CONDITIONAL CASH TRANSFERS AND COLLEGE PERSISTENCE: EXPERIMENTAL EFFECTS OF A RANDOMIZED NEED-BASED GRANT PROGRAM*

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We use the random assignment of a privately funded Wisconsin grant to estimate the impacts of financial aid on college persistence among Pell Grant recipients at 13 public universities over three years. This is the first statewide experimental study of a means-tested conditional cash transfer program requiring full-time college attendance and using an award process that facilitates the identification of effects on college retention and academic progress, conditional on initial college choice. Estimated experimental treatment impacts, measured as the difference in retention rates of students in the treatment and control group three years after the program began, range from positive sixteen percentage points for students who had modest academic preparation for college to negative six percentage points for well-prepared students. This effect heterogeneity yields a null average treatment effect. The effect heterogeneity is mirrored in effects on how the grant influenced students' time use; the subgroup experiencing positive educational impacts appear to have used the aid to avoid working intensively. After testing several alternative explanations for the negative effects, we present suggestive evidence that well-prepared students, because their families had somewhat better finances, were disproportionately likely to lose the means-tested treatment grant and responded negatively to the uptick in the cost of attendance resulting from that loss. We conclude that researchers need to further investigate the dynamic effects of conditional cash transfers, and explore the potential for enhancing their modest effects through better targeting.

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I. INTRODUCTION

The individual and social benefits of a college education are substantial. Even in the midst of the current recession, the unemployment rate of bachelor's degree recipients is less than half that of high school graduates who never attended college (Carnevale and Rose 2011). College graduates earn nearly $400,000 more (in present discounted value) than high school graduates over a lifetime and there is some evidence that the returns are larger for students who are the least likely to complete college degrees (Barrow and Rouse 2005; Brand and Xie 2010).

Do means-tested conditional cash transfer programs effectively promote college persistence among students from low-income families? We exploit the random assignment of a need-based Wisconsin financial aid grant to address that key policy question. The recent expansion of college enrollment has not been matched by a comparable expansion of college completion, as the proportion of the population attaining some college has grown much faster than the proportion of the population attaining bachelor's degrees (Turner 2004; Goldin and Katz 2008; Bowen, Chingos, and McPherson 2009). Low rates of college completion are especially prevalent among students from socioeconomically disadvantaged families (Belley, Frenette, and Lochner 2010, 2011). Some have suggested that these trends may be partly attributable to rapid increase in the costs of college attendance unmatched by comparable increases in student financial aid, leading to growth in the net price (Goldin and Katz 2008; Bowen, Chingos, and McPherson 2009). Research often suggests that low-income families are especially price sensitive, even after they make the initial decision to attend college (Dynarski 2003; Bowen, Chingos, and McPherson 2009; Deming and Dynarski 2010).

Efforts to discount the price of college are widespread. More than $177 billion in financial aid was distributed to undergraduates in 2010–2011, including $47 billion in federal

grants ($34 billion of which was the Pell Grant), almost $30 billion in institutional grants, $9 billion in state grants, and nearly $7 billion in private grants. The remainder consisted of loans, work-study funds, and tax credits. In comparison to the average cost of tuition and fees (currently $8,244 for in-state students at public universities) grant are fairly small: an average Pell is $3,828, and state grants are about $620 per full-time-equivalent student (College Board 2011).

Broadly speaking, the effects of conditional cash transfer programs on educational attainment are mixed (Angrist, Oreopolus, and Williams 2010; Gertler and Sepulveda, forthcoming), and clean identification strategies are uncommon in studies of financial aid (Cellini 2008; Goldrick-Rab, Harris, and Trostel 2009; Bettinger, 2011). Due to political and ethical concerns, as well as their substantial expense, randomized trials examining the impact of financial aid are very rare, and most have focused on merit- or performance-based programs in a single or small number of colleges involving fairly homogenous samples (Deming and Dynarski 2010; Harris and Goldrick-Rab 2012). This paper describes experimental impact estimates of a means-tested $3,500 per year grant intended to improve persistence rates among public university students at 13 institutions across Wisconsin. We find that over three years the treatment had several results. For the group most at risk of dropping out of college—students with weaker high school preparation—the grant induced sizable positive improvements in enrollment rates, credit attainment, and grades. We see approximately similar impacts on how those students spent their time. However, on average the estimated effects on outcomes were null. We explore several possible explanations, but suggest that part of the story lies in some unexpected negative impacts, which may be related to the grant's eligibility criteria. Since the grant was delivered to the most advantaged Pell Grant recipients (those who enrolled in college

right out of high school and attended full-time), the vast majority were students with fairly strong academic preparation whose families were on the margin of Pell eligibility. After receiving the treatment grant in their freshman year, some then lost it in their sophomore year, apparently due to small improvements in their family income that rendered them Pell-ineligible. While in theory they still faced the same cost of attendance as the control group, the unexpected decline in their aid packages appears to have been hard to accommodate, triggering a negative response. We consider the possibility that students misinterpreted the grant as essentially a change in permanent income or they may have been loss averse (Kahneman and Tversky 1979; Thaler and Sunstein 2008). We point to the need for further investigation of the dynamics of conditional cash transfer programs and the importance of considering effect heterogeneity.

The remainder of the paper is organized as follows. Section II reviews prior evidence on the effects of financial aid on college persistence. Section III describes the experimental design and data. Section IV present estimates of impacts on educational outcomes, and heterogeneous treatment effects are discussed in Section V. Section VI concludes with discussion of implications for future research, policy, and practice.

II. FINANCIAL AID AND COLLEGE PERSISTENCE

College financial aid is a conditional cash transfer whose main condition is college attendance. Since the estimates of aid's impacts are often said to be modest and the largest need-based grant, the federal Pell, is delivered based strictly on financial need, the question of whether need-based financial aid should be reformed by attaching additional conditions is currently debated in policy circles (Bettinger 2011; Chronicle of Higher Education 2011). Among the most

common and popular conditions, especially for state aid programs, is the requirement that students also have strong academic preparation for college.[1]

One challenge facing policymakers aiming to promote college completion, rather than simply college attendance, is that very few studies of financial aid manage to assess the independent impact of aid among students who have already made the decision to attend college. Using national, state, and institutional samples, most prior studies using quasi-experimental methods examine the impacts on enrollment only, or the combined impacts on college enrollment and persistence (e.g., McPherson and Shapiro 1991; Kane 1994; Light and Strayer 2000; Bound and Turner 2002; DesJardins et al. 2002; Paulsen and St. John 2002; Seftor and Turner 2002; van der Klauuw 2002; Stinebrickner and Stinebrickner 2003; Bettinger 2004; Singell 2004; Singell and Stater 2006; Kane 2007; Stater 2009). In summarizing that work, Deming and Dynarski (2010) note that eligibility for $1,000 in grant aid seems to increase college *enrollment* rates by four percentage points and "also appears to increase completed schooling" (294; italics added).

The few rigorous studies estimating impacts of aid among college-goers focus on programs with multiple conditions (e.g. academic preparation and/or college performance) and thus have little to say about the impacts of strictly need-based aid. For example, the Georgia HOPE program (distributed to high-achieving students and requiring strong performance in college) appeared to increase college persistence rates by five to eleven percentage points and degree completion rates by three or four percentage points (Dynarski 2008), but also reduced the fraction of students enrolling full-time and inducing students to take easier courses and majors

[1] For example, from 2006 to 2010, the federal government offered the Academic Competitiveness Grant to Pell recipients who had completed a "rigorous secondary school program of study" in an effort to induce more students to take challenging courses in high school (Choy et al. 2009).

(Cornwell, Kyung, and Mustard 2005). The West Virginia PROMISE program (similar to the HOPE) seemed to boost four-year bachelor's degree completion rates by 26% (from a base of 27 percentage points) (Scott-Clayton 2011). Two experimental studies with Canadian university-based aid that required strong academic performance in college yielded modest impacts (Angrist, Lang, and Oreopoulos 2009; Angrist, Oreopoulos, and Williams 2010), while three experimental studies of scholarship programs, targeted to students in poverty (primarily mothers receiving welfare) at community colleges in Louisiana, Ohio, and New York, and requiring specific credit thresholds and grades in college, produced increases in both attempted credits, and completed credits (Richburg-Hayes et al. 2009; Cha and Patel 2010; Richburg-Hayes, Sommo, and Welbeck 2011). It is not clear what lessons such studies have for those seeking to assess programs with no academic requirements.

The potential that aid has heterogeneous treatment effects on persistence is rarely considered, though it is suggested by at least two studies. In an analysis of a Ohio program, Bettinger (2010) estimated that an unexpected increase in aid for a group of socioeconomically disadvantaged students had a small positive effect on their first-year persistence rates, while reducing aid for more advantaged students did not diminish persistence. Similarly, in a national study Alon (2011) identified much larger positive benefits grants (including federal, state, and institutional) on first-year persistence for students in the bottom half of the income distribution and virtually no effects for students in the top half.

III. EXPERIMENTAL DESIGN AND DATA

We analyzed the first cohort of students served by the Fund for Wisconsin Scholars grant program, hereafter referred to as the Wisconsin Scholars Grant (WSG).[2] The program is supported by a $175 million endowment and spends approximately $4 million annually, making it one of the largest need-based grant programs in the state (Pope 2010).

III.A. The Intervention

Decisions about the WSG's rules and operation belong to the Fund's board of directors and research studies are conducted independently. Random assignment is a mechanism used by the program—while it facilitates rigorous evaluations, the program is not operating exclusively (or even primarily) for research purposes, and student participation is not predicated on research participation. This presents a distinct advantage for evaluators, who have the opportunity to study a typical program as it really works rather than examining a trial program created for research purposes. As Heckman has argued, "causal models are tools for policy analysis" and thus the most useful experiments create little to no disruption (or Hawthorne effects) (2005, 136). This program fits that description: the people participating in the WSG program are those who would participate even if a research study were not occurring.

The WSG provides Pell-eligible university students with a $3,500 grant per year for up to five years, with a total maximum award of $17,500 per student.[3] Students are first chosen for the

[2] More information on the Fund for Wisconsin Scholars is at www.ffws.org. The program was first announced in December 2007 and program details became publicly available in September 2008.

[3] A student is eligible to receive the Pell Grant if his or her expected family contribution, as determined by completion of a federal aid application and a federal need analysis methodology, is below a certain value ($4,041 in the 2008-2009 academic year). For more details see Dynarski and Scott-Clayton (2007).

award after they begin college (thus it does not affect the decision whether and where to initially attend) and the grant is transferable among all public colleges and universities in Wisconsin.[4]

In order to be eligible to receive the WSG, students must have residency in the state of Wisconsin, attend and graduate from a Wisconsin public high school, enroll full-time in a Wisconsin public university for the first time within three years of completing high school, receive a federal Pell Grant, and have an unmet need (net of all grant aid) of at least $1 (see Appendix A for more on Wisconsin public higher education, which includes 13 universities).

In the interest of fairness and in an effort to avoid a disappointment effect, eligible students are identified with administrative records. The names of eligible students are sent to a state agency overseeing the distribution of several grant and loan programs, where the process of random assignment is coordinated with input from the research team.[5]

Renewal of the grant requires that students continue to be Pell-eligible and enroll at a Wisconsin public university or two-year college where they are registered full-time (at least 12

[4] The amount increases or decreases if a student changes sectors – for example, if a student moves to a two-year public college, the grant declines to $900 per semester. Wisconsin has two types of two-year colleges, including 13 branch campuses of the UW System and 16 technical college districts offering certificates and degrees. The Fund for Wisconsin Scholars also offers a grant of $900 per semester to two-year students, and in a separate analysis we are examining the effects of that grant.

[5] In the program's first year of operation—the cohort analyzed here— secondary checks on eligibility were not performed before or immediately following random assignment. Instead, when accepting the offer of the grant, students had to affirm specific pieces of their eligibility that could not be checked with available administrative records including their resident status, graduation from a public high school, and initial entry into college. Some students did not complete the eligibility form, or reported themselves ineligible and thus nine percent of students did not "take up" the grant. All 600 students selected for "treatment" were immediately sent the award letter. Six weeks later, when the Fund for Wisconsin Scholars began to distribute the grant money, 49 of the students had still not replied to the letter (institutional financial aid officers were asked to follow up with students and reportedly did so). Of the 551 respondents, seven students indicated they were not eligible for the grant. Thus, in December 2008, 544 students (91% of the original sample) were sent the first grant payment. In Subsample 2, described later in the paper, that percentage was 92.

credits) at the start of each term and make satisfactory academic progress.[6] The grant is awarded at the start of each term. In the program's first year, students were not reminded about renewal criteria and after that the program issued a few emails containing "different messages about eligibility, transferring, good luck with classes, and other general information."[7] In order to have the grant reinstated after an absence from college, students have to notify their financial aid office and the program's executive director.[8]

The WSG is awarded after students begin college, and students meeting the eligibility criteria are told they were chosen at random (see Appendix B for the award letter). So while the program is quite simple and generous compared to many others, especially those conducted for research purposes, it is not as straightforward and well-known as long-established programs like the federal Pell Grant. Interviews we conducted with a stratified random sample of 36 students at four universities shortly after they were awarded the grant revealed that some thought the grant was a "scam" and were suspicious enough to seek more information from their financial aid officers.[9] Program awareness continued to be low; surveys we administered in the months after the program began and again a year later showed that barely half of students offered the grant knew that it was a part of their financial aid package. Moreover, interviews we conducted with financial aid officers revealed that they found the program complicated and time-intensive to administer. At the same time, they also said the complexity led them to communicate with

[6] Satisfactory academic progress, also required by the Pell Grant, typically means a C average or equivalent and "academic standing consistent with the requirements for graduation" from the institution.
[7] This is an excerpt from a personal communication from the Fund's Executive Director to the research team.
[8] The Fund's Executive Director reports that very few students did so.
[9] Specifically, we interviewed students attending four of the 13 universities, with the sample of 36 drawn at random among students who consented to an interview. Stratification of that sample was performed according to treatment status, race, and gender.

students about the program more often than they otherwise would have, and that it was common for private aid programs to be idiosyncratic and for grants to have strings attached.

III.B. Sampling

In September 2008, 3,157 new freshmen receiving Pell Grants were determined eligible for the WSG using administrative records. Six hundred grant recipients were selected using simple random assignment (no blocking by university was employed). From the remaining pool of 2,557 students, we drew a stratified random sample of 900 students to serve as the control group, not including the entire pool of non-recipients because of the costs of data collection and diminishing statistical returns to control group size with a fixed treatment group (Bloom 2005). In selecting the control group, we blocked the list of non-recipients by university in order to facilitate the collection of an oversample of non-white students. Thus, the size of the control group is 50% larger than the treatment group, and contains a larger proportion of students attending more racially and ethnically diverse institutions.[10]

The full sample includes 1,500 Pell Grant recipients, with 40% randomly assigned to receive the offer of the WSG. Table I reports descriptive statistics for the full sample at baseline, as well as for two analytic subsamples. Fifty-seven percent of the full sample is female, 24.6% are members of a racial/ethnic minority group, and 53.4% are first-generation college students.[11] In fall 2008, the average adjusted gross income of their parents was just under $30,000 and the

[10] We employ sampling weights to adjust for the unbalanced allocation of students between the treatment and control groups. The sampling weights are calculated as the (inverse) probability of selection. In the treatment group, the probability is the same for all students regardless of campus. In the control group, the calculation is analogous, except that the probability of selection in the control group varies by campus because of the number of students selected to be assigned to treatment and the selection of a larger control group (over-sampling) in more diverse campuses.

[11] Targeted racial/ethnic minority groups include African-Americans, Native Americans, Hispanics, Southeast Asians, and multiracial students who are from at least one of these groups. Information on race for the full sample was obtained from a student survey, as it is not included in the FAFSA.

average expected family contribution was $1,633. Because of the grant's eligibility criteria, their mean age was just over 18, and just 2.7% were independent for tax purposes.

While recipients and non-recipients of the offer of the WSG should be equivalent on both observable and unobservable baseline characteristics due to random assignment, we confirm that balanced allocation with a series of checks using measures drawn from administrative records and survey data. Table II reports selected student characteristics, indicating the means and for the control and treatment groups and p-values for the differences between them. Of the 51 comparisons, only two are statistically different based on two-tailed t-tests (both in Subsample 2, a point we return to later). This is fewer than the number expected by chance alone. We also report the results of a joint significance test of all the variables listed and again fail to reject that they are jointly different from zero. We therefore conclude that the control and treatment groups were equivalent at baseline.

Before students assigned to treatment received the WSG, students assigned to the treatment and control groups had equivalent financial aid packages. Both groups had total aid packages of about $10,450. Nearly 80% had a subsidized loan (Stafford or Perkins) and almost one-fourth had an unsubsidized Stafford loan. Total initial loan burden was just under $3,300 (see Appendix Table A.1, Panel 1).

Students were notified of the WSG on October 22, 2008—the second month of their first semester of college. Upon verification that they met the eligibility criteria, funds were received by the colleges and universities and distributed by the financial aid officers at the end of the term; for most students, the award appeared in their aid package in early December. Compared to the control group, students assigned to treatment had total financial aid packages that were

$1,849 larger overall, containing $2,854 more in grant aid (p<.01) and $958 less in student loans (p<.01), as well as a non-significant reduction in work-study (see Appendix Table A.1, Panel 2). Thus a clear expected and immediate impact of the treatment was a reduction in student debt—over three years this accumulated to more than $3,100 (see Appendix Table A.2, Panel 3).

Of the 92% of students who received the grant in the first year, a sizeable fraction lost the grant. Almost 24.4% lost it immediately after their first year of college, and another 20.3% lost it after their second year. While students offered the WSG had more money than the control group after the grant was put in students' aid packages, losing it meant that their aid packages declined (for example, they lost $2,109 after year 1, while the control group lost $1,373; after year 2 the treatment group lost $1,429 while the control group lost $688). Loss of the grant was partially, but not fully, offset by the restoration of aid that had been initially crowded out, and by a reduction in tuition costs if students dropped to part time (see Appendix C for information on crowding out, Appendix Table A.2 for the change in financial aid packages by year, and covariate-adjusted estimates in Appendix Table A.5). The differences between treatment and control groups in the *slopes* of total aid packages over time are statistically significant (p<.05) after year 1 and near the threshold of statistical significance after year 2 (p=.12). Yet even after losing the WSG, compared to the control group, students in the treatment group still had more financial aid.

IV. AVERAGE IMPACTS OF TREATMENT ON EDUCATIONAL OUTCOMES

Next, we describe the average treatment effects on college retention rates, credit completion, and grade point average over three years.

IV.A. Data, measurement, and estimation

We measure college retention using data from the National Student Clearinghouse (NSC), a non-profit organization founded in 1993 that serves as the nation's only source for college enrollment and degree verification. It is a centralized reporting system that collects publicly available directory information obtained from the colleges and universities attended by 92 percent of American undergraduates. All public universities in Wisconsin participate in the NSC.[12] Since it is directory information, we access NSC data for all but a small fraction of the sample, and in Appendix D we describe reasons for missing observations.

We measure attempted and completed credits and grade point average with data from the UW System, and observe that information only for students who remained enrolled in that university system (however, note that there was no impact of the treatment on transfer rates). We derive credits from grade point average data, meaning that they indicate that a student registered for and completed a credit, passing a college class with a D or above. Credits for pass/fail classes, which are not included in GPA calculations, are not recorded with this measure.[13] Credits derived from pre-college enrollment, including Advanced Placement tests, are also not included. We report grade point average for students enrolled each term, and for students who are not enrolled in a term, we use the GPA from the last term enrolled, following Scott-Clayton (2010), recognizing that estimation of causal effects on GPA is not as

[12] Only 12 colleges in Wisconsin who participate in the IPEDS did not participate in the NSC as of 2008-2009. The largest of these is Herzing University, a for-profit institution with a student enrollment of under 1,500. Total enrollment at these 12 schools (none of which are public institutions) is just over 7,000 students.
[13] The courses-attempted data show that a very small proportion of credits enrolled are not GPA-bearing.

straightforward as with other academic outcomes.[14] We measure attempted credits on the tenth day of the semester.[15]

We utilize covariates representing family and educational background to increase statistical power and as sensitivity checks to address any baseline nonequivalence. Some of our covariates are measures that are unbalanced for one of our subsamples (parental education and expected family contribution) and other covariates include those which theory suggests are associated with educational outcomes (race, gender, age, and initial college of attendance). Parental education comes from a survey sent to students in fall 2008, shortly after WSG recipients were notified. We have these survey data for 78% of the full sample, and for nearly all of Subsample 2. The other covariates are from university records and are available for every student in Subsample 2. We report covariate-adjusted estimates in Appendix Tables A.7 through A.9. In general, all of our results are very robust to these adjustments, which is unsurprising given the very small differences between control and treatment groups at baseline.

IV.B. Baseline equivalence checks

We have measures of enrollment (via NSC) for the full sample, and as described earlier, checks indicate that random assignment effectively balanced the treatment and control groups (see Table II). However, due to federal privacy laws, we obtain information on credits and grades from the UW System for just over three-quarters of the full sample. Characteristics of that sample (termed 'Subsample 1') appear in Table I. Subsample 1 is disproportionately male

[14] Students can only have grades if they are enrolled; thus if the grant influences enrollment, then this could give the false appearance that the program influenced GPA when in fact it may be that different students were enrolled and had grades observed.

[15] Very few students completed degrees during this time frame (just 3% earned associate's degrees). We calculated two alternate measures to account for degrees—one that simply measured degrees, and one that included both persistence and degrees as outcomes. The results do not differ and we use the latter in these analyses.

and includes more students who are not part of a targeted minority group. However, representation in the subsample is equivalent for the treatment and control groups (76% in the treatment group vs. 77% in the control group), and there are no statistically significant differences between the two on observable characteristics at baseline (see Table II).

IV.C. Experimental estimates of effects on education outcomes

Panel 1 of Table III reports average treatment effects on enrollment for each of three academic years for both the full sample and Subsample 1. While students randomly assigned to receive the WSG had slightly higher rates of retention during fall and spring terms in 2008-2009 and 2009-2010, compared to the control group, that positive impact (ranging from 1.3 – 2.8 percentage points) was not statistically significant, and by 2010-2011 the point estimate diminished to near zero.[16]

Moreover, on average, we find no statistically significant impact on average credits completed. Students completed 66.2 credits (Subsample 1), and impacts on credits around the threshold for receipt of the WSG (12 credits per term—or 72 credits through three years) suggest that students were less likely to complete 12 credits per term, if assigned to treatment. The results indicate a 1.8 percentage point negative impact on completion of a full-time credit load that is not statistically significant.[17] Given these small point estimates, the general lack of statistical significance is unsurprising. The experiment had sufficient power to detect impacts on dichotomous outcomes no smaller than six to eight percentage points.

[16] The same pattern holds for both Subsample 1 (see Table III, Panel 1) and Subsample 2 (see Table III, Panel 2), and remains with covariate-adjusted estimates (see Appendix Table A.6).
[17] The effect is more negative and statistically significant for Subsample 2.

V. HETEROGENEOUS TREATMENT EFFECTS

Researchers modeling the effects of financial aid often assume that students respond in similar ways to the intervention. However, we began with an explicit hypothesis based on prior theory and research that financial aid would have different effects for different students (Goldrick-Rab, Harris, and Trostel 2009). Growing heterogeneity among both students and institutions over higher education over time would seem to increase the potential that this should be taken into account when estimating effects (Smith 2008). Here, given the national effort to focus on promoting strong college preparation, we describe heterogeneous effects according to high school preparation.

V.A. Data, measurement, and estimation

Statistical power for the analyses of subgroup effects is somewhat limited due to the small sample sizes resulting from missing data. For example, the data needed to identify high school preparation is available only for 55% of the full sample (Subsample 2 in Table I). While it would have been preferable to explicitly stratify the sample for random assignment based on pre-treatment characteristics, this was not the program's design. For these reasons, we view this part of the experimental analysis as exploratory, and we conduct sensitivity checks.

Academic preparation, as measured by a student's high school coursework, is a stronger predictor of college success than even GPA or test scores (Adelman 1999, 2006). Wisconsin has comparatively low requirements for high school graduation, generating a fair bit of variation among college applicants (Achieve 2011). Being well-prepared for college (and competitive for admission to top schools) requires completing rigorous high school coursework—and it also makes students eligible for merit-based financial aid. In this case, it means that students received

the Academic Competitiveness Grant (ACG) if they were Pell recipients, enrolled full-time, and had completed a "rigorous high school curriculum" with a 3.0 high school GPA.[18] Fully 80% of the full sample met the ACG standards (as expected, since they all gained admission to universities and enrolled on-time and full-time).[19] Most of the remaining 20% missed the ACG criteria by only a few courses. Only a small fraction (less than 10% of that 20%) had very few of the required ACG courses. For reasons we elaborate on later, ACG receipt also affected their financial aid package—both initially and over time.

We next divide the sample into well-prepared students (proxied by ACG receipt) and modestly-prepared students (no ACG) and test for whether the treatment and control groups were equivalent at baseline within each subgroup.[20] As expected, well-prepared students differ from modestly-prepared students on characteristics typically associated with academic advantage. For example, they are less likely to come from a racial/ethnic minority group and more likely to have parents with higher levels of income and net worth. In other words, they are closer to the margin of Pell eligibility. As Appendix Table A.5 illustrates, we identify a few non-equivalences in the smaller group of modestly-prepared students (20% of the sample). In that group, students randomly assigned the WSG had more parental income ($31,649 vs. $20,808, $p<.001$) than students in the control condition. Since this alone could give the false appearance of more

[18] In Wisconsin, that meant that students had to either pass two AP courses (with a three or higher on the exam) or two IB courses (with a four or higher on the exam) or complete the following: four years of English, three years of math with one course higher than Algebra I, three years of science with one year of two of the three following courses (biology, chemistry, and physics), three years of social studies, and one year of foreign language, fine arts, or technical education. These requirements were in place for students who graduated from Wisconsin high schools in 2007 or 2008. They exceed the requirements for graduation from a Wisconsin public high school.

[19] For comparison purposes, only about 25% of all freshmen and sophomores in Wisconsin higher education received the ACG in 2006-2007 (Choy et al. 2009).

[20] Approximately five percent of modestly prepared students had the ACG in their initial aid package at the beginning of the fall 2008 semester (panel 1 of Appendix Table A.2). Financial aid officers took several months to verify ACG eligibility (which requires counting courses on transcripts by hand) and subsequently revised that award. Thus we use the actual disbursement of the ACG (panel 2) to define whether a student got the grant, and thus are in the "well-prepared" category.

positive treatment effects for that subgroup, we perform several sensitivity checks described on the following pages.

V.B. Estimated heterogeneous treatment impacts on education and other behaviors

As Panel 2 of Table III illustrates, we find widely divergent effects of the WSG for students with more and less rigorous high school preparation, beginning in the first term in which they were notified about the grant.[21] In fall 2008, among modestly-prepared students, we estimate that treatment caused a 12.5 percentage point increase (from 77.6 to 90.1%, $p<.05$) in the percent of students completing 12 or more credits, and a slightly smaller decrease in the percent completing less than 12 credits. There was also a positive impact on GPA for that group; receiving the WSG raised it from 2.2 to 2.7 ($p<.01$). In contrast, no such changes were observed for well-prepared students, whose rate of completing 12+ credits was much higher to begin with (92.6% of the control group completed 12+ credits).

Our estimates indicate that positive treatment effects on credits and grades continued to occur for modestly-prepared students in the next term, while negative effects on completed credits began to emerge for well-prepared students. In spring 2009, just 79% of well-prepared students with the WSG completed 12 or more credits, compared to 84.8% of the control group ($p<.10$).

Among modestly-prepared students, estimated treatment impacts on retention emerged one year after treatment began and grew fairly steadily over the next two years. If they were not offered the WSG, many students who were only modestly prepared in high school left college:

[21] Since the money had not yet arrived, we consider the possibility that the effects were again due to baseline non-equivalence. However, the covariate-adjusted estimates confirm our sense that the effects are due to treatment.

22.2% did not enroll for a second year of college, and another 27.2% did not enroll for a third year. The treatment appears to have reduced attrition; by spring 2011, nearly three years after treatment began, the positive impact estimate of being offered the WSG was 15.9 percentage points (81.7% of the treatment group was still enrolled, compared to just 65.8% of the control group, $p<.05$). If, over time, these positive effects show up as similar increases in college graduation, then the WSG would pass a cost-benefit analysis (see Appendix E for details).

In contrast, while attrition was lower for well-prepared students, assignment to receive the WSG exacerbated it by 6.2 percentage points (in spring 2011, 79.7% of the treatment group compared to 85.9% of the control group was still enrolled, $p<.05$). These differences in treatment impact estimates by high school preparation are statistically significant (see the last column of Table III, Panel 2).

We also estimate heterogeneous treatment impacts on credits and grades. The treatment seems to have caused modestly-prepared students to attempt more credits, and well-prepared students to attempt fewer. Over three years, assignment to receive the WSG increased the number of completed credits by 10.4 for modestly-prepared students (on a base of 56 for the control group), and decreased the number completed by well-prepared students by 3.4 (though that effect was not significant, the base for the control group was 73.2). Assignment to treatment caused a positive increase in GPA for the modestly-prepared students—from 2.2 to 2.6 ($p<.01$)—and no change for well-prepared students.

We replicate our results with covariate-adjusted estimates (including but not limited to all covariates with imbalances, and college fixed effects). The results we next describe do not substantively differ from the covariate-adjusted estimates (Appendix Tables A.7, A.8, and A.9);

if anything, the effects and the variation in effect heterogeneity become even larger. In addition, we consider whether the imbalanced characteristics exerted more influence on the outcomes of interest by regressing the outcomes on baseline characteristics (using the control group) and then predicting the outcomes for the treatment group. We then test for treatment-control differences in the predicted outcomes and find few differences that could be explained by differences in initial characteristics. The results (found in Appendix Table A.6) again seem to confirm that the imbalance is not the likely cause of the pattern of results observed. Given the correlation between academic preparation and race, and the fact that we oversampled for minority students, we also test for the presence of treatment heterogeneity by race, but do not find significant differences in the impact estimates (results available from the authors upon request).

The heterogeneous impacts on educational outcomes are somewhat similar to the heterogeneous impacts on student behaviors. We examine the impact of the WSG on the amount of time students spent working, rates of intensive work (20+ hours per week), and number of hours studying (see Table IV for unadjusted estimates and Appendix Table A.9 for covariate-adjusted estimates). We find differences in behaviors between the two subgroups. For example, 16.3 percent of modestly-prepared students assigned to treatment worked more than 20 hours per week during their second year of college, compared to 33.5 percent of control students ($p<.05$). The impact on work was much smaller for well-prepared students (17.9% for treatment, 23.5% for control, not significant) (for more details see Benson and Goldrick-Rab 2011). While the difference in impact on the amount of work in year 2 for the two subgroups is not statistically significant, the impact on changes in work behaviors over time is – the offer of the grant seems to have stemmed what otherwise would have been a sharp uptick in rates of intensive work only for modestly-prepared students.

V.C. Explaining heterogeneous treatment impacts

What accounts for the observed effect heterogeneity by academic preparation, and the apparent negative impacts for well-prepared students? It seems unlikely that group differences at baseline or institutional effects drive either (Appendix Table A.5). Since well-prepared students are unevenly distributed across campuses, we also included college fixed effects in our covariate adjustments, but this has no influence on the results (Appendix Tables A.7-A.9).[22] It seems most likely that effect heterogeneity—although not the negative effects—is related to the differential response of students to receiving the grant money. The modestly-prepared students appear to have used the money to increase their time spent on school, while the well-prepared students did not.

The negative impacts we estimate for well-prepared students also appear to be valid. They accrue after students lose the WSG, and imply that the effect of that loss offsets any positive effect of gaining the grant. While all students awarded the WSG were at risk of losing it, the risk of loss was higher for students on the margin of Pell eligibility—which because of the relationship between high school preparation and family wealth comprises the group of well-prepared students in the sample. Fully 91.5% of modestly-prepared students maintained Pell eligibility, compared to 83.7% of well-prepared students (p=.02—Panel 2 of Appendix Table A.4). Losing Pell eligibility causes students to lose most or all of their need-based grants, and for those in the treatment group, causes them to also lose the WSG. In other words, treatment

[22] 70% of well-prepared students are concentrated in six (of 13) universities and 79% of modestly-prepared students are concentrated in four universities. In our analytic sample, four universities have at least 10 well-prepared students and at least 10 modestly-prepared students. The results we present do not appear in tables but are available from the authors upon request.

exacerbates the amount of money lost when students become Pell ineligible—creating a steeper increase in their net cost of attendance.

This is what we observe. As Appendix Table A.2 shows, after year 1 well-prepared students initially offered the WSG experience a loss of financial aid (and thus an uptick in their college costs) of $1,166 compared to the control group ($p<.01$, Panel 1). After year 2, they lose another $856 in aid, compared to the control group ($p<.10$, Panel 2). These are changes over time, but they also mean that by year 3 a simple comparison of the total financial aid received by students assigned to treatment and control reveals that the treatment provided no additional money to well-prepared students in that year (see Appendix Table A.4).

Of course, if the loss of the WSG was due to a loss of Pell eligibility, this implies that the student's family experienced a gain in resources that in theory should have allowed them to accommodate the loss of financial aid (and corresponding increase in the price of college). If this is true, then it should have offset the loss, but the additional income may not have been truly discretionary from students' standpoints (e.g., because the income accrued to their parents).

Basic economic theory and prior research (Deming and Dynarski 2010) would posit that the hike of about $1,000 in the cost of continuing to attend college might be sufficient to cause some students to drop out. But the explanation would be stronger if psychological processes were also at work along with that rational response. We suspect this to be the case, at least for some of the students. For example, we have reason to believe that some did not anticipate the loss of financial aid, and that it may have caused them stress and anxiety. While the surveys revealed that as freshmen only 10.6% of students in the sample expected their packages to remain the same during their time in college, more than half of the students were myopic in their

thinking about money.[23] Only 37% felt very or extremely confident that they could get help from other people if faced with financial problems, and just 65% felt they handled unforeseen situations well.[24] Thus some of the students may be loss averse, and thus react more strongly to declines in their aid package (Thaler 1981; Knetsch and Sinden 1984; Shefrin and Statman 1985; Odean 1998; Vendrick, Maarten, and Woltjer 2007; Levitt, List, Neckermann, and Sadoff 2011). This would mean that students felt that having received and lost aid made them feel worse off than if they had not been offered the WSG.

Another potential explanation, more in line with standard utility maximization, is that students misinterpreted the grant to be permanent income (or really "semi-permanent" for the years they remain in college). Such a misperception could have led them to increase consumption and, specifically, to lock themselves into future consumption, e.g., by signing a long-term lease on an expensive apartment or buying a car and then having to drop out of college to work more and pay these bills.

VI. CONCLUSION

Need-based financial aid might seem like a fairly straightforward incentive, offering individuals cash in exchange for college attendance. But the patterns we observe are not entirely straightforward.

First, while prior studies of financial aid emphasize average treatment effects, our results suggest the presence of heterogeneous responses to aid, independent of institutional influences. We identify a strong and positive response to the conditional cash transfer for one group of

[23] For example, when asked which option they would prefer, 20% of freshmen selected $75 right now, 15% said $100 in 3 months, 16% said $250 in a year, and 45% said $500 in 3 years.
[24] This data does not appear in tables but is available from the authors upon request.

students: those who were only modestly-prepared for college. For these students, an offer of an additional $3,500 per year in grant aid is estimated to cause them to accrue about an additional half-semester of college, ten more college credits, and helped them achieve a higher GPA after three years. Their prospects for graduation within six years of college entry appear markedly improved. On the other hand, well-prepared students did not appear to benefit from the offer of additional grant aid. It did not increase the terms of college they completed, or their credits or grades, and they did not appear to use the money to substantially reduce their work hours, even though almost 14% were working more than 20 hours per week while trying to earn good grades.

These findings are inconsistent with some recent work on conditional cash transfers. Based on his studies of studies in primary and secondary schools, Fryer (2011) concludes that "financial incentives had little or no effect on the outcomes for which students received direct incentives, self-reported effort, or intrinsic motivation." As with studies before us (Deming and Dynarski 2010), we find positive impacts from financial incentives tied to the outcome of college enrollment (for one subgroup). Attaching incentives to inputs (e.g., study time) might have been even more effective for these students, but it seems equally likely that the efficacy of financial output-focused incentives depends on the specific output, its production function, and the nature and size of the incentive.

Second, our results suggest that removing financial aid—effectively increasing the net price of college after a student has already begun school—may have negative consequences. Human capital theory is based on the assumption that people are rational in their education decisions, but our findings suggest otherwise for the well-prepared students in this sample. Our evidence of possible loss aversion is far from definitive and there are possible alternative

explanations, but the possibility is not unreasonable given prior research of other types of decisions.

VI.A. Policy implications

Federal financial aid programs have for decades been one of the nation's single largest educational programs—and are fast becoming one of the largest line items in the federal budget for social policy. The recent exponential growth in costs associated with financial aid, and the federal Pell Grant program in particular, has led some policymakers to call for cuts in funding.

Some might interpret our results to suggest a need to better target financial aid programs. Convincing those who distribute financial aid to narrow their focus and doing so without introducing complexity to the application for financial aid are two major challenges. This is an area ripe for further investigation. It is unfortunate that, as Jeffrey Smith notes, "policies that affect individuals close to the margin on a particular choice often get analyzed with evidence that applies to everyone making a particular choice" (2008, 11), since a reliance on average treatment effects may lead to the elimination of policies or programs that could become much more effective through better targeting (Schuck and Zeckhauser 2006).

There may be some utility in considering our findings in relation to the many proposals around the country to either increase tuition or cut financial aid in response to financial crises. If the loss aversion we observe here is common among students, such moves could reduce retention rates and possibly graduation rates as well. Instead, states and institutions could avoid changing course for students who are already enrolled, implementing changes only for new students. This is similar to efforts to fix tuition for four or five year periods.

Finally, the results also indicate a need to more carefully consider the benefits and costs of front-loading financial aid. This practice is widely utilized as a mechanism to affect college choices. However, by structuring in losses it may reduce the benefits associated with grants partially or even entirely— a contention supported by at least one prior study (DesJardins, Ahlburg, and McCall 2002).

VI.B. Limitations and Future Research

Our analysis is based on a single experiment and has limitations. First, we were not able to stratify the sample ex ante to increase statistical power in tests for heterogeneous treatment effects. In addition, these estimated impacts of the WSG are based on a cohort of students who attended college in the midst of a deep recession. In one sense, the challenge of affording college was substantial, and potential for the grant to make a difference great. On the other hand, the recession could have altered which students might have been considered "marginal" attendees in ways that make it difficult to predict whether the grant's impact should be smaller or larger than under more typical economic conditions. With a weak job market, less-well-off students might have been more likely to attend college and these students might have experienced different effects of the grant, or credit-constrained students might not have been able to attend college at all.

We believe, and have argued elsewhere, that the need for more comprehensive field trials is critical to improving educational and social policy (Harris and Goldrick-Rab 2012). Paying greater attention to the potential for heterogeneous treatment effects, and working to identify the mechanisms through which interventions operate, will help researchers develop better theories of

individual behavior and help policymakers figure out the groups and conditions under where reforms can be effectively targeted and brought to scale.

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Table I. Selected Characteristics of University Students at Baseline (Fall 2008) by Sample

	Full	Subsample 1			Subsample 2		
Characteristic	Total	In sample	Not in sample	p-value	In sample	Not in sample	p-value
Assigned to treatment (%)	40.0	40.7	37.5	0.299	43.1	36.2	0.006
Gender (%)							
Male	42.7	41.5	46.9	0.111	38.5	48.2	0.001
Female	57.3	58.5	53.1	0.111	61.5	51.8	0.001
Race/ethnicity (%; student self-report)							
Targeted minority	24.6	22.7	29.9	0.024	22.7	29.9	0.024
Non-targeted group	75.4	77.3	70.1	0.024	77.3	70.1	0.024
Average age (years)	18.2	18.2	18.2	0.857	18.2	18.2	0.860
Married (%)	0.8	0.8	0.8	0.966	0.8	0.8	0.968
Has child(ren) (%)	1.0	1.0	1.0	0.984	0.9	1.1	0.762
First in family to attend college (%)	53.4	53.9	51.9	0.588	54.2	52.5	0.574
Father has an AA or higher (%)	29.3	28.9	30.7	0.583	29.8	28.6	0.686
Mother has an AA or higher (%)	36.7	36.0	39.2	0.362	35.3	38.5	0.267
Financially dependent on parents (%)	97.3	97.3	97.0	0.771	97.3	97.1	0.836
Average expected family contribution ($)	1,633	1,623	1,665	0.784	1,658	1,600	0.657
Zero expected family contribution (%)	30.6	31.2	28.5	0.384	29.4	32.1	0.285
Parent(s)' adjusted gross income ($)	29,963	29,621	31,165	0.209	30,142	29,731	0.689
Parent(s)' investment net worth ($)	5,056	4,791	5,988	0.201	5,253	4,802	0.545
First year student, no prior enrollment (%)	95.9	96.1	95.4	0.649	96.2	95.5	0.519
First year student, prior enrollment (%)	3.9	3.9	3.7	0.879	3.8	4.1	0.777
NSC identifies no prior enrollment (%)	79.2	79.1	79.5	0.880	77.9	80.8	0.205
Terms of prior enrollment, if not first time	1.8	1.7	2.1	0.058	1.7	1.9	0.344
P-value from global F-test				<0.001			<0.001
Sample Size	1500	1167	333		828	672	

SOURCES: Data come from a student's 2008 FAFSA except for race, which is self-reported on a survey, and the last two measures of first-time enrollment, which are based on National Student Clearinghouse data.

NOTES:

(1) The full sample is used for impact estimates on enrollment. Analytic subsample 1 is used for impact estimates on credits, enrollment intensity, and GPA, while analytic subsample 2 is used for impact estimates on financial aid.

(2) Only complete cases are used; no imputation is performed for missing data.

(3) Targeted minority groups include: African-Americans, Latinos, Southeast Asian, Native American, and multiracial. "Targeted" refers to a policy of the University of Wisconsin System, in which all sample participants began college.

(4) Parent income and investments (dependent students only) had few extreme values with undue influences and were therefore trimmed (Winsorized) at the 95th percentile (Tukey, 1962).

(5) The global F-test includes all above measures except race and terms of prior enrollment.

Table II. Selected Characteristics of University Students at Baseline (Fall 2008) by Assignment to Treatment

	Full			Subsample 1			Subsample 2		
Characteristic	T	C	p	T	C	p	T	C	p
Gender (%)									
Male	42.3	43.3	0.711	39.6	44.0	0.154	37.2	40.3	0.399
Female	57.7	56.7	0.711	60.4	56.0	0.154	62.8	59.7	0.399
Race/ethnicity (%; student self-report)									
Targeted minority	25.1	24.0	0.681	23.4	21.7	0.578	23.4	21.7	0.578
Non-targeted group	74.9	76.0	0.671	76.6	78.3	0.578	76.6	78.3	0.578
Average age (years)	18.2	18.2	0.942	18.2	18.2	0.885	18.2	18.2	0.644
Married (%)	1.0	0.5	0.298	1.1	0.4	0.254	1.1	0.3	0.196
Has child(ren) (%)	1.2	0.8	0.456	1.1	0.9	0.815	0.8	1.0	0.817
First in family to attend college (%)	53.4	53.5	0.958	53.0	55.0	0.534	54.5	53.7	0.852
Father has an AA or higher (%)	30.1	28.3	0.518	30.1	27.4	0.388	30.2	29.3	0.802
Mother has an AA or higher (%)	37.3	35.9	0.634	37.7	33.7	0.215	36.7	33.2	0.349
Financially dependent on parents (%)	97.5	97.0	0.572	97.7	96.9	0.440	97.8	96.8	0.414
Average expected family contribution ($)	1,656	1,603	0.669	1,638	1,604	0.809	1,684	1,621	0.697
Zero expected family contribution (%)	29.6	31.9	0.362	29.9	32.8	0.332	26.1	34.0	0.019
Parent(s)' adjusted gross income ($)	30,417	29,403	0.314	30,113	28,985	0.321	31,257	28,587	0.049
Parent(s)' investment net worth ($)	4.582	5,643	0.155	4,442	5,240	0.340	4,937	5,693	0.468
First year student, no prior enrollment (%)	96.1	95.7	0.681	96.0	96.2	0.849	95.8	96.9	0.401
First year student, prior enrollment (%)	3.5	4.3	0.468	4.0	3.8	0.849	4.2	3.1	0.401
NSC indicates no prior enrollment (%)	79.0	79.4	0.722	79.4	78.7	0.797	78.7	76.8	0.534
Terms of prior enrollment, if not first time	1.8	1.8	0.856	1.7	1.7	0.764	1.8	1.7	0.868
P-value from global F-test			0.921			0.483			0.435
Sample Size	600	900		475	692		357	471	

SOURCES: Data come from a student's 2008 FAFSA except for race, which is self-reported on a survey, and the last two measures of first-time enrollment, which are based on National Student Clearinghouse data.

Notes:

(1) Targeted minority groups include: African-Americans, Latinos, Southeast Asian, Native American, and multiracial. "Targeted" refers to a policy of the University of Wisconsin System, in which all sample participants began college.

(2) Parent income and investments (dependents only) had few extreme values with undue influences and were Winsorized at the 95th percentile (Tukey, 1962).

(3) The global F-test includes all above measures except race and terms of prior enrollment.

Table III, Panel 1. Impacts on Educational Outcomes Over Three Years.

	Full Sample					Subsample 1					
Outcome	Full	T	C	Impact	SE	Full	T	C	Impact	SE	
Fall 2008											
Enrolled (NSC)	98.1	98.3	97.8	0.5	0.7	97.9	98.1	97.7	0.4	0.9	
Completed credits	--	--	--	--	--	14.1	14.2	13.9	0.3	0.2	
1-11 credits (%)	--	--	--	--	--	10.7	10.7	10.7	0.0	1.9	
12+ credits (%)	--	--	--	--	--	88.4	88.8	87.8	1.0	1.9	
Cumulative GPA	--	--	--	--	--	2.6	2.6	2.5	0.1	0.1	
Spring 2009											
Enrolled (NSC)	94.8	95.5	93.8	1.7	1.2	94.8	95.2	94.3	0.9	1.4	
Completed credits	--	--	--	--	--	12.2	12.3	12.0	0.3	0.3	
1-11 credits (%)	--	--	--	--	--	17.7	18.1	17.1	1.0	2.3	
12+ credits (%)	--	--	--	--	--	73.8	74.1	73.4	0.7	2.7	
Cumulative GPA	--	--	--	--	--	2.5	2.5	2.5	0.0	0.1	
Fall 2009											
Enrolled (NSC)	87.1	87.7	86.4	1.3	1.8	86.5	86.9	86.0	0.9	2.1	
Completed credits	--	--	--	--	--	10.8	11.0	10.6	0.4	0.4	
1-11 credits (%)	--	--	--	--	--	13.0	12.8	13.3	-0.5	2.1	
12+ credits (%)	--	--	--	--	--	66.7	68.0	65.1	2.9	2.9	
Cumulative GPA	--	--	--	--	--	2.5	2.5	2.5	0.0	0.1	
Spring 2010											
Enrolled (NSC)	84.1	85.3	82.5	2.8	2.0	84.3	85.1	83.3	1.8	2.3	
Completed credits	--	--	--	--	--	9.8	9.8	9.9	-0.1	0.4	
1-11 credits (%)	--	--	--	--	--	14.0	16.6	10.5	6.1	2.1	***
12+ credits (%)	--	--	--	--	--	60.2	58.5	62.4	-3.9	3.1	
Cumulative GPA	--	--	--	--	--	2.5	2.5	2.5	0.0	0.1	
Fall 2010											
Enrolled (NSC)	81.2	81.3	81.0	0.3	2.1	80.6	80.8	80.3	0.5	2.5	
Completed credits	--	--	--	--	--	9.2	9.4	9.0	0.4	0.4	
1-11 credits (%)	--	--	--	--	--	14.0	13.1	15.1	-2.0	2.2	
12+ credits (%)	--	--	--	--	--	55.1	56.6	53.1	3.5	3.1	
Cumulative GPA	--	--	--	--	--	2.5	2.5	2.5	0.0	0.1	

Table III, Panel 1. Impacts on Educational Outcomes Over Three Years (Continued).

	Full Sample					Subsample 1				
Outcome	Full	T	C	Impact	SE	Full	T	C	Impact	SE
Spring 2011										
Enrolled (NSC)	78.4	78.0	79.0	-1.0	2.3	77.8	77.7	77.9	-0.2	2.6
Completed credits	--	--	--	--	--	8.7	8.6	8.8	-0.2	0.4
1-11 credits (%)	--	--	--	--	--	11.2	11.4	11.1	0.3	2.0
12+ credits (%)	--	--	--	--	--	54.6	53.7	55.8	-2.1	3.1
Cumulative GPA	--	--	--	--	--	2.5	2.5	2.5	0.0	0.1
Cumulative Outcomes (2008-2011)										
Completed credits	--	--	--	--	--	66.2	66.7	65.5	1.2	1.8
1-71 credits (%)	--	--	--	--	--	42.8	44.0	41.2	2.8	3.1
72+ credits (%)	--	--	--	--	--	56.6	55.8	57.6	-1.8	3.1
Number of semesters	5.2	5.3	5.2	0.1	0.1	5.2	5.2	5.2	0.0	0.1
Cumulative GPA	--	--	--	--	--	2.5	2.5	2.5	0.0	0.1
Ever transferred (%)	22.3	21.2	23.7	-2.5	2.3	23.3	22.7	24.1	-1.4	2.7
Sample Size	1500	900	600			1167	475	692		

SOURCES: Enrollment measures come from the National Student Clearinghouse and includes enrollment anywhere. Completed credits and grades are from the University of Wisconsin System; credits and grades attained elsewhere are not observed.
NOTES:
(1) All GPA and credit outcomes are for GPA-bearing credits and are trimmed to remove unlikely or impossible reports.
(2) * signifies p<.10, ** signifies p<.05, and *** signifies p<.01.
(3) The number of semesters includes fall and spring semesters only.
(4) Only NSC outcomes are observed for the full sample. Credit and GPA outcomes for the full sample are denoted with dashes.

Table III, Panel 2. Impacts on Educational Outcomes Over Three Years.

	Subsample 2				Modestly-prepared students					Well-prepared students					Diff. in impacts by Acad.
Outcome	T	C	Impact	SE	T	C	Impact	SE		T	C	Impact	SE		Prep.
Fall 2008															
Enrolled (NSC)	98.0	98.3	-0.3	1.0	98.6	99.3	-0.7	1.6		97.9	98.0	-0.1	1.1		0.779
Attempted credits	14.6	14.4	0.2	0.1	14.2	14.0	0.2	0.3		14.7	14.5	0.2	0.1		0.992
Completed credits	14.4	14.1	0.3	0.2	15.1	14.0	1.1	0.7		14.3	14.1	0.2	0.2		0.240
1-11 credits (%)	8.7	9.8	-1.1	2.0	9.9	20.6	-10.7	5.5	*	8.4	7.3	1.1	2.1		0.045
12+ credits (%)	90.8	89.7	1.1	2.1	90.1	77.6	12.5	5.6	**	90.9	92.6	-1.7	2.1		0.019
Cumulative GPA	2.7	2.7	0.0	0.1	2.7	2.2	0.5	0.2	***	2.7	2.8	-0.1	0.1		0.004
Spring 2009															
Enrolled (NSC)	96.1	96.4	-0.3	1.4	97.2	92.5	4.7	3.7		95.8	97.3	-1.5	1.5		0.123
Attempted credits	14.1	14.1	0.0	0.2	14.1	12.8	1.3	0.6	**	14.1	14.5	-0.4	0.3		0.013
Completed credits	12.8	12.9	-0.1	0.3	12.3	10.7	1.6	0.8	**	13.0	13.4	-0.4	0.3		0.020
1-11 credits (%)	15.7	14.8	0.9	2.6	21.1	25.8	-4.7	6.8		14.3	12.1	2.2	2.7		0.349
12+ credits (%)	77.6	80.3	-2.7	3.0	71.8	61.5	10.3	7.6		79.0	84.8	-5.8	3.1	*	0.051
Cumulative GPA	2.7	2.6	0.1	0.1	2.6	2.2	0.4	0.2	***	2.7	2.7	0.0	0.1		0.002
Fall 2009															
Enrolled (NSC)	89.6	89.7	-0.1	2.2	91.5	77.8	13.7	5.8	**	89.2	92.5	-3.3	2.3		0.006
Attempted credits	12.8	12.9	-0.1	0.3	12.3	10.7	1.6	0.8	**	13.0	13.4	-0.4	0.3		0.020
Completed credits	11.5	11.3	0.2	0.4	10.5	8.5	2.0	1.0	**	11.8	12.0	-0.2	0.5		0.048
1-11 credits (%)	12.3	12.9	-0.6	2.4	14.1	17.3	-3.2	5.9		11.9	11.9	0.0	2.7		0.619
12+ credits (%)	71.4	69.6	1.8	3.3	67.6	50.5	17.1	7.9	**	72.4	74.2	-1.8	3.6		0.029
Cumulative GPA	2.6	2.6	0.0	0.1	2.6	2.1	0.5	0.1	***	2.7	2.7	0.0	0.1		0.001

Table III, Panel 2. Impacts on Educational Outcomes Over Three Years (Continued).

	Subsample 2					Modestly-prepared students					Well-prepared students					Diff. in impacts by Acad.
Outcome	T	C	Impact	SE		T	C	Impact	SE		T	C	Impact	SE		Prep.
Spring 2010																
Enrolled (NSC)	87.1	86.8	0.3	2.5		91.5	73.7	17.8	6.1	***	86.0	90.0	-4.0	2.7		0.001
Attempted credits	11.4	11.4	0.0	0.4		11.0	9.1	1.9	1.0	*	11.5	12.0	-0.5	0.5		0.045
Completed credits	10.1	10.7	-0.6	0.5		9.4	7.9	1.5	1.0		10.3	11.4	-1.1	0.5	**	0.023
1-11 credits (%)	15.1	10.2	4.9	2.4	**	22.5	16.7	5.8	6.3		13.3	8.7	4.6	2.5	*	0.852
12+ credits (%)	61.1	68.2	-7.1	3.5	**	54.9	46.4	8.5	8.1		62.6	73.3	-10.7	3.8	***	0.032
Cumulative GPA	2.6	2.6	0.0	0.1		2.6	2.2	0.4	0.1	***	2.7	2.7	0.0	0.1		0.002
Fall 2010																
Enrolled (NSC)	82.4	83.4	-1.0	2.8		83.1	72.8	10.3	6.8		82.2	85.9	-3.7	3.0		0.060
Attempted credits	10.4	10.7	-0.3	0.5		10.2	8.3	1.9	1.1	*	10.4	11.3	-0.9	0.5	*	0.021
Completed credits	9.8	9.9	-0.1	0.5		9.5	7.1	2.4	1.1	**	9.9	10.5	-0.6	0.6		0.020
1-11 credits (%)	11.2	13.8	-2.6	2.4		14.1	18.2	-4.1	5.8		10.5	12.8	-2.3	2.7		0.776
12+ credits (%)	60.2	58.8	1.4	3.6		57.7	37.4	20.3	8.0	**	60.8	63.9	-3.1	4.0		0.009
Cumulative GPA	2.6	2.6	0.0	0.1		2.6	2.2	0.4	0.1	***	2.7	2.7	0.0	0.1		0.002
Spring 2011																
Enrolled (NSC)	80.1	82.0	-1.9	2.9		81.7	65.8	15.9	7.1	**	79.7	85.9	-6.2	3.1	**	0.005
Attempted credits	9.9	10.4	-0.5	0.5		9.6	7.5	2.1	1.1	*	10.0	11.1	-1.1	0.5	**	0.009
Completed credits	8.9	9.5	-0.6	0.5		8.0	6.5	1.5	1.1		9.1	10.3	-1.2	0.6	**	0.026
1-11 credits (%)	10.4	10.6	-0.2	2.2		16.9	17.0	-0.1	6.0		8.7	9.0	-0.3	2.3		0.974
12+ credits (%)	55.7	60.9	-5.2	3.6		47.9	37.2	10.7	8.0		57.7	66.5	-8.8	4.0	**	0.029
Cumulative GPA	2.7	2.6	0.1	0.1		2.6	2.2	0.4	0.1	***	2.7	2.7	0.0	0.1		0.003

Table III, Panel 2. Impacts on Educational Outcomes Over Three Years (Continued).

	Subsample 2					Modestly-prepared students					Well-prepared students					Diff. in impacts by Acad.
Outcome	T	C	Impact	SE		T	C	Impact	SE		T	C	Impact	SE		Prep.
Cumulative Outcomes (2008-2011)																
Completed credits	69.1	69.8	-0.7	1.9		66.4	56.0	10.4	4.3	**	69.8	73.2	-3.4	2.1		0.004
1-71 credits (%)	40.9	34.9	6.0	3.6	*	47.9	58.2	-10.3	8.1		39.2	29.3	9.9	3.9	**	0.025
72+ credits (%)	58.8	65.0	-6.2	3.6	*	52.1	41.4	10.7	8.1		60.5	70.7	-10.2	3.9	***	0.020
Num. of semesters	5.3	5.4	-0.1	0.1		5.4	4.8	0.6	0.2	***	5.3	5.5	-0.2	0.1	*	0.002
Cumulative GPA	2.7	2.6	0.1	0.1		2.6	2.2	0.4	0.1	***	2.7	2.7	0.0	0.1		0.003
Ever transferred (%)	23.5	22.3	1.2	3.1		28.2	25.0	3.2	7.3		22.4	21.7	0.7	3.4		0.763
Sample Size	357	471				71	111				286	360				

SOURCES: Enrollment measures come from the National Student Clearinghouse and includes enrollment anywhere. Completed credits and grades are from the University of Wisconsin System; credits and grades attained elsewhere are not observed.

NOTES:

(1) All GPA and credit outcomes are for GPA-bearing credits and are trimmed to remove unlikely or impossible reports.

(2) * signifies p<.10, ** signifies p<.05, and *** signifies p<.01.

(3) The p-value represents a test for differences in T-C impacts between modestly and well-prepared students.

(4) "Modestly-prepared" students did not receive the Academic Competitiveness Grant (ACG) in 2008-09, while "well-prepared" students received the grant.

(5) The number of semesters includes fall and spring semesters only.

Table IV. Impacts on Work and Study Outcomes Over Two Years by Treatment Status and Academic Preparation.

	Subsample 2					Modestly-prepared students					Well-prepared students					Diff. in impacts by Acad. Prep.
Outcome	T	C	Impact	SE		T	C	Impact	SE		T	C	Impact	SE		
Fall 2008																
Worked (%)	43.9	48.9	-5.0	4.3		39.5	54.4	-14.9	10.2		44.7	47.9	-3.2	4.7		0.293
Worked on campus (%)	19.7	20.3	-0.6	3.4		9.1	12.0	-2.9	6.1		21.7	22.0	-0.3	3.9		0.718
Worked off campus (%)	26.8	34.2	-7.4	3.9	*	34.1	46.3	-12.2	9.9		25.4	31.8	-6.4	4.2		0.582
Hours worked	5.7	6.5	-0.8	0.7		6.0	7.9	-1.9	1.9		5.6	6.2	-0.6	0.8		0.554
Hours worked on campus	1.6	1.6	0.0	0.3		0.6	0.8	-0.2	0.4		1.8	1.8	0.0	0.4		0.616
Hours worked off campus	4.3	5.0	-0.7	0.7		6.0	7.0	-1.0	1.9		3.9	4.6	-0.7	0.7		0.844
Worked 20+ hours/week (%)	9.3	13.0	-3.7	2.7		11.6	13.6	-2.0	6.8		8.8	12.8	-4.0	2.9		0.784
Hours studied in last 24 hours	3.3	3.2	0.1	0.2		3.5	3.5	0.0	0.6		3.3	3.1	0.2	0.2		0.794
Fall 2009																
Worked (%)	55.1	60.9	-5.8	4.1		55.1	59.9	-4.8	9.7		55.1	61.1	-6.0	4.5		0.910
Worked on campus (%)	22.5	21.2	1.3	3.5		20.4	14.3	6.1	7.5		23.0	22.6	0.4	3.9		0.497
Worked off campus (%)	36.4	42.8	-6.4	4.0		38.8	46.1	-7.3	9.6		35.9	42.2	-6.3	4.4		0.923
Hours worked	7.9	10.4	-2.5	0.9	***	7.4	11.9	-4.5	2.3	**	8.0	10.0	-2.0	1.0	**	0.298
Hours worked on campus	2.2	2.1	0.1	0.4		1.8	1.5	0.3	0.9		2.2	2.2	0.0	0.4		0.814
Hours worked off campus	5.7	8.2	-2.5	0.9	***	5.6	10.4	-4.8	2.3	**	5.8	7.8	-2.0	1.0	**	0.256
Worked 20+ hours/week (%)	17.7	25.2	-7.5	3.4	**	16.3	33.5	-17.2	8.2	**	17.9	23.5	-5.6	3.7		0.198
Hours studied in last 24 hours	3.4	3.1	0.3	0.2		3.8	3.3	0.5	0.6		3.3	3.1	0.2	0.2		0.686

Table IV. Impacts on Work and Study Outcomes Over Two Years by Treatment Status and Academic Preparation (Continued).

	Subsample 2					Modestly-prepared students					Well-prepared students				Diff. in impacts by Acad. Prep.
Outcome	T	C	Impact	SE		T	C	Impact	SE		T	C	Impact	SE	
Change between fall 2008 and fall 2009															
Worked (%)	11.0	13.0	-2.0	5.0		16.3	8.8	7.5	10.6		10.0	13.9	-3.9	5.6	0.340
Worked on campus (%)	3.7	0.4	3.3	3.7		13.6	1.8	11.8	8.0		1.8	0.1	1.7	4.1	0.254
Worked off campus (%)	9.6	9.6	0.0	4.5		6.8	1.8	5.0	9.4		10.2	11.1	-0.9	5.1	0.578
Hours worked	2.3	4.3	-2.0	1.0	**	1.4	4.9	-3.5	2.0	*	2.5	4.2	-1.7	1.1	0.414
Hours worked on campus	0.6	0.5	0.1	0.4		1.4	0.7	0.7	0.9		0.5	0.4	0.1	0.5	0.516
Hours worked off campus	1.6	3.5	-1.9	0.9	**	-0.3	4.0	-4.3	1.9	**	1.9	3.4	-1.5	1.0	0.199
Worked 20+ hours/week (%)	9.1	13.3	-4.2	3.7		4.7	22.4	-17.7	8.3	**	10.0	11.4	-1.4	4.1	0.077
Hours studied in last 24 hours	0.1	-0.1	0.2	0.2		0.3	-0.4	0.7	0.6		0.0	0.0	0.0	0.2	0.382
Maximum sample size	288	373				49	79				239	294			

SOURCE: Fall 2008/2009 WSLS surveys

NOTES:

(1) All outcomes are trimmed to remove unlikely or impossible reports.

(2) * signifies p<.10, ** signifies p<.05, and *** signifies p<.01.

(3) The rightmost column for difference in impacts is the p-value testing for differences in T-C impacts between modestly and well-prepared students.

(4) The sample includes only students who responded to both the fall 2008 and fall 2009 surveys.

(5) "Modestly-prepared" students did not receive the Academic Competitiveness Grant (ACG) in 2008-09, while "well-prepared" students received the grant.

(6) The sample reported here is a subsample of subsample 2. The treatment and control groups are baseline equivalent; results available upon request.