Need-Based Aid and Student Outcomes:

The Effects of the Ohio College Opportunity Grant

Eric Bettinger
Stanford University School of Education
May 2010

ABSTRACT

This paper exploits a natural experiment to estimate the effects of need-based aid policies on first year college persistence rates. In Fall 2006, Ohio abruptly adopted a new state financial aid policy which was significantly more generous than the previous plan. Using student-level data and very narrowly defined sets of students, I estimate a difference-in-differences model to identify the program effects. The new program decreased dropout rates by two percent. It also increased the likelihood that students attend a four-year campus and increased their GPA's after one year.

After years of emphasizing college access, policymakers have become increasingly concerned with college completion. The reason for this is clear. Students in the United States are starting college but not completing it. For example, between 1971 and 2001, total enrollment increased by 78 percent while degree receipt increased by only 48 percent (Department of Education 2008). Additionally, while the percentage of 23-year olds with some college experience increased by 31 percent between 1971 and 1999, degree completion by this age increased by only 4 percent (Turner 2004). Part of this decline is due to students taking more time to complete degrees (e.g. Turner 2004, Dynarski and Deming 2008), yet whereas the U.S. previously led the world in the percentage of the population having bachelor's degrees, it has now lost that leadership. Over the last three decades, cohort-based completion rates have increased by 2-3 percentage points across cohorts in the US while other OECD countries such as the UK and France have seen 10-15 percentage point increases in completion rates (OECD 2007).

College affordability is one potential and oft-cited reason why some students may start college but not complete it (e.g. St. John, Cabrera, Nora, and Asker 2000). Moreover, recent federal and state financial aid policies (e.g. Federal Academic Competiveness Grants) have aimed at improving retention through improving college affordability. However, there is surprisingly little research on how need-based aid programs affect students' collegiate outcomes

Need-based aid in general has received substantial interest from educators, policymakers, and academics. Most of the research has focused on the effects of Pell grants on initial enrollment in and choice amongst colleges. The general consensus in this research is that financial aid and the corresponding college costs affect students' enrollment decisions

(e.g. Kane 1999, Ehrenberg and Sherman 1984, Leslie and Brinkman 1987). In terms of measuring the effects of aid on college outcomes, the literature is much less developed although recent work by Dynarski (2008) and Bettinger (2004) shows evidence that college graduation and retention rates improved with the generosity of the benefits.

Why has there been so little research on aid and retention? Empirically, establishing a causal link has proven difficult. Need-based aid programs are means-tested programs. As such, need-based aid recipients come from poorer families and simple comparisons between recipients and non-recipients (e.g. Wei and Carroll 2002) may not show the effects of need-based aid programs. Similarly, comparisons based on variation in the size of students' grants are flawed since college choice and the size of students' need-based aid grants are directly connected. Students who attend more expensive (and often higher quality) schools are eligible for larger grant awards than students at other colleges or universities, and even in the absence of need-based aid grants, more academically qualified students are more likely to attend more expensive schools. To measure accurately the impact of aid on retention, researchers must exploit variation in need-based grants that is independent of college choice.

In this paper, I exploit a unique natural experiment to measure the effects of need-based aid policies on student outcomes. The natural experiment that I focus on occurred in the 2006-2007 academic school year in Ohio. In this year, Ohio embarked on one of the nation's most ambitious, need-based financial aid policies, the Ohio College Opportunity Grant (OCOG), abandoning the previous policy, the Ohio Instructional Grant (OIG). The changeover to the OCOG program was abrupt, and the changes in financial aid packages were potentially large. A student who started college in 2006 could receive a financial aid award that was as much as 60 percent higher than an identical student who started college in 2005.

Similarly, some students saw their aid packages decline as compared to what they would have been if they entered college a year earlier.

To identify the effects of the change in financial aid policies on students' outcomes, I use student-level data for nearly 86,000 students who filed a Free Application for Federal Student Aid (FAFSA) in either 2005 or 2006 and attended college for the first-time in either the 2005-2006 or 2006-2007 school years. I focus only on students who were declared as dependent students by FAFSA guidelines. My data come from the Ohio Board of Regents (OBR), the agency that regulates higher education in Ohio. Because OBR collects comprehensive data on college enrollment in all Ohio's public 2- and 4-year colleges, I can track students within and across schools. I can distinguish between students who withdraw from school and students who transfer to other Ohio schools. The data also include student demographics, college entrance (ACT) exams, and other student descriptors from the surveys conducted on the ACT exam and on the FAFSA. The FAFSA data are the exact data used by the state and federal governments to determine the amount of students' need-based grant eligibility. The outcomes I focus on include choice of institution at enrollment, persistence through the first year, transfer behavior during the first year, total hours attempted during the first year, and grade point average.

Empirically, I use a difference-in-differences methodology to identify the effects of the program. Specifically, I identify students who entered college in different years yet appear similar across financial characteristics. Although these students have nearly identical financial characteristics, their financial aid awards may differ because of their exposure to the new financial aid program. For simplicity, I identify three sets of students in these comparisons. The first group is the policy "winners." Although similar, "winners" in 2006

received higher financial aid awards than their counterpart "winners" from 2005. The second group is the policy "losers." The awards for policy "losers" in 2006 were less than the awards given to similar students in 2005.[1] The final group of students were unaffected by the program in either year. I first measure the changes within these groups of students across cohorts. I then compare these changes across various types of students to measure the effect. To gain more precision, I can control more narrowly for students' financial characteristics and match students across years who have nearly identical financial characteristics.

I find evidence that student outcomes are sensitive to the amount of financial aid that they receive. Students who received more aid after the change in regimes were less likely to drop out of college or transfer from their original institution after one year than they would have been under the old regime. I find that an increase in aid of $1,000 corresponds to about a 2 percentage point decrease in drop-out rates. Students who benefited from the program were also more likely to attend a four-year campus and had slightly higher GPA's after one year.

This paper is organized as follows: Section 1 provides an overview of previous literature on the effects of need-based aid and discusses both the OIG and OCOG grant programs. Section 2 describes the data in greater detail and presents the empirical strategies. Section 3 presents the results, and Section 4 discusses the policy implications including a cost-benefit analysis of the program. Section 5 concludes.

[1] Of course, students who started college in 2005 were neither won nor lost. Their financial aid was determined by the prior financial aid system (OIG). They would have been "winners" or "losers" had they enrolled in college a year later. We use the term "winner" and "losers" to denote sets of students with similar characteristics. "Winners" either benefited or would have benefited from the policy change while "losers" did not benefit or would not have benefited. Our data are not longitudinal. We rely on repeated cross sections of students.

I. Background

Previous Research on Need-Based College Grants

Researchers have long focused on the effects of financial aid on student outcomes. Early studies of the Pell Grant generally failed to find any significant positive enrollment effect coming from the program (e.g. Manski and Wise 1983, Hansen 1983, Kane 1996). Reviews of the literature generally concluded that Pell Grants have not improved enrollment rates among low-income students and minorities but that they have likely affected which colleges students choose to attend (Kane 1999, Ehrenberg and Sherman 1984, Leslie and Brinkman 1987). Long (2004) similarly finds that students' college choice depends strongly on price, distance, and quality.

Part of the difficulty in this early research was trying to separate the effects of financial aid from confounding factors. More recent research has attempted to use "natural experiments" such as discontinuities in aid awards or changes in programs to identify the causal effects of financial aid programs. For example, Kane (2003) uses discontinuities in the Cal Grant award program to estimate the impact of the program on college decisions. He finds that the grant increased college attendance by 3 to 4 percentage points. He also concludes that the program affected college choice. Seftor and Turner (2002) focus on how need-based aid affects older, nontraditional students. They find that Pell Grants affect enrollment decisions of non-traditional students. Dynarski (2003) uses discontinuities arising from the removal of the Social Security Administration's Survivor Benefit Program. By comparing students who were eligible in the last year of the program and students who would

have been eligible had the program continued, Dynarski finds sizable effects on both access and completion.

Only a few papers focus on the effects of aid on retention decisions. Dynarski (2004), for example, shows that large, state-run merit programs also increased completion rates, and Bettinger (2004) uses discontinuities in the Pell formulae caused by small differences in family size and the number of kids in college. He finds that Pell Grants increase students' completion rates during their first year in college. In ongoing work, Goldrick-Rab and Harris (2009) examine the effects of need-based awards on students' outcomes. They are evaluating a random experiment in Wisconsin which increased aid to some students after their initial enrollment in college.

Ohio's Need-Based College Grants

Through the 2005-2006 school year, Ohio offered need-based financial aid solely based on family size and income. The award program was called the Ohio Instructional Grant (OIG). Its awards ranged from $174 to $2,190.

In late-2005, there was a growing perception that the state was not providing sufficient aid for low-income families. Enrollment and completion rates for low-income families continued to lag behind other states, and the gap between low-income and high-income enrollment and completion rates was increasing. For example, drop-out rates after the first year of college were more than 50 percent higher for students who were Pell eligible (Bettinger 2004). Additionally, the National Center for Public Policy and Higher Education (2004) had given Ohio an "F" grade on student affordability in its 2004 report. This

perception that Ohio's higher educational institutions were unaffordable led to Ohio replacing OIG with a much more generous program.

In the 2006-2007 school year, Ohio introduced a new, much more ambitious, need-based financial aid program called the Ohio College Opportunity Grant (OCOG). For students initially enrolling in the 2006-2007 school year, OCOG awards ranged between $300 and $2,496. While the range of the rewards was similar to the prior program, the distribution was not. Together with the Pell Grant, students' need-based aid award increased by 10 percent, and some students saw increases in aid as high as 60 percent relative to what similar students would have received in the preceding year.

One of the key differences in the OIG and OCOG programs was the eligibility criteria. Under OIG, the state only used income and family size to determine eligibility. By contrast, OCOG used students' estimated family contributions (EFC's) from the FAFSA as the basis for rewarding financial aid. EFC relies on family size and a broader definition of income, but it also incorporates details on family assets and the number of children in college. As a result of this shift, students from families with low incomes but high assets or alternative sources of income received less state aid while poor families with no alternative sources of income were able to receive more aid than before.

Students were grandfathered into the program, so if a student entered college under the OIG program, they remained in the OIG program throughout their college career. Similarly, a student who entered college under OCOG remained in OCOG throughout their college career. The OIG schedule between the 2005-06 and 2006-07 school years was unchanged, so we can identify how big an entering student's financial aid package would have been in 2006-07 had the program not changed.

II. Data and Empirical Strategies

Data

The data for this project come from the Ohio Board of Regents (OBR). OBR gathers admissions, transcript, and graduation data for students at Ohio's public institutions of higher education. OBR has collaborative arrangements enabling them to link students' college data with FAFSA and ACT and SAT records. The ACT exam is the most commonly used exam in Ohio, and the accompanying survey includes student-reported data on high school performance. The FAFSA data include information about the finances of both students and their families. The data also include students' "estimated family contributions" (EFC's). The EFC's are used in computing students' Pell Grant awards as well as their OCOG awards. I can use family size and income to compute students' OIG awards. I do not observe any supplementary institutional aid, either need- or merit-based.

One limitation of the data is that it only includes students attending Ohio public universities. Students from Ohio that attend universities in other states and students that attend private schools in Ohio are excluded from the sample. A majority of these students are

"dependent" by FAFSA standards (i.e. under the age of 24, unmarried, and without children), and so I focus solely on dependent students.

I focus on incoming first year students in the 2005-2006 and the 2006-2007 school years. Students in the 2005-2006 cohort were eligible for the Ohio Instructional Grant. Their financial aid was determined completely by their parents' income and family size. The students in the 2006-2007 cohort were eligible for the Ohio College Opportunity Grant. Their financial aid awards were determined by students' Expected Family Contribution. Students must submit a FAFSA to receive federal aid such as Pell Grants, and only a handful of non-FAFSA filers received state aid. I therefore limit this discussion to students who filed a FAFSA. Because the OIG schedule was unchanged between 2005 and 2006, I can use income and family size data to estimate what the OIG awards would have been for entering students in 2006-2007 even though they received aid under the new regime. I can similarly measure what students OCOG awards would have been for students entering in 2005 even though they received their aid awards under OIG.

Table 1 provides summary statistics for the sample. The table is divided into FAFSA filers and non-FAFSA filers among the two cohorts of students. It also distinguishes between students at main university campuses (UM students) and those who attend branch campuses or two year colleges (non-UM Students). As in higher education more generally, females outnumber males among FAFSA filers, but they are underrepresented among non-filers. Students at main campuses of universities tend to be slightly younger than students at other campuses, and this is consistent across both cohorts and filing statuses. There are similar proportions of non-white students at both main and other campuses for FAFSA filers, but non-FAFSA filers are a little less racially diverse at other campuses. About 87 percent of FAFSA

filers at main university campuses took the ACT while only 61 to 64 percent of filers at other campuses took the ACT exam. Rates of taking the ACT exam for non-FAFSA filers are much lower due to their propensity to take the SAT exam as a substitute. The difference in ACT scores shows the differing abilities between UM and non-UM students.

Table 1 also illustrates the difference between the different financial aid regimes across the two years. In 2005, about 21 percent of students at main university campuses received state aid. In 2006, this increased to 23 percent. Similarly at university branch and two-year campuses, the proportion of students receiving state aid increased by two percentage points. In terms of the size of the grants, Pell Grant amounts across the two years are fairly similar (within $150),[2] yet there are large differences in state aid received. State financial awards to students receiving any aid increased by almost $700 at university main campuses, representing a 52 percent increase in aid. At other campuses, state aid awards increased by 54 percent.

Table 1 also shows outcomes for students across the two years. Among FAFSA-filers, the overall number of hours students completed in their first term remained about the same across time (15.1 at main university campuses and about 12.9 hours at other campuses). Although non-FAFSA filers have fewer hours completed at other campuses, their results remain fairly consistent across the two cohorts. There was also very little change in the proportion of students who either moved from their initial institutions (by transfer or dropout) and in the proportion of students who dropped out of higher education completely. The only

[2] I compute the Pell Grants using the cost of attendance at a four-year main university campus. Actual Pell Grants depend on the cost of the university and students' levels of engagement (full-time versus part-time). Given that the level of financial aid may influence students' choices of campus and level of engagement, I report the level of Pell Grants that students would have had if they had attended similar schools.

exception is non-FAFSA filers at other campuses were more likely to transfer in the 2006 cohort (an increase from 10 percent to 13 percent).

In Table 2, I begin to show the basic elements of my identification strategy. I identify three distinct groups of students who, as groups, would have had different experiences with the financial aid regimes for the 2005-2006 and 2006-2007 school years depending on when they entered college. To identify these groups, I estimated the aid that they would have received under either the OIG or OCOG schedules. In the final three columns, I present t-statistics comparing the types of students from one year to the next within each group.

The first group I identified consists of people for whom the change in policy worsened or would have worsened their college aid package. If individuals did or would have received less aid under the new regime, I labeled them a "loser" under the policy change. About 4.1 percent of all FAFSA-filing students had worse aid packages under the new system. All of these policy "losers" would have received aid under OIG; however, only 29 percent of them received aid under OCOG. In other words, a policy “loser” type of student would have definitely received a financial aid award if he/she started school in 2005, but if he/she started school in 2006, the likelihood of receiving an award dropped dramatically. Moreover, as Table 2 shows, this group received about $550 less aid as a result of the shift in the aid program. Their state aid went from $807 to $250. Pell Grant aid decreased by almost $70 from the 2005 to the 2006 cohort of policy “losers”; these students were about 8 percentage points less likely to qualify for a Pell Grant in the later cohort. These families have lower incomes than the families of students whose aid was unaffected by the policy change, but they

have smaller family size than both other groups. They also have more predicted assets[3] than families that benefited from the shift in financial aid regime.

The second group includes people for whom the program would not have or did not lead to any change in their financial aid. About 68.8 percent of all FAFSA filers were in this second group. All of these "status quo" students received no state aid, and their average Pell Grant awards are small (less than $100). From the 2005-2006 to 2006-2007 cohorts, parental incomes increased, predicted assets decreased slightly, and family size remained constant.

The students in the final group were the "winners" in the policy change, those who had or would have had better college aid packages as a result of the policy shift. About 27.0 percent of all FAFSA-filing students were in this group. On average, these students' state financial aid packages increased by over $800 going from $1,225 to $2,030 across the two cohorts, corresponding to a 66 percent increase in state aid. Pell Grant aid increased slightly. Family size remained constant among this group across cohorts while parental income and predicted assets both fell slightly. "Winners" and "losers" differ along several dimensions. Winners were generally poorer than other students, and they have fewer assets and larger family size.

The next few rows show some basic demographics on the sample. The changes in demographics between the cohorts are statistically similar in each of the three groups for gender, race, age and the likelihood that the student lives on campus. One interesting note is

[3]I observe all of the income variables for both students and parents but do not observe any of the assets for parents or children. However, I do know families' estimated family contribution from the U.S. Department of Education. I can use the formula for the EFC and the income data to get a measure of assets. I call this the "predicted assets" of the family.

the racial comparison across the three groups. About 40 percent of the winners in the policy shift were nonwhite whereas about 21 percent of the losers in the policy shift were nonwhite and 13 percent of the "status quo" were nonwhite. There were declines in the proportion of students taking the ACT exam across all of the groups. Additionally, test scores increased in all three groups. The increase in test scores comes either from improved student preparation or is the result of some less prepared students not taking the ACT exams.

The final rows in Table 2 show a few college outcomes for these three groups across cohorts. Among the "losers," students in the later cohort completed fewer semester hours during their fall term. The "losers" also saw their dropout rates rise although the increase is not statistically significant. Among the "status quo" and "winners," dropout and transfer rates declined. Hours completed also increased for "status quo" students. The key to knowing the impact of the policy will be seeing whether the increases for winners are statistically different than the changes for the other groups. I turn to this in the next section.

Empirical Strategy

I use a difference-in-differences approach to estimate the change in policies, comparing the change in outcomes for winners and losers of the policy shift with the outcomes of those individuals for whom the policy had no effect. Specifically, I assume that students in group *i* have expected outcomes given by equation 1:

$$(1) \qquad E[y_{it}] = a_i + b_t + p_{it}$$

where i can take on values of *w, s,* or *n* representing policy winners, status quo students, or policy losers respectively, t can take on values of *0* (pre-program) or *1* (post-program), and p_{it} represents the program effect. The program effect is always zero in the absence of the

program (i.e. t=0) and takes on a value of p_w and p_n for the winners and losers respectively under the program. The program effect is zero for the status quo students.

In any period, the difference between the outcomes of any two groups *i* and *j* is given by equation 2:

(2) $E[y_{it} - y_{jt}] = (a_i - a_j) + (p_{it} - p_{jt})$

The second term $(p_{it} - p_{jt})$ completely disappears in time 0.

I focus on how the winners and losers faired relative to the status quo students. So, for example, if I compute the difference over time in the differences between winners and status quo students, I get the effect for winners.

(3) $E[y_{w1} - y_{s1}] - E[y_{w0} - y_{s0}] = p_w$

Similarly, the effect of the program on the policy losers is given by equation 4:

(4) $E[y_{n1} - y_{s1}] - E[y_{n0} - y_{s0}] = p_n$

In actually estimating the effects, I can use equation 5:

(5) $y_{it} = \alpha_i + \beta_t + \gamma\ (winner)_i * (post\text{-}reform)_t + \delta\ (loser)_i * (post\text{-}reform)_t + \varepsilon$

where winner, loser, and post-reform are indicators for the type of student or the time period in question. I can augment equation 5 by including covariates to control for individual student characteristics. In some specifications, I include zip code fixed effects so that my estimates compare similar students within narrow geographic areas.

The key identifying assumptions in the difference-in-differences approach are that there are fixed, time-invariant differences across groups and that the treatment is the only factor altering these differences over time. If the groups of individuals have different trends or trajectories, it would violate these identifying assumptions. Ideally, I would like to observe the types of students who would have been winners and losers under the new policy for many

years before the change in policy. Unfortunately, I only have data for two years – the year before the reform and the year after.

Another weakness of the difference-in-differences strategy is that I only observe the average effect of the program on each group, but the treatment effects could be quite heterogeneous within groups. For example, among policy losers, close to 25 percent of students received $250 less in total aid under the new policy than they would have received under the old regime. By contrast, about five percent of losers received $1,974 less than what they would have received. If the effects of the program vary by the amount of money the students lose or gain, then the average effects from the difference-in-differences may hide important heterogeneity in the elasticity of college outcomes to college aid.

One strategy which might help us control for some of this heterogeneity is to define the groups of "winners" and "losers" more narrowly. With my data, I can actually compute the exact EFC that each family would have had. Rather than control for winners or losers as a homogenous group, I can control for $100 increments in students' EFC amounts.[4] I can include a fixed effect for each of these groups (replacing α_i in equation 5) and look for differences in outcomes across years within these groups. In other words, rather than dividing both cohorts into three types of students (i.e. winners, losers, and status-quo students), I can further divide the groups into more homogeneous groupings (e.g. policy winners who had an EFC between $100 and $200). By including these fixed effects, I can estimate the effect of

[4] The EFC categorical variables are exactly collinear with the rewards that students received in the second year. For simplicity, I put in controls for the exact OCOG that students had in 2006 year or that they would have potentially had if they had started school in 2006.

the program by comparing students across years with almost identical financial characteristics.

Another strategy which might enable us to allow for greater heterogeneity in the effects of the program is an instrumental variable strategy. I use the policy change as an instrument for the amount of aid that students receive and estimate Equation 6. In so doing, I can examine how the amount of financial aid affects students' college outcomes.

(6) $y_{it} = \alpha_i + \beta_t + \psi*(financial\ aid) + \varepsilon$

The instrumental variables can provide unbiased estimates of aid if two conditions are met. First, I must assume that policy change does not change student outcomes for reasons other than the change in financial aid. If the policy change improved students' outcomes through other mechanisms, then the instrumental variables models will not have causal interpretations. The second condition is that the policy affected financial aid. I have already demonstrated in Table 2 that this assumption has been met.

One of the challenges in measuring the effects of financial aid policies on persistence is that the policies may also have affected who attends Ohio colleges. For example, in Table 2, the number of "losers" attending college dropped by about 625 students – nearly a 27 percent decline among "losers" once the policy was enacted. In 2005-2006, these students had OIG awards that provided some aid. Similar students who entered college a year later had less favorable terms. Similarly, the number of "winners" enrolled in 2006-2007 was about 650 students higher than in the previous year.

The problem with measuring the effects of the policies across these cohorts is that OCOG may have introduced new "winning" students to college. I do not know if these new students were a random sample of other winners, if these students were less prepared than

other winners, or if the increase in the number of winners attending college was even the result of OCOG. This knowledge is necessary to know whether or not the estimated effects are biased upward or downward.

To see this, consider the estimated effects from the difference-in-differences parameters in Equation 5. These represent the average change in persistence for the cohort of winners; however, suppose that the program had the effect of increasing the percentage of winners in the sample by η percent. If this is the case, then the estimated difference-in-differences parameter γ will be equal to

$$\gamma = \eta*\tau + (1-\eta)*\pi \quad (7)$$

where τ is the difference in persistence rates between the newly attending students and the status quo students and π is the change in persistence rates as a result of OCOG for winners who would have attended college even in the absence of the program.

If the newly attending students are similar to the overall population of winners, then $\tau=\pi=\gamma$ and my estimates are not biased. However, if these new "winners" are less prepared then existing winners then τ is likely lower than π, and the estimated difference-in-differences parameter is likely understated. I discuss the relationship of τ and π after I present my estimates below.

III. Empirical Results

Difference-in-Differences Estimates

Table 3 shows my baseline difference-in-differences estimates of OCOG's effects. I focus on how financial aid changes as a result of the Ohio College Opportunity Grant. In Column 1, I just present the simple difference-in-differences estimates without any covariates.

My standard errors correct for heterogeneity across individuals in this column and throughout the table. The "winner main effect" shows that winners received on average $4,680 more than the status quo students in both state and federal aid combined. Similarly, the "loser main effect" shows that policy losers received about $2,027 more than status quo students in both state and federal aid. Given that the status quo students received almost no aid (see Table 2), these main effects are statistically significant. The "post period" coefficient indicates that there was a small increase in overall aid of $2 but the increase was not statistically significant.

The key rows in Table 3 are the first two which report the difference-in-differences effects. Echoing the results from Table 2, losers' financial aid awards drop by about $630 while winners' awards increase by about $860. The results suggest that the program dramatically changed financial aid awards, and the estimated effects are statistically significant. In Column 2, I add controls for basic covariates including age, race, gender, whether the student lived on campus, whether the student took the ACT exam, and the student's ACT score. The key difference-in-differences estimates change very little from Column 1. In Column 3, I include fixed effects for the zip codes of students' permanent home address. Again, the estimates change very little.

In Columns 4 and 5, I also include fixed effects for the EFC categories described in the previous section. I am in essence comparing the financial aid awards across years of students living in the same zip code and having similar financial backgrounds (defined very narrowly by EFC categorical dummy variables). The estimated changes in financial aid as a result of the program are very similar to the estimates in the other columns. Policy losers' financial aid awards fall by about $630 with the introduction of the OCOG award while policy winners'

financial aid awards increase by about $750. These results clearly demonstrate that the policy led to dramatically different financial aid awards.

In Table 4, I now turn to the effects of the program on students' college outcomes. The first columns of Table 4 focus on the likelihood that students withdraw from college during their first year. The next columns focus on whether students transferred to another campus or dropped out. The "main" effects show that winners and losers are more likely to drop-out or transfer as compared to the "status quo" students. These effects are significant in my basic regressions but, at least for policy winners, the estimated effects become small and insignificant once I include fixed effects for students' financial backgrounds. Policy losers appear to be 4-5 percentage points more likely to dropout and about 3 percentage points more likely to either transfer or dropout.

My estimated difference-in-differences effects differ for both winners and losers. Policy winners appear to have lower drop-out rates than the status quo students. This estimated effect is around 2 percentage points for both drop-out and transfer behavior. The estimate is stable and consistent even in my most restrictive of specifications. Students who entered school in the 2006-2007 school year and were beneficiaries of the change in financial aid policy regimes were less likely to dropout than similar students who started college the previous year. On the other hand, policy losers' dropout and transfer rates appear unmoved by the change in the financial aid policy. The point estimate is remarkably close to zero and statistically insignificant.

The changes in aid were almost equal and opposite for winners and losers, yet the estimated effects are very different. One possibility is that these groups of individuals (i.e. winners and losers) have different elasticities of demand for education than each other. It

might also be that I just lack statistical precision in estimating the effects for losers. The group of policy losers is much smaller than the group of policy winners. The standard error bands were sufficiently large that I could not verify that the estimated effects are different for winners than for losers.

Instrumental Variables Estimates

Another way to estimate the results is to estimate an instrumental variables model. The advantage of the instrumental variable model is that I can estimate how the results differ by the amount of financial aid that students received. The first stage of the instrumental variables already appeared in Table 3. The instrumental variables estimates now appear in Table 5. I use the interaction of policy winning and the post-policy regime and the similar interaction for policy losers as instruments for the overall amount of aid received.

The first five columns focus on whether students dropped out. I find that an increase of $1,000 of financial aid leads to a reduction in the dropout rate of 1.7 to 2.4 percentage points, depending on the specification. This is true even when I control for categories of families' EFC's and when I control for geographic location. Similarly, I find that a $1,000 increase in financial aid leads to a reduction of transferring or dropping out of a similar magnitude.

These results are similar to those found in other work on retention. Using a very different methodology, Bettinger (2004), for example, found that a $1,000 change (in 2000 dollars) leads to a 2-4 percentage point reduction in dropout rates in students' first years of college. My point estimates are quite similar to these magnitudes.

An important point in the instrumental variables analysis is that the variation that is driving the instrumental variable estimates comes from the poorest part of the population. This is the group with the greatest variation in financial aid awards as a result of the policy change, and the LATE interpretation of the instrumental variables estimate centers on this population.

IV. Discussion

Robustness

The difference-in-differences estimates are valid if the differences in the trajectories of the different types of students (i.e. winners, losers, and status quo students) are constant over time except through the influence of the program. One potential problem is that the program led to differences in the composition of students from one year to the next. For example, suppose that the program increased enrollment among the poorest students in the state. These students would have likely been "winners" under the new policy, and the new financial aid might have increased the likelihood that these students enroll. As observed in Table 2, there was an increase in the population of "winners" over this period, and the average income of the "winning" population declined.

One specification check would be to see if students' characteristics changed differentially across years. To test this, I compare how student academic characteristics changed over time across groups. I report these estimates in Table 6. The difference-in-differences estimates show the relative change in composite, math, and English ACT scores across both winners and losers. In both cases, there are no significant differences across student achievement over time. Almost all of the point estimates are negative suggesting that

if anything, there has been a slight decline in winners and losers test scores over time. The lack of significance may indicate that the difference-in-differences strategy is identifying similar students.

Table 6 may also shed light on possible biases in my results. As observed in Table 2, the overall number of "winners" increased suggesting that the program could have led to an increase in college attendance among potential winners. In Equation 7, my difference-in-differences estimates are likely biased downward if new "winners" are less prepared than existing winners. Table 2 showed that winners' incomes dropped over time and Table 6 showed that the average ACT score declined as well (although this change was not statistically significant). The decline in income and test scores may suggest less prepared students did enroll as a result of the financial aid change and suggest, if anything, that the estimated difference-in-differences estimates are an understatement of the true effect.

Possible Mechanisms

There are some other outcomes that might shed light on the potential mechanisms that might explain the effect. For example, in Table 7, I examine the effects of the program on the likelihood that students attend a main university campus or a university branch campus. My difference-in-differences estimates suggest no change in the probability of enrolling at a 4-year university main campus; however, I find that there is a 1.5-2 percent increase in the likelihood that "winners" start college at any 4-year campus (i.e. a university main campus or a branch campus). Previously these students would have attended a two-year college. It could be that the increase in academic rigor that comes with attending a four-year campus may have increased the likelihood that students persist.

I also examine whether the program led to changes in the number of credit hours attempted. I find no significant effect of the change in financial aid regimes for winners, although I do find that losers actually reduce the number of hours that they take in their first semester. I also find that GPA's increased for winners by about 0.04 GPA points. The estimate is significant for winners, and while it is similar in magnitude for losers, it is not significant, likely due to the smaller sample size. This increase is small, accounting for about 3.6 percent of a standard deviation. The increase in GPA may suggest additional engagement in school. I do not observe hours that students work, but we know from prior studies that financial aid reduces the likelihood that students work (Stinebrickner and Stinebrickner 2008). We also know that working has negative impacts on student performance (Stinebrickner and Stinebrickner 2003). While I cannot test this mechanism, the working-financial aid relationship found in other literature may explain my finding here.

Cost-Benefit Analysis

Is the program worth it? I can do a simple cost-benefit analysis by comparing the costs of the program to the overall benefits. Given that I only have outcomes for the first year, I have to make some assumptions on the possible outcomes in subsequent years to measure the overall cost effectiveness. In the best case scenario, the decrease in dropout rates after the first year persists until students eventually earn their college degrees. In this case, the increase in degree receipt would be 2 percentage points. In the worst case scenario, the decrease in dropout rates only last for one year after which time students leave school having completed only one additional year of college.

To compute the internal rate of return, I estimate the following present discounted value in Equation 8:

$$(8) \quad \sum_{t=4}^{44} \frac{\alpha * N_w * y(C)}{(1+r)^t} - \sum_{t=0}^{3} \frac{\delta_W * N_w * A_w}{(1+r)^t} - \sum_{t=1}^{3} \frac{\alpha * N_w * y(C=t)}{(1+r)^t} + \sum_{t=0}^{3} \frac{\delta_L * N_L * A_L}{(1+r)^t}$$

where α denotes the proportion of students who stay in school as a result of the program; N_w and N_L denote the total number of winners and losers respectively; *y(t)* represents individuals' earnings which is a function of the years attended college (C); A_w and A_L denote the aid expense or savings for winners and losers respectively; δ_w and δ_L denote the fraction of winners and losers who remain in school after the first year; and r represents the discount rate. I assume that individuals work for 40 years after college and that college only takes four years to complete.

The first term in Equation 8 is the increase in lifetime earnings for students who now complete college rather than dropping out.[5] The second term denotes the additional cost in financial aid associated with the policy. This additional aid is paid to all winners who remain in school. The third term denotes the opportunity cost that winners face in staying in school. This only applies to those who stay in college as a result of the program. The final term denotes the cost savings for losers since their financial aid falls as a result of the program. Given that we did not observe statistically significant changes in enrollment for losers, we assume that their enrollment patterns were unchanged by the policy.

I use the previous tables for most of my estimates. There were 11,095 winners and 1,701 losers. Winners' awards increased by $806 per year while losers' awards decreased by

$557 per year (see Table 2). The overall impact on dropout rates (α) was about 2 percentage points. About 65 percent of winners and 69 percent of losers persisted after the first year. I use income measures from the College Board (2007b).

For my best case scenario, I assume that the increase in earnings is the difference between the earnings reported for a college degree ($50,900) and that of having some college but no degree ($37,100). I assume that the foregone earnings in any given year is only a function of the number of years of college that a student had completed at that time, and I assume a 10 percent return to each year of education.

In the best case scenario, the 2 percentage point decrease in dropout rates leads to a two percentage point increase in degree completion. In this case, I find that the internal rate of return to the aid program is 5.1 percent. If I allow the retention rate to decay over time, the internal rate of return can be as high as 5.5 percent.

In the worst case scenario, the 2 percentage point decrease in dropout rates only lasts one year. In this case, the opportunity cost of attending college is only one year, but the benefit from lifetime earnings is smaller representing only one additional year of schooling. Additionally, the costs of maintaining the program changes since winners (and losers) who would have remained in college in the absence of the program still have a change in their awards. In this case, the internal rate of return is -0.01 percent. If I allow the retention rate to decay over time for each group, the internal rate of return is just 0.03 percent.

The return in the best case scenario is similar to other educational programs. As a comparison, the rate of return for Project STAR's class size experiment was between 5 and 10

[5] There may be other positive benefits of education both in terms of an individual's lifetime outcomes and the externalities that their increased education may produce. These other effects are more difficult to quantify but

percent (Schanzenbach 2007), and Dynarski (2008) shows that the internal rate of return for merit-based aid programs typically located in the South (e.g. Georgia Hope) is near 9 percent. In the worst case scenario, however, the return is not good. It is not negative but it does not suggest cost efficiency.

The program could increase its efficacy if it was more effective in targeting students whose outcomes are likely affected by the new financial aid policy. The group of "winners" represents almost 25 percent of all students who filed FAFSA's. Most winners were not affected by the program, and their retention rates increased by just two percent. If policymakers could more effectively target the subsidies to the students who were on the margin of dropping out, it would reduce the overall costs of the program by decreasing the expenditures on winners who would have stayed in college regardless of the increase in aid. In more tangible terms, the state has to increase its expenditure by roughly $860 for 11,095 students. Of those 11,095 students, roughly 220 change their behavior and stay in college for an additional year. The rest either dropout like they planned or stay in college as they planned. The entire stream of benefits is realized by these 220 students. Improving the targeting would essentially reduce the total number (11,095) that receives additional aid.

Of course, as in any cost-benefit analysis, there are some caveats to the analysis. For example, I have not computed any non-wage benefits to college. I have assumed that college enrollment does not increase in response to the program. I have also not accounted for tuition increases or increases in the generosity of the program. Still the range of possible internal rates of return provides a benchmark from which it is possible to start evaluating the overall

---

would likely increase the overall return to college.

effectiveness of the program. As more data are collected, it will be possible to distinguish whether the best case or worst case scenarios are closer to the truth.

V. Conclusion

The results suggest that financial aid does impact students' decisions to withdraw from college. Using the change in policy regimes in Ohio, I show that increases in aid awards corresponds to roughly a 2 percentage point increase in college persistence rates during students' first year in college. Students were more likely to attend a four-year campus in response to the program and students' GPAs increased by a small but significant amount. Prior literature has largely examined the effects of financial aid on access, and my results contribute to this literature by demonstrating that aid influences both college choice and college persistence.

If the effects on college persistence extend beyond the first year through graduation, then the program appears to have a positive return of about 5 percent. If, on the other hand, the effects do not extend beyond the first year, the program would not appear to be cost effective. Regardless of the duration of the effects, improvements in program targeting could increase the cost-effectiveness of the program.

References

Advisory Committee on Student Financial Assistance. 2002. *Empty promises: The myth of college access in America*. Washington D.C.: U.S. Department of Education.

Bettinger, Eric. 2004. How financial aid affects persistence. In *College choices: The economics of where to go, when to go, and how to pay for it*, ed. Caroline M. Hoxby, Chicago: University of Chicago Press and NBER.

College Board. 2007a. *Education pays 2007*. Accessed 07/08/09 Available at [http://www.collegeboard.com/prod_downloads/about/news_info/cbsenior/yr2007/ed-pays-2007.pdf]. Retrieved on 13 July 2009.

College Board. 2007b. *Trends in Student Aid 2007*. Available at [http://www.collegeboard.com/prod_downloads/about/news_info/trends/trends_aid_07.pdf]. Retrieved 12 July 2009.

Dynarski, Susan. 2002. The behavioral and distributional implications of subsidies for college. *American Economic Review 92(2):* 279-285.

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

Dynarski, Susan. 2004. The new merit aid. In *College choices: The economics of where to go, when to go, and how to pay for it* ed. Caroline Hoxby. Chicago: University of Chicago Press and NBER.

Ehrenberg, Ron, and D. Sherman. 1984. Optimal financial aid policies for a selective university. *Journal of Human Resources* 19 (2): 202–230.

Hansen, W. Lee. 1983. The impact of student financial aid on access. In, *The crisis in higher education* ed. Joseph Froomkin. New York: Academy of Political Science, pp. 84–96.

Kane, Thomas, J. 1996. Lessons from the largest school voucher program: Two decades of experience with Pell Grants. In *Who chooses? Who loses? Culture, institutions and the unequal effects of school choice* ed. Bruce Fuller and Richard Elmore. New York: Teachers College Press.

Kane, Thomas J. 1999. The price of admission: Rethinking how Americans pay for college. Washington D.C.: Brookings Institution.

Kane, Thomas J. 2003. A quasi experimental estimate of the impact of financial aid on college-going. NBER Working Paper. Available at [http://www.nber.org/papers/w9703]. Retrieved 13 July 2009.

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

Leslie, Larry L., and Paul T. Brinkman. 1988. *The economic value of higher education.* New York: MacMillan.

Long, Bridget T. 2004. How have college decisions changed overtime? An application of the conditional logistic choice model. *Journal of Econometrics 121(1-2:,* 271-296.

Manski, Charles and David Wise. 1983. College choice in America. Cambridge, MA: Harvard University Press.

McPherson, M.S., and M.O. Shapiro. 1998. *The student aid game: Meeting need and rewarding talent in American higher education*. Princeton, NJ: Princeton University Press.

National Center for Public Policy and Higher Education. 2004. *Measuring up 2004: The national report card on higher education.* San Jose, CA.

Schazenbach, Diane. 2007. What have researchers learned from Project STAR? In *Brookings Papers on Education Policy 2006/2007* eds. Tom Loveless and Frederick Hess. Washington D.C.: Brookings Institution Press.

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

Stinebrickner, Ralph and Todd Stinebrickner. 2003. Working during school and academic performance. *Journal of Labor Economics 21*(2): 473-491.

Stinebrickner, Ralph and Todd Stinebrickner. 2008. The effect of credit constraints on the college drop-out decision: A direct approach using a new panel study. *American Economic Review 98*(5): 2163-84.

Van der Klaauw, W. 2002. Estimating the effect of financial aid offers on college enrollment: A regression-discontinuity approach. *International Economic Review* 43(4): 1249-1287.

Wei, Christina, and C. Dennis Carroll. 2002. Persistence and attainment of beginning students with Pell Grants. NCES Publication 2002169. Washington, D.C.: National Center for Education Statistics.

Table 1. Descriptive Statistics
Full-time First-time Freshman in 2005-2006 and 2006-2007

Variable	2005-2006 Cohort				2006-2007 Cohort			
	Non-FAFSA Filers		FAFSA Filers		Non-FAFSA Filers		FAFSA Filers	
	UM Students	Non-UM Students	UM Students	Non-UM Students	UM Students	Non-UM Students	UM Students	Non-UM Students
Female	.49	.47	.54	.54	.50	.45	.53	.52
Lives on Campus	.78	.01	.72	.03	.77	.02	.72	.04
Age	18.4 (0.6)	18.7 (0.8)	18.4 (0.6)	18.8 (1.1)	18.4 (0.6)	18.7 (.86)	18.4 (0.6)	18.7 (1.3)
Non-White	.21	.15	.21	.20	.21	.18	.21	.20
Hours Completed by End of 1st Semester	15.3 (2.1)	11.6 (4.1)	15.1 (2.0)	12.8 (3.2)	15.2 (2.1)	11.3 (4.2)	15.1 (1.9)	13.0 (3.1)
Left Institution After 1 year	.07	.10	.26	.51	.08	.13	.25	.51
Left Higher Education After 1 Year	.16	.41	.14	.37	.16	.41	.14	.37
Took ACT exam	.45	.47	.87	.64	.47	.47	.87	.61
ACT Composite Score (36=max)	22.7 (4.2)	19.0 (3.6)	22.2 (4.4)	19.2 (3.7)	22.9 (4.2)	19.5 (3.6)	22.4 (4.5)	19.2 (3.6)
Received State Financial Aid	.01	.01	.21	.34	.01	.02	.23	.36
OIG Grant	19.2 (265.0)	26.4 (239.6)	273.4 (614.0)	451.1 (747.1)	--	--	--	--
OCOG Grant	--	--	--	--	16.5 (232.5)	30.5 (258.6)	450.2 (890.1)	719.6 (1,049.7)
OIG Grant (cond'l >0)	2,355 (1,771.2)	1,749.1 (892.0)	1,275.5 (693.4)	1,311.3 (703.1)	--	--	--	--
OCOG Grant (cond'l >0)	--	--	--	--	2,499.1 (1,414.4)	1,899.4 (790.0)	1,944.2 (719.0)	2,015.2 (689.0)
Pell Grant (Cond'l on >0)	--	--	2,790.0 (1,256.8)	2,908.4 (1,243.8)	--	--	2,930.5 (1,234.7)	3,051.5 (1,208.9)
N	10,045	10,555	24,747	17,450	11,647	9,917	24,519	16,543

Note that non filers are restricted to dependent students by birth year restrictions. Standard deviations appear for non-binary variables. Data are for first time college freshman entering Ohio public colleges and universities in Fall 2005 and 2006.

Table 2. Descriptive Statistics by Winner/Loser Status

Variable	2005-2006 Cohort FAFSA Filers			2006-2007 Cohort FAFSA Filers			T-test on Equality Across Cohorts w/i Groups		
	"Losers" OIG>OCOG	Status Quo OIG=OCOG	"Winners" OIG<OCOG	"Losers"	Status Quo	"Winners"	"Losers"	Status Quo	"Winners"
Received State Financial Aid	1	0	.86	.29	0	1	--	--	--
Total Amount of State Aid	807.2	0	1,223.5	249.9	0	2,029.5	32.06**	--	80.75**
Received Pell Grant	.79	.10	1	.71	.10	1	5.28**	0.02	--
Total Pell Aid	1,315.8	96.1	3,553.0	1,247.2	98.2	3,605.2	2.17**	0.78	5.80**
Parental Income (AGI)	23,254	81,328	19,053	23,888	84,624	18,321	1.89*	9.42**	2.35**
Predicted Family Assets	1,746.7	7,258.2	225.3	1,811.3	7,105.9	-177.1	0.37	2.05**	4.60**
Expected Family Contribution	3,673.2	15,869.1	496.3	4,515.5	16,107.8	443.9	4.24**	4.10**	5.79**
Family Size	3.18	4.13	3.86	3.16	4.13	3.84	0.66	0.59	1.04
Female	.58	.53	.57	.56	.51	.56	1.08	3.60**	1.26
Lives on Campus	.31	.49	.31	.30	.51	.31	0.27	3.62**	0.26
Age	18.8	18.6	18.7	18.9	18.5	18.7	2.05**	7.97**	1.99**
Non-White	.22	.13	.41	.20	.13	.39	1.52	1.06	2.81**
Took ACT exam	.71	.82	.67	.67	.81	.66	3.20**	1.81*	1.55
ACT Composite Score (36=max)	20.2	21.9	19.2	20.5	22.1	19.3	1.44	4.98**	2.15**
Hours Completed by End of 1st Semester	13.7	14.4	13.6	13.5	14.6	13.6	2.09**	5.91**	1.99**
Left Institution After 1 year	.41	.32	.48	.42	.31	.46	0.36	1.30	2.76**
Left Higher Education After 1 Year	.30	.19	.37	.31	.18	.35	0.26	1.76*	3.18**
N	2,325	29,442	10,430	1,701	28,266	11,095			

Data are for first time college freshman entering Ohio public colleges and universities in Fall 2005 and 2006 who filed a FAFSA.

Table 3. OLS Estimates of Impact of OCOG on Total Aid

	Dependent Variable = Total Aid (000's)				
	1	2	3	4	5
	Difference-in-Difference Effects				
Winner*Post Period	0.856 (0.019)***	0.856 (0.018)***	0.856 (0.018)***	0.750 (0.006)***	0.750 (0.006)***
Loser*Post Period	-0.628 (0.044)***	-0.631 (0.045)***	0.627 (0.045)***	-0.628 (0.020)***	-0.629 (0.020)***
	Main Effects				
Winner	4.680 (0.014)***	4.574 (0.014)***	4.544 (0.014)***	1.573 (0.015)***	1.567 (0.016)***
Loser	2.027 (0.027)***	1.972 (0.027)***	1.958 (0.027)***	1.337 (0.017)***	1.332 (0.017)***
Post Period	0.002 (0.003)	0.005 (0.003)*	0.006 (0.003)*	0.003 (0.003)	0.004 (0.003)
Covariates	No	Yes	Yes	Yes	Yes
Zip Code FE	No	No	Yes	No	Yes
EFC Category FE	No	No	No	Yes	Yes
N	83,259	83,259	83,259	83,259	83,259
R Squared	0.89	0.89	0.89	0.98	0.98

Robust standard errors are provided in parentheses. Covariates include gender, race, age, whether students took the ACT, ACT score, and whether students lived on campus. Data are for first time college freshman entering Ohio public colleges and universities in Fall 2005 and 2006 who filed a FAFSA.

Table 4. OLS Estimates of Dropout and Transfer

Dependent Variable	Dropout					Transfer				
	1	2	3	4	5	6	7	8	9	10
	Difference-in-Difference Effects					Difference-in-Difference Effects				
Winner*Post Period	-0.015 (0.015)	-0.019 (0.007)***	-0.019 (0.007)***	-0.022 (0.007)***	-0.022 (0.007)***	-0.014 (0.008)*	-0.018 (0.008)*	-0.017 (0.008)**	-0.020 (0.008)***	-0.018 (0.008)**
Loser*Post Period	0.009 (0.015)	-0.001 (0.014)	-0.001 (0.014)	0.001 (0.014)	0.000 (0.014)	0.011 (0.016)	0.003 (0.016)	0.004 (0.016)	0.005 (0.016)	0.006 (0.016)
	Main Effects					Main Effects				
Winner	0.182 (0.005)***	0.095 (0.005)***	0.085 (0.005)***	0.019 (0.026)	0.007 (0.026)	0.162 (0.006)*	0.079 (0.006)***	0.071 (0.006)***	0.002 (0.030)	-0.010 (0.029)
Loser	0.114 (0.010)***	0.048 (0.009)***	0.044 (0.009)***	0.050 (0.010)***	0.048 (0.010)***	0.096 (0.011)*	0.096 (0.011)***	0.028 (0.010)***	0.035 (0.011)***	0.032 (0.011)***
Post Period	-0.006 (0.003)*	-0.001 (0.003)	0.001 (0.003)	-0.001 (0.003)	0.000 (0.003)	-0.005 (0.004)	-0.005 (0.004)	0.002 0.004	0.000 (0.004)	0.002 (0.004)
Covariates	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Zip Code FE	No	No	Yes	No	Yes	No	No	Yes	No	Yes
EFC Category FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
N	83,259	83,259	83,259	83,259	83,259	83,259	83,259	83,259	83,259	83,259
R Squared	0.03	0.13	0.15	0.13	0.15	0.02	0.09	0.11	0.09	0.12

Robust standard errors are provided in parentheses. Covariates include gender, race, age, whether students took the ACT, ACT score, and whether students lived on campus. Data are for first time college freshman entering Ohio public colleges and universities in Fall 2005 and 2006 who filed a FAFSA.

Table 5. IV Estimates of the Impact of the Amount of Financial Aid on Dropout and Transfer

Dependent Variable	Dropout					Transfer				
	1	2	3	4	5	6	7	8	9	10
Amount of Need-Based Aid (000's)	-0.017 (0.008)**	-0.019 (0.007)***	-0.019 (0.007)**	-0.024 (0.008)***	-0.024 (0.008)***	-0.016 (0.008)*	-0.018 (0.008)**	-0.018 (0.008)**	-0.023 (0.009)**	-0.022 (0.009)**
Covariates Included	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Zip Code FE	No	No	Yes	No	Yes	No	No	Yes	No	Yes
EFC Category FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Observations	83,259	83,259	83,259	83,259	83,259	83,259	83,259	83,259	83,259	83,259

Robust standard errors are provided in parentheses. Covariates include gender, race, age, whether students took the ACT, ACT score, and whether students lived on campus. Data are for first time college freshman entering Ohio public colleges and universities in Fall 2005 and 2006 who filed a FAFSA.

Table 6. ACT Comparisons in Difference-in-Differences

Dependent Variable	Composite ACT Score			Math ACT Score			English ACT Score		
	1	2	3	4	5	6	7	8	9
Winner * Post Period	-0.077	-0.032	0.002	-0.113	-0.041	-0.009	-0.165	-0.123	-0.75
	(0.072)	(0.072)	(0.071)	(0.075)	(0.074)	(0.074)	(0.092)	(0.092)	(0.092)
Loser * Post Period	-0.056	0.007	0.010	-0.070	-0.007	-0.003	-0.100	-0.043	-0.045
	(0.151)	(0.152)	(0.151)	(0.159)	(0.158)	(0.158)	(0.192)	(0.193)	(0.193)
Covariates Included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip Code FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
EFC Category FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	64,012	64,012	64,012	64,012	64,012	64,012	64,012	64,012	64,012
R-squared	0.24	0.28	0.28	0.23	029	0.29	0.21	0.25	0.25

Robust standard errors are provided in parentheses. Covariates include gender, race, age, and whether students lived on campus. Data are for first time college freshman entering Ohio public colleges and universities in Fall 2005 and 2006 who filed a FAFSA and took the ACT.

Table 7. Difference-in-Difference Estimates for Other Outcomes

Dependent Variable	Enrollment in Main Campus		Enrollment in 4-year College		Credits Earned in 1st Semester		GPA in 1st Semester	
	1	2	3	4	5	6	7	8
Winner*Post Period	0.008	0.008	0.019	0.015	-0.006	-0.022	0.036	0.037
	(0.006)	(0.005)	(0.006)***	(0.006)***	(0.040)	(0.040)	(0.018)**	(0.018)**
Loser*Post Period	0.023	0.014	0.009	-0.003	-0.238	-0.248	0.041	0.042
	(0.012)*	(0.011)	(0.013)	(0.012)	(0.086)***	(0.086)***	(0.035)	(0.035)
Winner	0.013	0.028	-0.022	-0.009	0.087	0.087	-0.014	0.008
	(0.024)	(0.023)	(0.025)	(0.023)	(0.150)	(0.149)	(0.063)	(0.062)
Loser	-0.049	-0.042	-0.036	-0.029	-0.128	-0.085	-0.033	-0.027
	(0.009)***	(0.008)***	(0.010)***	(0.008)***	(0.063)**	(0.063)	(0.026)	(0.025)
Post Period	0.001	0.002	0.000	-0.001	0.074	0.060	-0.037	-0.046
	(0.003)	(0.003)	(0.003)	(0.003)	(0.020)***	(0.020)***	(0.008)***	(0.008)***
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip Code FE	No	Yes	No	Yes	No	Yes	No	Yes
EFC Category FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	83,259	83,259	83,259	83,259	83,259	83,259	80,543	80,543
R-squared	0.49	0.59	0.33	0.46	0.22	0.26	0.17	0.19

Robust standard errors are provided in parentheses. Covariates include gender, race, age, whether students took the ACT, ACT score, and whether students lived on campus. Data are for first time college freshman entering Ohio public colleges and universities in Fall 2005 and 2006 who filed a FAFSA.