.406)			(9.102)		
CC eligible*2007	-1.449			0.072		
	(8.026)			(8.769)		
CC eligible*2008	-2.303			-1.071		
	(8.080)			(8.821)		
CC eligible*2009	2.039			2.882		
	(7.643)			(8.574)		
CC eligible*2010	6.140			11.617		
	(7.672)			(8.529)		
CC eligible*2004-2010		0.349			1.632	
		(6.828)			(7.685)	
CC eligible*2004-2006			-2.097			-3.074
			(7.280)			(8.112)
CC eligible*2007-2010			1.605			4.048
			(6.930)			(7.786)
Outcome mean		651.93			639.97	
N	17362	17362	17362	17362	17362	17362
R-squared	0.056	0.056	0.056	0.051	0.051	0.051

Notes: All models include year effects and control for household size, parental income (measured in constant 2010 dollars), and the square of parental income. Robust standard errors appear in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

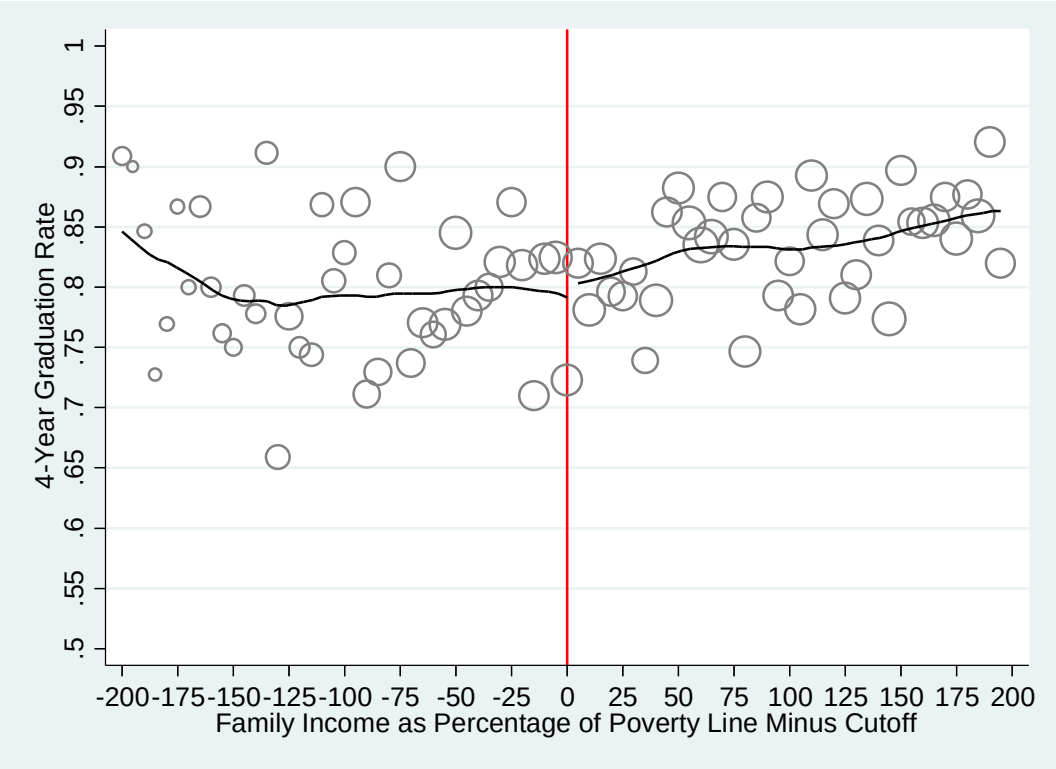
Appendix Table A2. Effects of Covenant Eligibility on Major Choice

	First Major in STEM			Final Major in STEM		
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
CC eligible	0.058	0.058	0.058	0.035	0.035	0.035
	(0.047)	(0.047)	(0.047)	(0.046)	(0.046)	(0.046)
CC eligible*2004	-0.037			-0.020		
	(0.061)			(0.060)		
CC eligible*2005	-0.117**			-0.087		
	(0.056)			(0.055)		
CC eligible*2006	-0.045			-0.011		
	(0.055)			(0.054)		
CC eligible*2007	-0.093*			-0.085		
	(0.055)			(0.054)		
CC eligible*2008	-0.011			-0.011		
	(0.055)			(0.054)		
CC eligible*2009	-0.056			-0.027		
	(0.053)			(0.052)		
CC eligible*2010	0.006			0.015		
	(0.053)			(0.052)		
CC eligible*2004-2010		-0.047			-0.029	
		(0.048)			(0.047)	
CC eligible*2004-2006			-0.070			-0.042
			(0.050)			(0.049)
CC eligible*2007-2010			-0.035			-0.022
			(0.048)			(0.047)
Outcome mean		0.36			0.36	
N	16588	16588	16588	16588	16588	16588
R-squared	0.053	0.053	0.053	0.052	0.051	0.051

Notes: All models include year effects and the following student covariates: gender, race and ethnicity, high school GPA, and SAT/ACT score. We control for students with missing covariate information with indicator variables. All models also control for household size, parental income (measured in constant 2010 dollars), and the square of parental income. STEM majors include the following majors (CIP codes): Agriculture, Agriculture Operations, and Related Sciences (01); Natural Resources and Conservation (03); Architecture and Related Services (04); Computer and Information Sciences and Support Services (11); Engineering (14); Biological and Biomedical Sciences (26); Mathematics and Statistics (27); Physical Sciences (40); Psychology (42); and Health Professions and Related Clinical Sciences (51).

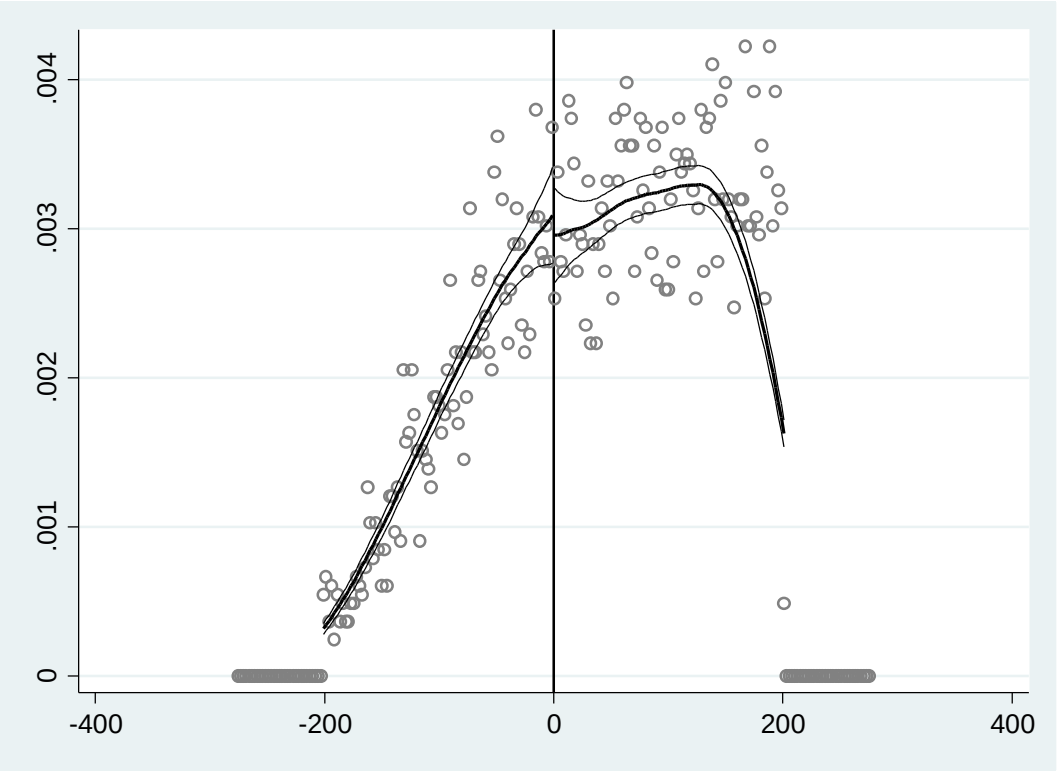
Appendix Figures

Figure A1. Effect of Covenant Eligibility on 4-Year College Graduation Rates: Cohorts 2007 to 2010



Notes: Means are plotted for 5-percentage-point bins of family income (expressed in terms of a percentage of the federal poverty line relative to the cutoff) and depicted as hollow circles; the larger the circle the greater the number of students. A weighted local polynomial regression of degree zero fits a line on each side of the cutoff using a triangular kernel and a bandwidth of 50 percentage points, where weights are equal to the number of students in each circle.

Figure A2. Density Plot of Running Variable: Distance from Income Cutoff Expressed as Percentage of Federal Poverty Line



Notes: Plot generated using "DCDensity" command written by McCrary (2008); Lighter lines represent 95% confidence interval; McCrary test's estimate of discontinuity has t-stat of 0.62 and so we fail to reject the null hypothesis of no discontinuous jump at cutoff value of running variable.

Using default bin size calculation, bin size =	2.40
Using default bandwidth calculation, bandwidth =	74.41
Discontinuity estimate (log difference in height):	-.049
	(.079)