Financial Aid Package Composition and Postsecondary Outcomes for Low-income Students: Evidence from the Spartan Advantage Program

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Abstract

This study examines the impact of substituting loans with need-based gift aid on postsecondary outcomes for low-income students. Using a regression discontinuity design based on the income eligibility threshold for Michigan State University's Spartan Advantage Program (SPAD), I estimate how altering the composition of a financial aid package—specifically replacing loans with gift aid—affects college outcomes for students just below the federal poverty line. While the change in aid composition did not affect persistence or graduation rates, it did influence students' major choices. SPAD recipients were less likely than non-recipients to remain in STEM fields between their admissions application—submitted before receiving receiving their financial aid package—and graduation. Further, they were more likely to move toward majors that better reflected their academic preparation, indicating improved fit between students and their field of study. These findings suggest that changes in aid composition can meaningfully shape academic choices, even in the absence of effects on persistence or degree completion.

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INTRODUCTION

As the cost of higher education continues to rise, students from low-income backgrounds face increasing challenges in affording and succeeding in postsecondary education, exacerbating a substantial attainment gap (Goldrick-Rab et al., 2016). The benefits of college access and completion are well-documented, including higher weekly wages, greater lifetime earnings, improved employment levels, higher civic participation rates, and better early-life outcomes for the children of degree holders (Lochner, 2011; Oreopoulos and Salvanes, 2011; Page and Scott-Clayton, 2016; Torpey, 2021). However, many students are unable to realize these benefits without incurring substantial student loan debt, which may significantly impact both enrollment decisions and career paths.

As illustrated in Figure 1, the amount of student loans and gift aid[1] nearly tripled between 2000 and 2009, leading to a surge in research on the effects of increased aid—whether through loans, gift aid, or both—on student enrollment, persistence, graduation, and major choice. Since 2010, however, total financial aid has stabilized, with gift aid emerging as the predominant form of financial support. The practice of replacing loans with gift aid has also become increasingly common at many institutions, resulting in significant changes in the composition of aid packages for individual students.

Numerous universities[2] have implemented various forms of no-loan policies or similar aid programs that cover unmet financial needs with gift aid for certain students. Despite the growing adoption of these policies, there remains a significant gap in understanding how changes in the composition of aid packages affect student outcomes. This paper seeks to address this gap by examining a financial aid program that replaces loans with gift aid, analyzing its impact on student persistence, graduation rates, and major choice.

Specifically, I study the Spartan Advantage (henceforth "SPAD") program at Michigan State University (MSU), which is designed to reduce loan balances for in-state students with the greatest financial needs. Due to the structure of the program, students on either side of the eligibility threshold receive similar amounts of total aid, but the composition of their aid

[1]The term "gift aid" is used here to refer to any financial aid a student receives that does not require repayment, whether merit- or need-based.

[2]This includes several Ivy League schools, the majority of the University of California system, the University of North Carolina at Chapel Hill, and the University of Illinois at Urbana-Champaign, among others.

packages differs. Over four years, students who narrowly qualify for SPAD receive over $16,000 more in gift aid than those just above the eligibility threshold, accompanied by a reduction of more than $12,000 in loans. While there is a modest increase in total aid, the bulk of the increase in gift aid effectively replaces loans, resulting in a predominantly grant-based aid package.

The results suggest that replacing loans with gift aid has no impact on persistence or graduation. However, the change in aid composition appears to influence students' academic decisions, particularly their major choice. SPAD recipients are less likely to remain in STEM fields and more likely to switch into majors better aligned with their academic preparation. Notably, these patterns stand in contrast to students' initial preferences: at admission, future SPAD recipients are more likely to select STEM majors and more likely to choose majors with median ACT scores exceeding their own. By graduation, these differences have reversed: SPAD recipients are less likely to be in STEM fields and more likely to graduate with a major more closely aligned with their own academic preparation, as measured by median ACT scores. This suggests that reducing reliance on loans shifts students' priorities, allowing them to reoptimize their major decisions in response to changes in their expected financial burden—though the long-term implications of these shifts are less clear.

The structure of this paper is as follows: The next section frames the unique contributions of this paper within the existing financial aid literature. This is followed by an overview of the SPAD program, a description of the data, and a discussion of the methodological approaches. The paper then presents key findings and concludes with a discussion of broader implications.

LITERATURE REVIEW

Researchers have proposed various mechanisms through which financial aid may influence students' postsecondary outcomes. Cabrera et al. (1990) and Cabrera et al. (1992) extended Tinto (1975)'s attrition model by incorporating a student's ability to pay, suggesting that financial stress affects both academic and social integration. Nora et al. (2006) argued that increased financial aid reduces the need for students to work, thereby increasing their free time and lowering their stress levels. Further, need-based gift aid is believed to enhance students' investment in their education by relaxing credit constraints (income effect) and reducing attendance costs relative to outside options (substitution effect) (Dynarski and Scott-Clayton, 2013; Castleman and Long, 2016).

Previous empirical research in this field is extensive, encompassing various student outcomes, such as enrollment (Deming and Dynarski, 2009; Page and Scott-Clayton, 2016; Harris and Mills, 2021), academic performance (Clotfelter et al., 2018; Denning, 2019; Nguyen et al., 2019), major choice (Rothstein and Rouse, 2011; Castleman et al., 2018; Broton and Monaghan, 2023), and postgraduate financial health (Denning et al., 2019; Smith et al., 2020). Generally, increases in gift aid have been shown to positively impact academic outcomes and STEM degree completion (Bettinger, 2004; Scott-Clayton, 2011; DesJardins and McCall, 2014), whereas the effects of loans on these outcomes are less clear (Chiteji, 2007; Rothstein and Rouse, 2011). In contrast, loans are more closely associated with major choice, with most studies finding that increased loans lead students to choose higher-paying majors or pursue jobs with higher initial salaries (Minicozzi, 2005; Luo and Mongey, 2019; Gervais and Ziebarth, 2019).

State-level aid programs are significant sources of research on financial aid, with much of the literature examining the impact of simultaneous increases in both gift aid and total aid on student outcomes. One of the most extensively studied programs is the Wisconsin Scholar Grant (WSG), which provides up to $3,500 in gift aid to public high school students in Wisconsin who plan to attend any public 2- or 4-year university in the state. Recipients are randomly selected from the pool of students who completed the Free Application for Federal Student Aid (FAFSA), received a Pell Grant, and had at least $1 of remaining unmet need.

Numerous studies have documented significant positive effects of the WSG program. For example, Goldrick-Rab et al. (2012) reported that students who received $1,000 more in total aid were 2.8 to 4.1 percentage points (pp) more likely to persist to their second year and complete a full-time course load. Additionally, Goldrick-Rab et al. (2016) suggested that additional gift aid contributed to higher four-year graduation rates. Furthermore, Anderson et al. (2020) and Broton and Monaghan (2023) reported that WSG recipients were more likely to major in a STEM field.

Like the WSG, the Florida Student Access Grant (FSAG) awards in-state students gift aid that can be used at any 2- or 4-year public college in Florida. In the 2000-2001 academic year, students with an expected family contribution (EFC) less than $1,590 were eligible to receive approximately $1,300 in gift aid from the program. Using a regression discontinuity (RD) design, Castleman and Long (2016) and Castleman et al. (2018) reported that FSAG recipients had higher levels of persistence, credit completion, six-year graduation rates, and STEM course and degree completion than those who were just above the EFC threshold.

Universities have also provided rich opportunities for researchers to examine a variety of financial aid programs. For example, Clotfelter et al. (2018) studied the Carolina Covenant program at the University of North Carolina at Chapel Hill, which offers gift aid covering the full cost of attendance for high-achieving, low-income students (those below 200% of the federal poverty line), regardless of residency or transfer status. Early cohorts received little non-financial support and approximately $1,300 more in total aid and gift aid, with no significant change in loans. While these students did not have significantly better academic outcomes, they chose non-STEM majors at a higher rate. Students in later cohorts that received additional non-financial support and a $1,000 substitution of loans for gift aid (without a significant increase in total aid) demonstrated higher levels of credit completion and GPA, although there were no discernible impacts on their major choice.

Universities that have implemented no-loan policies present a setting most comparable to the one explored in this paper. Since the early 2000s, nearly 100 four-year universities across the U.S. have adopted some version of a no-loan policy. These policies vary in scope—some eliminate the need for loans for all students receiving aid, whereas others target students with a predetermined level of need. However, they may not entirely eliminate loans if students opt to replace work-study awards with loans or take out loans to cover personal expenses. As a result, reductions in loans may not perfectly align with increases in gift aid, potentially leading to changes in the overall amount of aid received.

Rothstein and Rouse (2011) examined the implementation of a no-loan policy at a highly-selective university. Beginning with the fall 1998 entering cohort, students with an AGI below $40,000— representing just under 20% of all students—were covered by this policy. In early 2001, the university expanded the policy to include all aid recipients in future cohorts. Utilizing a difference-in-differences (DiD) based estimator, the authors found that holding an additional $10,000 in debt led students to accept jobs with annual salaries that were $2,000 higher on average. Conversely, they found that recipients of the no-loan policy tended to move from industries with high average salaries to those with lower salaries.

A more recent work by Hampole (2023) examined the staggered adoption of universal no-loan policies—where all students receiving aid qualify for the program—at 22 universities across the U.S. These universities tend to be more selective and expensive than typical four-year institutions and enroll students from more advantaged backgrounds. Using a DiD design, the author compared these universities to 10 similarly prestigious universities that did not have such policies. The study revealed that a $10,000 decrease in loans resulted in a 4% increase in the likelihood of choosing a "high-earning" major. However, this masked an intertemporal trade-off, as these high-earning majors often have lower initial earnings but higher long-term growth rates. Additionally, the author showed that these majors were more demanding. The effects were most pronounced for students in the bottom socioeconomic status tercile, supporting the notion of high debt aversion or binding credit constraints among lower-income students (Chen, 2008; Gicheva and Thompson, 2015).

Building on previous research, I exploit shocks to students' aid packages among those who just qualify for the SPAD program, comparing observations of their major choices before and after receiving the award notification. Moreover, I provide a comprehensive analysis of the effects on all components of a student's aid package—total aid, loans, gift aid, and work-study—rather than focusing solely on loans. This approach reveals the causal impact of a substantial shift in aid composition, accompanied by only a modest increase in total aid. Since SPAD does not include any non-financial components, I am able to isolate the effects of substituting loans with gift aid on various academic outcomes and major choice.

Additionally, SPAD recipients are more representative of the broader low-income student population that future policies are likely to prioritize. The SPAD program specifically targets students at or below the poverty line, focusing on those from significantly more disadvantaged backgrounds than those examined in the existing literature. With an estimated 1 in 8 individuals below the poverty line and 1 in 5 below 150% of the poverty line (Bureau of the Census for the Bureau of Labor Statistics, 2023), previous research has not adequately addressed the effects of financial aid on this substantial segment of the population.

Moreover, unlike the institutions that have adopted no-loan policies, MSU is a land-grant university that accepts over 75% of applicants and boasts a student body that is diverse both demographically and academically. Consequently, SPAD recipients more closely resemble the average financial aid recipient than those at more selective institutions. Furthermore, the SPAD program does not include a merit component in awarding or renewing aid; students need only maintain good academic standing to continue receiving support.

Therefore, this paper explores the effects of changes in financial aid composition on fundamentally different populations of students than those studied in previous works. Given that impacts may vary based on the basis of financial needs (Angrist et al., 2009; Herbaut and Geven, 2020), and that students with the greatest need are likely to be the primary targets of future policies, this work has significant relevance for a wide range of individuals.

SPAD PROGRAM

The SPAD program was first introduced to the fall 2006 entering cohort. Approximately 5% of each incoming class receives SPAD. Early cohorts consisted of 200-300 students, but as MSU enrollment has grown, the SPAD program has expanded accordingly. More recent cohorts have averaged 500-600 recipients.

To qualify for SPAD, students must file a FAFSA and meet all of the following criteria:

1. First-time enrollee: Students must be enrolling at MSU for the first time; neither transfer students nor previously ineligible returning students are eligible.
2. Full-time enrollment: Students must be enrolled full-time.
3. Michigan residency: Students must be Michigan residents.[3]
4. Dependent status: Students must be claimed as dependents for tax purposes.
5. Income threshold: Students must have an AGI below the federal poverty line for their household size and entry year.[4]
6. Maximum Pell Grant eligibility: Students must be eligible for the maximum Pell Grant.
7. Asset and investment limits: Students must not hold assets or investments that, when combined with income, would disqualify them based on federal poverty guidelines.

Students who qualify for SPAD can receive aid for up to 10 semesters, provided that they continue to meet these requirements each year and maintain federal standards for satisfactory

[3] I restrict the sample to students who attended high school in Michigan, thus excluding a small number of students who qualify for in-state tuition via Veterans Affairs Educational Assistance Programs, migrant worker status, and other routes for receiving in-state tuition.

[4] MSU uses the Department of Health and Human Services poverty guidelines in administering financial aid.

progress toward their degree. While other state and MSU financial aid programs have financial need requirements, SPAD is unique in its use of the federal poverty line as an eligibility threshold.

While SPAD eligibility is determined by the requirements listed above, meeting these criteria does not always result in receiving the grant. Among first-time, in-state, dependent students enrolled full-time, approximately 75% of those just below the poverty line receive SPAD. Likewise, roughly 15% of students just above the poverty line also receive the grant, as shown in Figure 2. This imperfect take-up likely reflects several unobserved or inconsistently enforced components of eligibility. Two formal criteria beyond income may contribute: the asset requirement—which is rarely enforced and not observed for the entire span of my data[5]—and the maximum Pell Grant requirement, which is almost always satisfied by students below the poverty line.[6]

Yet even after accounting for these, other unobserved factors may contribute to the imperfect alignment between eligibility and receipt. For example, some cohorts were required to attend a SPAD orientation session, and failure to attend could result in loss of eligibility. However, individuals in the Office of Financial Aid suggested that this requirement was not strictly enforced, although I lack data to confirm attendance. I also do not observe whether students submitted the FAFSA or admissions application before the processing deadlines used to award SPAD. Finally, administrative error or discretion may also play a role. Taken together, these factors likely explain the imperfect take-up.

As mentioned earlier, SPAD aims to reduce the loan balances of students with the greatest financial needs, resulting in financial aid packages that are predominantly composed of gift aid and have minimal loans. However, the university encourages SPAD recipients to take out a small amount of loans each year as a means of fostering a personal investment in their education.[7] Despite this encouragement, many SPAD students decline all loans offered to them.[8] Of those

[5]According to administrators in the Office of Financial Aid, the asset requirement is evaluated on a case-by-case basis and seldom leads to disqualification from SPAD. Furthermore, fewer than 5% of students reported any assets in the two years of available asset data.

[6]In this dataset, fewer than 1% of students below the poverty line do not qualify for the maximum Pell Grant.

[7]An administrator in the Office of Financial Aid suggested that this encouragement of taking out loans was at one point a mandate, functioning as a kind of "self-help" requirement. However, the data indicate that this was not strictly enforced if it was indeed a policy, as many SPAD recipients had no loans, with these students spread across all program cohorts.

[8]Self-help components are built into many financial aid programs. Typically, these programs offer a maximum

SPAD recipients who do take out loans, few borrow more than $2,000 per year.

The SPAD program provides last-dollar aid, meaning it factors in all other external aid a student receives before the SPAD award is determined. In other words, SPAD is applied after other aid—such as the Pell Grant—has been accounted for in a student's aid package, which MSU compiles and sends out in one offer. Importantly, SPAD cannot increase a student's total aid beyond the cost of tuition, room and board, and other mandatory fees. This results in potential differences in the sources of aid within a SPAD student's package, but all recipients have similar levels of total aid, most of which is gift aid. For the most recent academic year, the typical SPAD award was $17,321 in institutional gift aid, which covered 73% of the remaining cost of attendance after factoring in the maximum Pell Grant ($6,345) that all recipients receive. If a student does not receive additional outside aid, their remaining unmet need is covered through a combination of extra gift aid via SPAD, federal work-study awards (up to $3,000 per year), and possibly loans.

Students who do not qualify for SPAD but have significant financial needs receive the Student Aid Grant (SAG), which provides up to $9,200 in institutional gift aid—over $8,000 less than the average SPAD award. SAG students near the cutoff for SPAD must also cover the full cost of attendance through financial aid, resulting in similar total aid to that of SPAD students but with significantly greater reliance on loans.

There is no separate application for either of these programs—students who submit a FAFSA are automatically considered for both. Most students are unaware of these programs until they receive their financial aid package, which typically occurs in March or April. With a deposit deadline of May 1st, students have the opportunity to consider their aid packages when deciding where to enroll.[9] Neither SPAD nor SAG include any non-financial support.

award, with any remaining attendance costs not covered by the award or external gift aid being met by self-help components (loans and work-studies). Some institutions, such as the one discussed in Rothstein and Rouse (2011) before it implemented a no-loan policy, cap the amount of self-help a student must undertake before additional gift aid is provided. At that particular university, students who worked 10 hours of work-study per week and took out $4,500 in loans annually were eligible to receive gift aid that covered all remaining attendance costs. I am unaware of aid programs beyond SPAD that suggest that students take out loans, but cover the full cost of attendance even if a student has $0 in loans.

[9]Historically, approximately two-thirds of in-state students who deposit at MSU do so between March 1st and May 1st.

DATA

I utilize a rich dataset of student-level administrative data from MSU, focusing on dependent, full-time Michigan residents who first enrolled between the fall of 2000 and the fall of 2020. The data include a variety of demographic, academic, and financial information derived from admissions applications, student academic records, and financial aid files. The dataset extends through spring 2024, allowing me to observe at least four years of outcomes for all cohorts and up to six years for all but the two most recent cohorts.

The admissions data contain demographic details such as race, gender, high school grade point average (GPA), parental education, first-generation status, entry term, and intended major. The major listed on the application is the student's official major until any changes are made. Academic records track each student's semester-by-semester progress, including the number of credits attempted and completed, cumulative GPA, major, and graduation term. Financial aid data include detailed family financial variables such as parental and student AGI, EFC, household size, and an itemized list of all sources and amounts of financial aid offered and disbursed to each student.[10] The SPAD program has a unique aid code, allowing for the identification of recipients.

Additionally, I integrate major-specific earnings information from the U.S. Census Bureau's Post-Secondary Employment Outcomes (PSEO) dataset. Specifically, I use the median annual earnings one year after graduation, reported for each CIP code in 3-year graduation cohort intervals, and expressed in 2022 real dollars.[11] I then match students by four-digit Classification of Instructional Programs (CIP) codes and nearest expected graduation cohorts, providing an associated earnings value for each student's admission and graduation majors. These earnings data help contextualize how changes in financial aid composition may influence students' academic decisions, particularly shifts in major between admission and graduation.

[10]All financial aid is expressed in real 2023 dollars using the Bureau of Labor Statistics' college tuition and fees price index (Series ID: CUUR0000SEEB01).

[11]The PSEO data is provided in 2022 dollars, adjusted using the Bureau of Labor Statistics' Consumer Price Index for All Urban Consumers—Series ID: CUUR0000SEEB01.

METHODOLOGY

Regression Discontinuity

As shown in Table 1, SPAD recipients differ significantly from the in-state MSU population across a range of characteristics. For example, while 76% of MSU in-state undergraduates are White, 9% are Black, and 55% are female, these percentages are 42%, 33%, and 62%, respectively, for SPAD recipients. Moreover, a significantly larger share of SPAD recipients are first-generation college students (56% vs. 21%), and they enter with lower HS GPAs (3.52 vs. 3.61).

A more appropriate comparison may be in-state financial aid recipients who do not receive SPAD. However, this group accounts for two-third of all in-state students and closely resembles the full in-state population. Additionally, because SPAD eligibility requires an AGI below the federal poverty line, the average SPAD recipient has an EFC of only $380 and an AGI of $12,606, compared to $29,101 and $125,318 for non-SPAD aid recipients—differences that are also statistically significant.

Due to the well-documented relationship between race, gender, first-generation status (Warburton et al., 2001; Chen, 2005; Arcidiacono and Koedel, 2014), family income (Jacobson and Mokher, 2009; Bailey and Dynarski, 2011; Solórzano et al., 2013; Backes et al., 2015; Hardy and Marcotte, 2020) and postsecondary outcomes, I focus my analysis on students near the income eligibility threshold in an RD framework to estimate the causal effect of receiving SPAD. In doing so, I remove possible confounding factors related to income or other student characteristics that could arise and instead compare students with similar observable characteristics. By restricting the dataset to dependent, in-state, non-transfer, full-time students, the income-based requirements become the primary determinants of SPAD eligibility. As students below the poverty line typically qualify for the maximum Pell Grant and the asset requirement is rarely enforced, AGI serves as the sole running variable in this analysis.

As discussed earlier and shown in Figure 2, not all students below the poverty line receive SPAD, while some above it do. This imperfect compliance with eligibility results in a fuzzy RD design, with AGI as the running variable. I center each student's AGI at $0 by subtracting the federal poverty line for their household size and entry year.[12] Students with a centered AGI at or below \$0 are eligible to receive SPAD. For example, the federal poverty line for a three-person household in 2006 was \$16,600 (Office of the Assistant Secretary for Planning and Evaluation, 2024). A student entering in fall 2006 from a three-person household with a total AGI of \$14,305 would have a centered AGI of -\$2,295, thus making them eligible for SPAD.

As this is a fuzzy RD design, I employ two-stage least squares (2SLS) estimation. The first-stage is specified in Equation (1):

$$SPAD_{i,t=1} = \alpha + \delta Below_{i,t=1} + f_1(\tilde{R}_{i,t=1}) + \epsilon_{i,t=1} \tag{1}$$

where $SPAD_{i,t=1}$ equals 1 if student i received SPAD in their first year (t=1), $Below_{i,t=1}$ is an indicator for SPAD eligibility equal to 1 if student i's centered AGI ($\tilde{R}_{i,t=1}$) is ≤ 0 at t=1, and $f_1(\tilde{R}_{i,t=1})$ represents a flexible function of the running variable. I focus on SPAD receipt during the first year since students falling below the poverty line after their first year are ineligible for SPAD, and few students who initially receive SPAD move above the poverty line in subsequent years. First-stage estimates range from 40 to 60 pp depending on the bandwidth, with all estimates being highly significant (t>17).

The second-stage takes the basic form shown in Equation (2):

$$Y_{it} = \alpha + \beta SPAD_{i,t=1} + f_2(\tilde{R}_{i,t=1}) + \gamma X_i + Cohort_i + College_{i,t=1} + \epsilon_{it} \tag{2}$$

where Y_{it} represents an outcome related to student i's persistence, graduation, or major; X_i is a vector of time-constant observable characteristics for student i; $Cohort_i$ and $College_{i,t=1}$ are cohort and initial college fixed effects, respectively; and all other variables retain their previous definitions. First-stage estimates of SPAD receipt ($SP\hat{A}D_{i,t=1}$) are used in place of $SPAD_{i,t=1}$ and $f_2(\tilde{R}_{i,t=1})$ is linear with an included interaction term with $Below_{i,t=1}$, allowing the relationship between AGI and the outcome to vary on either side of the eligibility cutoff. The

[12] Although federal poverty guidelines include the number of individuals under the age of 18, this factor is not considered in the SPAD eligibility criteria.

final model used in analysis is shown in Equation (3):

$$Y_{it} = \alpha + \beta S\hat{PAD}_{i,t=1} + \omega \tilde{R}_{i,t=1} + \lambda(Below_{i,t=1} \times \tilde{R}_{i,t=1}) + \gamma X_i + Cohort_i + College_{i,t=1} + \epsilon_{it} \quad (3)$$

where β represents the local average treatment effect (LATE) of SPAD receipt for students close to the eligibility threshold. Following the procedure outlined in Calonico et al. (2014), the optimal bandwidth is just over \$12,000, and a uniform kernel is used throughout the analysis.

Difference-in-Discontinuities

While the RD framework identifies the causal effect of SPAD receipt on outcomes like persistence and graduation, it is not well-suited to outcomes where students have observable pre-treatment choices—such as their major—that may already differ across the poverty line. For these outcomes, I adopt a difference-in-discontinuities (diff.-in-disc.) design, which compares changes in major characteristics between a student's admission major and their graduation major.[13] This approach nets out any pre-existing discontinuities in academic preferences that may result from other programs using the federal poverty line as an eligibility threshold, thereby isolating the effect of SPAD receipt on students' major trajectories.

MSU collects students' intended majors at the time of application, well before financial aid offers are distributed. As a result, initial major choices reflect expectations formed under a presumed non-SPAD financial aid offer. Then, the notification of SPAD receipt acts as an exogenous shock to a student's financial aid package and expected debt burden, potentially influencing whether they remain in their original major or switch to a different field.

Figure 3 presents a stylized, simulated example for illustrative purposes. Panel (a) shows a pre-existing (pre-aid notification) discontinuity at the poverty line, with students just below the poverty line having higher values. Panel (b) displays the post-treatment (after aid notification) relationship, where the discontinuity is no longer evident. An RD estimate using the post-period would suggest a null effect, while the diff.-in-disc. approach subtracts the pre-period discontinuity (τ_0) from the post-period discontinuity (τ_1). In this example, the estimated effect would be negative, capturing a meaningful shift once pre-existing differences are removed.

This strategy is necessary because the federal poverty line is used to determine eligibility for

[13] For students who do not graduate, I instead use their major during their final semester enrolled.

a wide range of means-tested federal programs. Students just below the threshold are more likely to have had exposure to programs such as Medicaid (pre-Affordable Care Act), Head Start, the Hill-Burton Act, Job Corps, and Job Opportunities for Low-Income Individuals (JOLI) Grants, which provide healthcare coverage, early childhood education, emergency medical services, and job and skill enhancement opportunities. These programs all use the federal poverty line as an eligibility criterion, increasing the likelihood that SPAD recipients were previously exposed to them.[14]

In addition, students below the poverty line are also more likely to participate in programs with income thresholds slightly above the cutoff, such as the Medicare Part D Low Income Subsidy (prescription drug coverage), SNAP (food assistance), the National School Lunch Program (free school lunches), and the Low-Income Household Water Assistance Program (utility bill assistance). Differential exposure to these programs could shape students' attitudes toward debt, financial risk, and career decision-making—preferences that can influence initial major choices.

This interpretation is supported by students' initial majors. As shown in Figure 4, students below the poverty line enter college with majors associated with median first-year earnings nearly $1,000 higher than those of students just above the threshold. While this estimated discontinuity falls just short of conventional significance ($p = 0.12$),[15] its direction and magnitude align with theoretical expectations and provide empirical motivation for the diff.-in-disc. design. Because students on either side of the threshold were unaware of their SPAD status when selecting their intended major—and MSU does not consider major choice in awarding aid—this difference is unlikely to reflect any impact of SPAD itself, but rather points to underlying financial attitudes shaped by differential exposure to federal means-tested programs.

By comparing within-student changes in major characteristics from application to graduation, the diff.-in-disc. strategy isolates the effect of SPAD from baseline differences that the RD design

[14]While individuals above the poverty line can qualify for these programs if they meet other requirements, those below the poverty line automatically qualify.

[15]Estimate from Equation 5, using the median earnings associated with student i's admission major as the outcome.

cannot capture. The model specification for this approach is outlined in Equation (4):

$$
\begin{aligned}
Y_{it} = \delta_0 + \delta_1 \tilde{R}_{i,t=1} + S\hat{PAD}_{i,t=1}(\gamma_0 + \gamma_1 \tilde{R}_{i,t=1}) + Aid_{it}[\alpha_0 + \alpha_1 \tilde{R}_{i,t=1} + \\
S\hat{PAD}_{i,t=1}(\beta_0 + \beta_1 \tilde{R}_{i,t=1})] + \gamma X_i + Cohort_i + College_{i,t=1} + \epsilon_{it}
\end{aligned} \tag{4}
$$

where Aid_{it} is an indicator equal to 1 for the graduation major observation (i.e., post-aid) and 0 for the admission major observation (i.e., pre-aid), β_0 represents the LATE, and all other variables are as previously defined.

RESULTS

RD Identifying Assumptions

The validity of the RD design relies on two assumptions: (1) that students near the threshold have, on average, similar observable and unobservable characteristics, and (2) that students cannot precisely manipulate their AGI to qualify for SPAD. To assess (1), I examine whether students on either side of the threshold are comparable in terms of observable characteristics. Figures A.1 and A.2 (available in the Online Appendix) plot all observable student characteristics. Visually, all differences across the poverty line are small. This is reinforced by the covariate balance check in Table A.1, based on Equation (5), which finds no statistically significant differences.

$$
X_i = \alpha + \beta Below_{i,t=1} + \theta \tilde{R}_{i,t=1} + \lambda(Below_{i,t=1} \times \tilde{R}_{i,t=1}) + \epsilon_i \tag{5}
$$

Additional analysis further reinforces this conclusion. First, an omnibus F-test, which tests the null hypothesis that all estimates in Table A.1 are equal to zero, returns a p-value of 0.403, indicating no significant differences. Second, regressing a student's total aid, gift aid, and loans during their first year on all observed student characteristics and plotting the predicted values shows continuity across the threshold.[16] These predicted values, displayed in Figure A.3 in the Online Appendix, reveal no significant jumps across the threshold. Taken together, this evidence reinforces the assumption that students just above and below the threshold are similar in their observable characteristics. While I am unable to assess differences in unobservable characteristics,

[16]See Table A.2 in the Online Appendix for estimated coefficients.

such as motivation or expectations, the absence of any discontinuities in observed traits provides indirect support for the assumption that unobservables are also smoothly distributed near the threshold.

Turning to the second assumption, manipulating AGI to qualify for SPAD would require both awareness of the program and familiarity with MSU's specific eligibility guidelines. However, SPAD is not widely advertised, and students who attempt to adjust their AGI face significant risks—FAFSA audits that detect intentional misreporting can result in delayed or complete loss of financial aid. To assess the potential for strategic manipulation, Figure 5 plots the density of students around the poverty line. Visually, there is no noticeable spike at the threshold. Additionally, the McCrary (2008) density test, which formally tests for discontinuities, returns a p-value of 0.564, providing further evidence against the presence of manipulation.

Financial Aid

To illustrate the impact of SPAD on financial aid, Figure 6 presents the amount of total aid, gift aid, and loans students around the poverty line receive in their first year. Among students just below the threshold, total aid remains relatively constant, but gift aid increases by approximately $2,500 and loans decrease by a similar amount—reflecting a compositional shift in financial aid. Table 2 complements this figure by reporting estimates of SPAD receipt on the cumulative amount of each aid component a student receives over four years.[17] Loans include subsidized, unsubsidized, and reported outside loans, but do not capture any non-reported loans or credit card debt.

As shown in the first row, receiving SPAD significantly increased a student's cumulative total aid during each of their first four years. In the first year, total aid increased by $1,338, driven by a $5,230 increase in gift aid and a $3,842 decrease in loans. This pattern continued in the second and third years, with SPAD recipients receiving increasing amounts of gift aid and decreasing loan amounts. Over four years, SPAD recipients received a marginally significant $4,609 more in total aid, with cumulative gift aid increasing by $16,465 and loans decreasing by $12,470. There was no significant change in work-study earnings at any point, though point estimates grew in magnitude each year.

[17] This analysis includes only students who enrolled for at least four years to better capture the impact of SPAD receipt on financial aid for graduates. This ensures that the main outcomes of the paper—relating to graduates' major choices—are reflective of the financial treatment outlined in Table 2.

To contextualize the size of these effects, column (5) of Table 2 shows the mean cumulative aid amounts for non-SPAD recipients within the optimal bandwidth. On average, non-recipients received $83,364 in total aid over four years, including $54,225 in gift aid and $27,756 in loans. The estimated increase of $16,465 in gift aid for SPAD recipients represents a 30% increase relative to non-recipients, while the $12,470 reduction in loans reflects a decline of approximately 45%. These magnitudes highlight the substantial shift in aid composition experienced by SPAD recipients, even though total aid increased by 5.5%.

On the basis of these point estimates, the substitution of loans for gift aid is not exact, as cumulative total aid for recipients was greater each year. Nonetheless, the predominant effect remains a shift in the composition of aid, with loans decreasing and gift aid increasing. While this shift is not one-to-one, it still represents a meaningful reallocation of financial aid. The impact of SPAD receipt on academic and major choice outcomes will be interpreted within this framework, recognizing that differences in total aid may also play a limited role.

Persistence and Graduation

Table 3 presents RD estimates of the effect of SPAD receipt on student persistence and graduation outcomes.[18] Across all measures, SPAD receipt had no statistically significant effect on persistence or graduation. The point estimates are small and positive for persistence into the second and third years (both 2 pp), and near zero for persistence into the fourth year (-1 pp). Similarly, the estimates for graduating within four, five, or six years, range from 4 to 5 pp, but none are statistically significant. There is also no significant impact on eventual degree attainment or on time to degree for those who graduate.

These results suggest that substituting loans for gift aid does not meaningfully affect persistence or completion for students below the poverty line. While reducing loans could ease debt-related concerns that might otherwise lead some students to drop out, these results provide no evidence of such an effect. One potential explanation is that the total amount of aid—rather than its composition—plays a more central role in supporting persistence, since it is total aid that determines whether students can cover educational costs and remain enrolled.

In this context, SPAD's influence may be concentrated at the enrollment margin rather than on persistence or graduation. As I cannot observe the timing of aid notification relative to

[18] See Figure A.4 in the Online Appendix for RD plots of these outcomes.

students' enrollment decisions, it is possible that both recipients and non-recipients enrolled under the assumption of a similar debt burden. As a result, both groups may have entered with comparable expectations and motivations to complete their degrees—limiting the potential impact of SPAD on post-enrollment outcomes.

Moreover, persistence and graduation rates among non-recipients are high—95% persist to the second year and 82% graduate—leaving limited room for significant improvement. However, the structure of the program—which offers funding for up to five years—could influence students' time to completion, making the absence of any detectable shift in time to degree or graduation within five or six years somewhat notable. In sum, neither the shift from loans to grants nor the modest increase in total aid had a meaningful impact on persistence or degree attainment.

Diff.-in-Disc. Identifying Assumptions

The validity of the diff.-in-disc. estimator relies on two key assumptions. First, in the absence of treatment, the confounding policy should remain constant over time. Specifically, students below the poverty line who entered before fall 2006 should have responded similarly to the confounding federal programs—namely, by selecting admission majors associated with higher earnings. Figure A.5 shows the median initial earnings associated with admission majors for cohorts entering between fall 2000 and fall 2005. In contrast to the period following the introduction of SPAD, there is no evidence of a discontinuity at the poverty line in earnings associated with admission majors during this earlier window, and the estimated difference is statistically insignificant.

However, the validity of this assumption is difficult to assess due to small cohort sizes, the limited number of pre-SPAD years, and the overlap with a recession. During economic downturns, students often shift away from majors with weaker labor market prospects (Goulas and Megalokonomou, 2019; Ersoy, 2020), gravitating toward traditionally higher-earning fields such as STEM (Liu et al., 2019). Because low-income students tend to be more responsive to economic shocks (Shibata, 2021), those just below the poverty line may have already been selecting higher-earning majors in response to the early 2000s recession, leaving limited room for further upward adjustment. Conversely, students just above the poverty line may have been more likely to shift their initial major choices during this period. These dynamics could have contributed to the similar admission major earnings observed on either side of the poverty line in the pre-SPAD period. As a result, this assumption remains difficult to verify with the available data.

Second, SPAD receipt must not interact with the effects of pre-existing programs that use the poverty line as a determinant of eligibility and generate the original discontinuity. That is, the introduction of SPAD should not alter how students respond to other programs tied to poverty status. If SPAD amplifies or dampens the influence of those policies, then the diff.-in-disc. estimate may not isolate the effect of SPAD alone. While this assumption is difficult to test directly, the analysis assumes that SPAD did not fundamentally change students' behavioral response to exposure to poverty-linked programs.

Other threats to validity could arise if SPAD induces more or different types of students to enroll. Data on financial aid offered to students who were admitted to MSU but did not enroll are unavailable, so I cannot compare the effect of a SPAD offer on the decision to enroll at MSU. However, Figure A.6 shows the densities of enrolled students around the poverty line for the fall 2003 to fall 2005 (left panel) and fall 2006 to fall 2008 (right panel) cohorts. With the exception of a modest spike just below the threshold in the later period, the density patterns appear similar across periods, suggesting that the introduction of SPAD did not substantially increase enrollment among students below the poverty line.[19]

Similarly, Table A.3 compares observable characteristics of students below the poverty line who enrolled at MSU during these two periods. With the exception of high school GPA—which increased by a significant 0.10 points—none of the differences are statistically significant. This modest GPA increase does not appear to reflect a broader shift in student composition. While not all identifying assumptions are directly testable, the available evidence supports the plausibility of the identification strategy.

Major Outcomes

To assess whether SPAD influenced students' academic trajectories, Table 4 presents results on a set of major-related outcomes. The first two rows use the same RD design applied in the financial aid, persistence, and graduation analyses, and examine whether students are more likely to ever change majors or end in a major associated with lower initial earnings relative to their

[19] The three years before and after the implementation of SPAD were chosen because trends in the enrollment of low-income students have shifted over the past few decades. More recent cohorts have a larger proportion of students below the poverty line, so including them in this figure could inaccurately suggest that SPAD induced greater enrollment of low-income students, which may not be the case.

admission major. The final two rows use the diff.-in-disc. framework to evaluate changes in major characteristics—specifically STEM classification and academic match—from entry to graduation. Column (1) includes all students and compares each student's entry major to the major declared in their final enrolled term, regardless of graduation status. Column (2) restricts the sample to graduates, capturing the major listed on their degree. This breakdown allows for the possibility that SPAD may influence major decisions differently for students who complete a degree versus those who do not, even if overall graduation rates remain unchanged.

Estimates in the top row suggest that SPAD had no significant effect on the likelihood of changing majors. In both columns, the point estimates are small and not statistically distinguishable from zero. Similarly, for students who switch majors, there is no evidence that SPAD recipients are more likely to move to majors associated with lower initial earnings relative to their admission major. Plots of these outcomes are provided in Figure A.7 of the Online Appendix.

In contrast to the RD estimates, the diff.-in-disc. results suggest that SPAD influenced how students' major choices evolved over time. For STEM,[20] the discontinuity at the poverty line shrinks by 13 pp when comparing students' admission majors to their final enrolled majors. For context, SPAD students entered with higher rates of STEM majors (20.3% vs. 19.1%) but were less likely to finish in a STEM field (18.4% vs. 20.1%). While this raw difference in means is smaller than the estimate in the table, it does not isolate the causal effect of SPAD as the diff.-in-disc. estimator does. A similar, though smaller, reduction of 9 pp is observed among graduates, with a similar trend occurring from admissions (20.3% vs. 18.8%) to graduation (18.9% vs. 19.8%). The reversal in the STEM gap suggests that SPAD influenced how students sorted into or out of STEM majors over time, potentially by easing financial pressures that may have previously pushed some students toward higher-earning fields.

To examine shifts in academic match, I construct a continuous measure of degree mismatch following Maragkou (2020). This measure compares each student's academic preparation, proxied by their highest ACT score, to the typical preparation of students in their major. Specifically, I subtract the student's ACT score from the median ACT score of the five prior cohorts within

[20]STEM majors are those that fall under the four primary 2-digit CIP codes of engineering (14), biological and biomedical sciences (26), mathematics and statistics (27), and physical sciences (40). This excludes fields such as computer science, health sciences, and social sciences, which are sometimes included in broader STEM definitions.

the same four-digit CIP code, calculated separately for their admission and graduation majors. A negative value indicates that the student is "undermatched" (i.e., their ACT exceeds the median score of the previous five cohorts in their major), while a positive value reflects "overmatch."

For example, a student in the fall 2018 cohort with an ACT score of 26 would be undermatched if they entered with an Education major (median ACT of 24 across the fall 2013–2017 cohorts; mismatch = -2) and overmatched if they entered with an Applied Mathematics major (median ACT score of 28; mismatch = 2). If a student submitted only SAT scores, I convert them to ACT equivalents using concordance tables from ACT (2018); if both are available, the higher score is used. The units are ACT composite points, which range from 1 to 36.

SPAD appears to influence the alignment between students' academic preparation and the difficulty of their chosen major. The estimates show a statistically significant reduction in degree mismatch—1.59 and 1.44 point decreases for all students and graduates, respectively—indicating that SPAD recipients are less likely to finish in majors where they are overmatched. On average, students around the poverty line enter MSU in majors where they are slightly overmatched—by about one ACT point—so a negative coefficient suggests that SPAD recipients move into majors that are better aligned with their academic preparation. This result is consistent with the observed movement away from STEM fields, which often are associated with higher academic abilities. This movement should not be interpreted as a shift to "easier" majors, but rather as a reallocation toward fields that may offer a better academic or personal fit once financial constraints are reduced.

Together, these results suggest that while SPAD does not increase overall major switching or prompt students to move to initially lower-earning majors, it does influence students' final major choices. These effects are consistent with the idea that reducing debt burdens can shift students' academic decisions away from strictly financially-motivated choices and toward better personal or academic fit. They also do not necessarily contradict the null effects on transitions to lower-earning majors, as the earnings measure used reflects only the first year in the labor market and may not capture longer-term differences in earnings trajectories.

Heterogeneity

To investigate how students from various backgrounds respond to changes in aid composition, Tables A.4 through A.6 present results of separate analyses by gender, race/ethnicity group, and first-generation status. Due to small subgroup sizes for many racial/ethnic categories, I only present results for White/Asian and non-White/non-Asian students.

Table A.4 reports how SPAD altered the composition of cumulative financial aid over four years for these different student subgroups. Across nearly all subgroups, the program resulted in a large increase in gift aid, paired with a roughly equal and offsetting reduction in student loans. As in the full analysis, most changes in total aid are modest, with some subgroups experiencing no statistically significant change

Among male students, SPAD increased cumulative gift aid by $13,537 and reduced loans by $11,375, with no statistically significant change in total aid, though work-study earnings increased by $1,302. For female students, total aid increased by a marginally significant $5,720, suggesting a net increase in support in addition to substantial loan substitution. Across racial/ethnic groups, SPAD led to a substantial reduction in loans for White and Asian and non-White/non-Asian students—over $13,000 and $10,000, respectively—but the former experienced a larger increase in gift aid relative to their decrease in loans. As a result, they received more than $7,000 in total cumulative aid due to SPAD, while non-White/non-Asian students did not experience an increase in total aid. Patterns also differed by first-generation status: first-generation students exhibited a particularly clean one-to-one substitution of loans for gift aid, receiving approximately $14,000 more in gift aid and a similarly-sized reduction in loans. In contrast, non-first-generation students received over $10,000 more in total aid and $1,104 more from work-study earnings.

These results suggest that only non-White/non-Asian students and first-generation students experienced changes that are best characterized as a pure substitution of loans for gift aid. For the other subgroups, the effects of SPAD reflect a mix of aid composition changes, increased total support, and in some cases, additional work-study earnings. As a result, later academic and major outcomes for these groups should be interpreted as responses to multiple dimensions of financial aid change, not solely to shifts in aid composition.

Table A.5 reports RD estimates of the effect of SPAD receipt on student persistence and graduation outcomes across these subgroups. With the exception of a marginally significant 4 pp increase in second-year persistence for White and Asian students, there are no statistically significant differences in persistence into the second, third, or fourth year of enrollment. The estimates are generally small in magnitude and tightly clustered around zero, suggesting that substituting loans for gift aid does not meaningfully affect persistence.

Similarly, SPAD receipt has no detectable effect on any graduation outcome. While estimates are consistently positive across all subgroups, the impacts on graduating within four, five, or six years are statistically indistinguishable from zero. The same holds for the likelihood of ever earning a degree. Among those who graduate, there is no significant impact on time to degree. These findings closely mirror the overall results presented in Table 3.

Table A.6 presents estimates on major-related outcomes. SPAD receipt had no significant impact on the likelihood of ever changing majors for any subgroup. While most estimates for switching to a lower-earning major are not statistically significant, there is suggestive evidence that non-White/non-Asian students were more likely to switch into majors associated with lower earnings (12 pp; $p<0.10$) following SPAD receipt.

Across most subgroups, SPAD receipt is associated with a shift away from STEM fields over time. While not all groups exhibit a full reversal of the initial STEM discontinuity, the general pattern is consistent: SPAD recipients entered college with higher rates of intended STEM majors relative to their peers just above the poverty line, but by their final semester were less likely to be in a STEM field. In contrast, among non-recipients, the proportion earning a STEM degree either remained stable or increased. These patterns suggest that reduced loan burdens may have allowed SPAD recipients to reevaluate earlier decisions and pursue fields more aligned with their interests rather than earnings potential. Estimated effects range from an insignificant 8 pp decline among male students to a significant 17 pp decline among non-first-generation students ($p<0.05$).

SPAD receipt is also associated with a reduction in overmatch for most subgroups. Except for male and non-first-generation students, all subgroups finished in less overmatched majors, indicating a shift toward fields more closely aligned with their academic backgrounds. This pattern is most pronounced among female, White and Asian, and first-generation students, with estimated declines in overmatch of 1.98 to 2.08 ACT points.

Taken together, these results suggest that SPAD had the largest overall impacts on non-first-generation, White and Asian, and female students—the groups that experienced not only loan substitution but also increases in total aid. For these students, later changes in major field and degree mismatch likely reflect responses to both the shift in aid composition and the increased financial support provided by SPAD. In contrast, first-generation and non-White/non-Asian students—for whom SPAD primarily substituted loans for gift aid—still experienced modest and meaningful changes, reinforcing the central role of aid composition in shaping student decisions. The stronger responses among groups with increases in total aid suggest that financial support beyond loan substitution may amplify these effects. Notably, SPAD receipt did not produce any significant changes in academic or major outcomes for male students.

Robustness Checks

Robustness checks for the overall estimates are presented in Tables A.7 through A.10. These tables evaluate the sensitivity of the main results to different bandwidths and estimation strategies. Specifically, columns (1) and (2) replicate the baseline specifications using alternative bandwidths of $6,000 and $18,000 to assess whether results are stable across narrower and broader groups of students. Column (3) reports results from a DiD-based approach that compares outcomes for students just above and below the poverty line before and after the introduction of SPAD, offering estimates from an alternative identification strategy. Finally, columns (4) and (5) implement placebo tests using thresholds $15,000 below and above the true SPAD cutoff, providing a check that effects do not appear where there is no treatment.

The DiD approach provides estimates of the impact of SPAD eligibility using the following equation:

$$Y_{it} = \beta_0 + \beta_1 Post2006_i + \beta_2 Below_{i,t=1} + \beta_3 (Post2006_i \times Below_{i,t=1}) + \gamma X_i + College_{i,t=1} + \epsilon_{it} \quad (6)$$

where $Post2006_i$ is an indicator equal to 1 if student i entered MSU in fall of 2006 or later,[21] $Below_{i,t=1}$ is an indicator of SPAD eligibility equal to 1 if student i's AGI in their first year was below the federal poverty line, and β_3 captures the estimated effect of SPAD eligibility. This

[21] Students were not grandfathered into the program, meaning a student who began at MSU in fall 2005 and met all SPAD eligibility requirement would not have received SPAD in subsequent years.

analysis includes students who entered MSU in fall 2000 or later and whose AGI was within \$12,000 of the poverty line—similar to the optimal bandwidth used in the main RD analysis—providing intent-to-treat (ITT) estimates on a similar group of students for ease of comparison with the RD results. See Appendix A for additional details on this approach, including event studies to evaluate the parallel trends assumption.

Columns (1) and (2) of Table A.7 demonstrate that the main pattern of changes in aid composition holds across alternative bandwidths: SPAD continues to generate large increases in gift aid (\$16,000 to \$19,000) alongside substantial reductions in loans (over \$11,000), with a modest net increase in total aid. Notably, the total aid effect becomes more precisely estimated as the bandwidth increases—from marginal significance for the \$6,000 bandwidth to highly significant for the \$18,000 bandwidth. This is consistent with expectations, as students farther above the poverty line have less financial need and receive less financial aid, leading to a clearer contrast with SPAD recipients.

Similarly, the DiD estimates in column (3) reveal the same broad pattern observed in the RD results. While the compositional change is smaller in magnitude, this is to be expected—the DiD approach reflects the effect of SPAD eligibility, not receipt. The consistency of the direction and composition of the effects across methods reinforces the robustness of the main findings. Figure A.8 contains event study plots illustrating these patterns.

The placebo thresholds in columns (4) and (5) show no evidence of an aid composition shift. The large and precisely estimated changes in gift aid and loans observed at the true threshold do not appear at these artificial cutoffs. While column (4) shows a marginally significant increase in gift aid and column (5) shows a large increase in work-study earnings, these patterns are inconsistent and not accompanied by reductions in loan aid, suggesting they do not reflect the kind of substitution observed in the main analysis.

Table A.8 reports robustness checks for persistence and graduation outcomes. Consistent with the main results, these specifications show no meaningful or statistically significant effects of SPAD receipt on persistence, graduation rates, or time to degree. The estimates in columns (1)-(3) are small in magnitude and relatively stable across bandwidths and identification strategies. See Figure A.9 for event studies for these outcomes. The placebo thresholds in the remaining columns yield no significant effects.

Finally, Tables A.9 and A.10 present robustness checks for major-related outcomes for all students and for graduates, respectively. While there is still no significant impact on the likelihood of ever changing majors, there are significant shifts in the earnings profile of those who do. Unlike the results for the optimal bandwidth, the alternative bandwidths in columns (1) and (2) of Table A.9 suggest that SPAD students are significantly more likely to move into lower-earning majors (24 and 10 pp, respectively). The estimates for graduates shown in Table A.10 are similar in sign and magnitude, though not significant. However, the DiD specification in column (3) shows no impact on either of these outcomes. The event study for movement to majors associated with lower earnings can be found in Figure A.10.

The final rows report robustness checks for outcomes related to STEM majors and degree mismatch. Columns (1), (2), (4), and (5) are estimated using the diff.-in-disc. framework, while estimates in column (3) come from an extension of the DiD approach to a triple-differences (DDD) design. This extension is conceptually analogous to the move from the RD to the diff.-in-disc., comparing changes in major outcomes from admission to graduation.[22]

The results show that the impacts on both STEM and degree mismatch outcomes largely disappear in the $6,000 bandwidth and in the DDD specification, though the estimates in the $6,000 bandwidth are imprecise due to a limited sample size. However, under the $18,000 bandwidth, the estimates remain similar to the main results—indicating a shift away from STEM fields and a reduction in mismatch among SPAD recipients. Figure A.11 contains event studies for these outcomes. As with prior outcomes, the placebo thresholds in columns (4) and (5) show no consistent effects, providing additional reassurance that the main findings are not driven by discontinuities at arbitrary cutoffs.

While the estimates from the DDD specification are not significant, this reflects a lack of impact from eligibility, not actual receipt. Meanwhile, the sensitivity of the major estimates to bandwidth suggests that impacts may not be concentrated narrowly around the poverty line ($6,000 bandwidth). Instead, impacts appear more clearly when using broader bandwidths (optimal and $18,000), which include students further below the poverty line. These students are more financially constrained and thus may be more responsive to changes in aid composition, particularly when it reduces expected debt. As a result, the larger bandwidth estimates may be

[22] See Appendix A for additional details on the DDD specification.

driven by the stronger responses of these students, while the narrower specification may lack both power and coverage of the most affected students.

DISCUSSION

Research on financial aid has primarily examined the effects of increasing total aid, whether through gift aid or loans. While such increases generally improve college access and degree attainment, there are limits to how much aid can be expanded. As a result, policymakers may increasingly turn to aid composition as a lever for influencing student outcomes. This paper provides an initial investigation of how such compositional shifts affect persistence, graduation, and major choice.

A large body of research finds that receiving additional gift aid increases persistence and graduation rates. For example, studies of federal and state programs—including the Pell Grant (Bettinger, 2004), state merit aid (Dynarski, 2005), private need-based aid (Goldrick-Rab et al., 2012; Goldrick-Rab et al., 2016), and need-based institutional grants (Castleman and Long, 2016)— consistently show that increases in gift aid lead to higher rates of persistence and degree completion. These effects are often strongest among students with high financial need, suggesting that liquidity constraints and debt aversion meaningfully influence these outcomes.

This study contributes to that literature by examining a different margin of aid policy: an increase in gift aid that is offset by a reduction in loans. Despite the well-documented positive effects of gift aid relative to no aid, I find no evidence that this compositional change affects persistence or graduation. This suggests that the amount of aid may play a more central role than its form, at least in terms of promoting degree completion. Further, this underscores the limits of composition-based reforms when students are already receiving the maximum amount of support.

However, the shift away from loans appears to reduce financial pressures, giving students greater flexibility in their academic decisions. Between admission and graduation, SPAD recipients were less likely than non-recipients to remain in STEM majors and more likely to switch into fields better aligned with their academic preparation. These results mirror the findings of Rothstein and Rouse (2011), who showed that increased loan burdens lead students to prioritize higher-paying majors and careers. In contrast, I find that reducing loans appears to relax this prioritization of earnings, allowing students to pursue fields that better align with their academic fit or interests.

While this greater freedom may improve students' personal or academic fit, it also represents a shift away from majors with stronger labor market returns. STEM majors, in particular, are often associated with higher starting salaries and more stable employment prospects (Altonji et al., 2012; Webber, 2014; Deming and Noray, 2018). These features may be especially attractive to debt-averse students anticipating large loan burdens. By lowering reliance on debt, SPAD may ease these financial constraints, but also reduce incentives to pursue high-earning fields.

This movement away from STEM could have longer-term implications for students' economic mobility. While I find no significant effect on initial earnings associated with students' graduating majors, I do not examine effects on longer-term earnings or observe outcomes related to graduate school attendance, career field, or job satisfaction. These dimensions may also be influenced by major choice and could provide important context for interpreting the observed movement away from STEM. If SPAD enables students to select majors that better align with their interests or academic strengths, improvements in these non-monetary outcomes may outweigh any foregone earnings. Thus, while the results raise concerns about potential economic trade-offs, they may also reflect gains in personal or academic fit that are not captured by short-term labor market measures.

Policy Implications

These observed shifts suggest that aid composition can influence how students navigate academic decisions. Replacing loans with gift aid provides students with greater flexibility to choose majors that better reflect their strengths or interests, rather than those primarily associated with high initial earnings. While I do not find any effect on persistence or completion, the potential for aid design to shape academic trajectories highlights the relevance of grant generosity as a policy lever.

These findings also point to the potential value of non-financial support to help students make informed academic and career decisions. If students respond to aid structure by adjusting their major or career plans, these shifts may reflect not only financial pressure but also uncertainty about long-term outcomes. In particular, movement away from high-return fields like STEM suggests that complementing financial aid with clear, accessible guidance could help students weigh their academic interests against potential labor market consequences and avoid unintended outcomes. This support could include one-on-one advising sessions with academic or financial aid counselors focused on aligning students' interests, preparation, and long-term goals. These sessions may be especially valuable during key decision points—such as after receiving aid packages, during first-year advising, or when students consider changing majors. In addition, providing students with clear and timely information—whether through interactive dashboards, decision support-tools, or other channels—on loan burdens, major-specific earnings, and degree requirements could help them better evaluate trade-offs and navigate both academic decisions and the complexities of the financial aid process.

Future Research

While this study focuses on outcomes for students who enroll, changes in aid composition may also affect other key decision points. At the enrollment margin, students who are particularly debt-averse may be more likely to attend college if they can rely on grants rather than loans. Aid composition may also influence where students choose to enroll, particularly when comparing institutions that offer a similar amount of total aid but differ in the generosity of their grants.

After graduation, lower debt burdens could influence decisions to pursue advanced degrees, take financial risks, or enter lower-paying careers that offer greater long-term fit. This raises questions about how aid composition affects educational attainment, financial behavior, and well-being in the years following college—including longer-term earnings and mobility outcomes. More research is also needed to better understand the behavioral mechanisms underlying observed responses and whether changes in behavior are driven by debt aversion, perceived risk, or shifting financial or informational constraints.

The results in this paper are unlikely to generalize to the large number of students further above the poverty line. Given the limited effects even among the most financially constrained students, impacts may be smaller—or operate through different mechanisms—among middle- and higher-income students who face different financial trade-offs. Effects may also differ across institutions, even for students with similar income levels. For instance, outcomes may not translate directly to students at community colleges, more selective universities, or institutions serving particularly debt-averse populations. Future work should explore whether similar patterns emerge across a broader range of income levels, academic preparation, and institutional settings.

These findings underscore the importance of considering not just how much aid students receive, but also the form it takes. As institutions and policymakers explore ways to improve student outcomes, the structure of aid packages may be a key lever for shaping academic and career trajectories.

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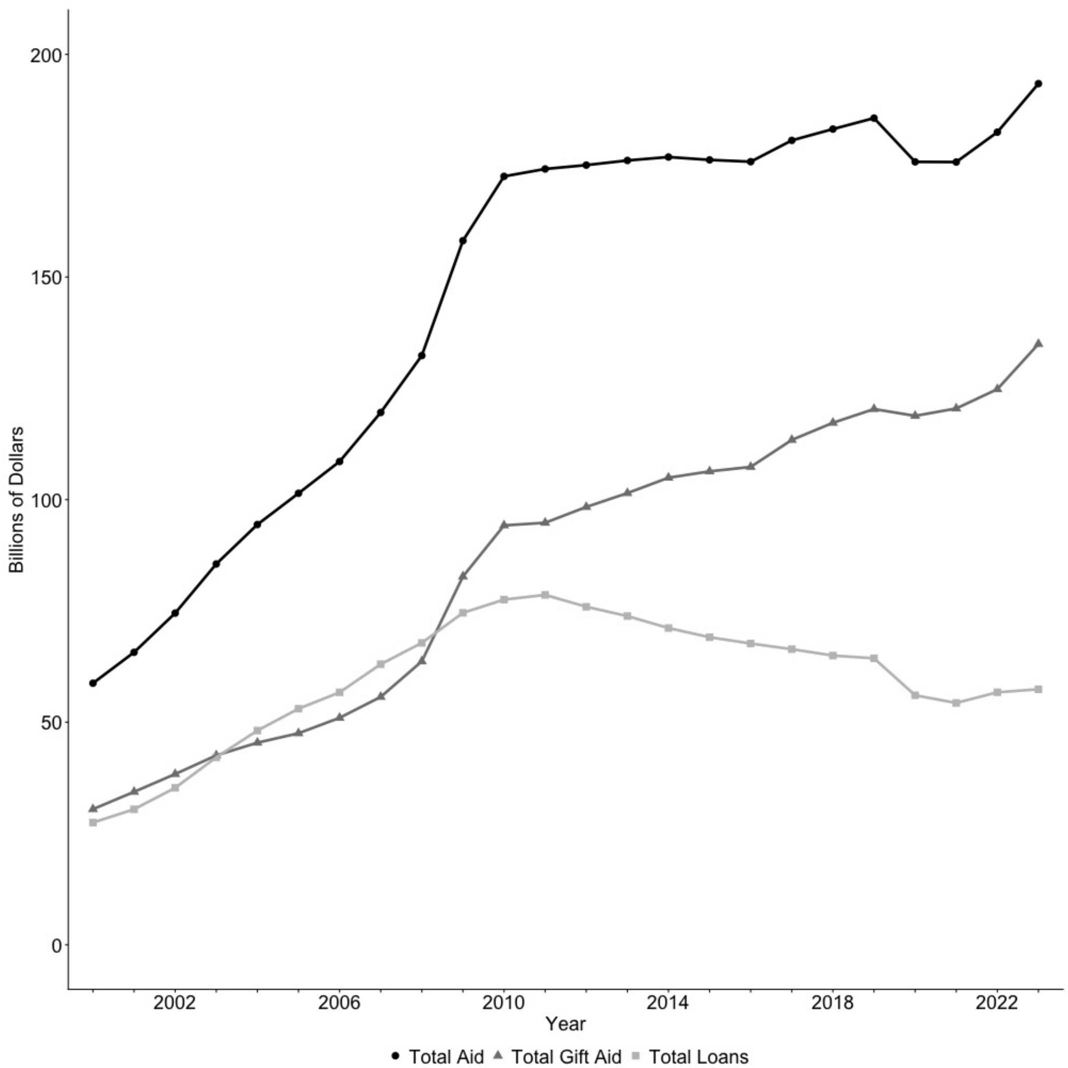
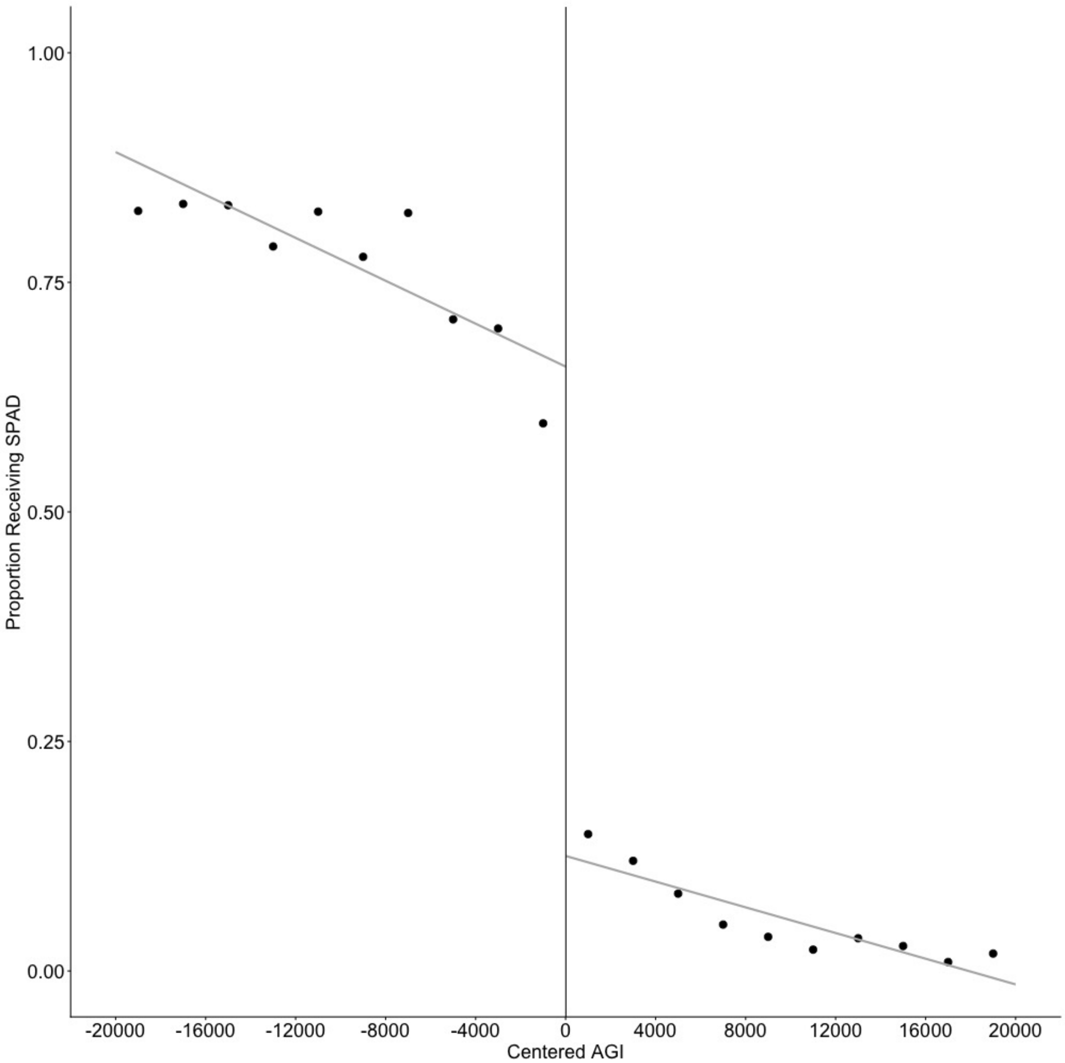
TABLES AND FIGURES



Notes: Source: College Board Trends in Student Aid 2024. Values reflect all federal, state, and institutional gift aid and federal and non-federal loans.

Figure 1. Trends in Financial Aid (2000 to 2023).



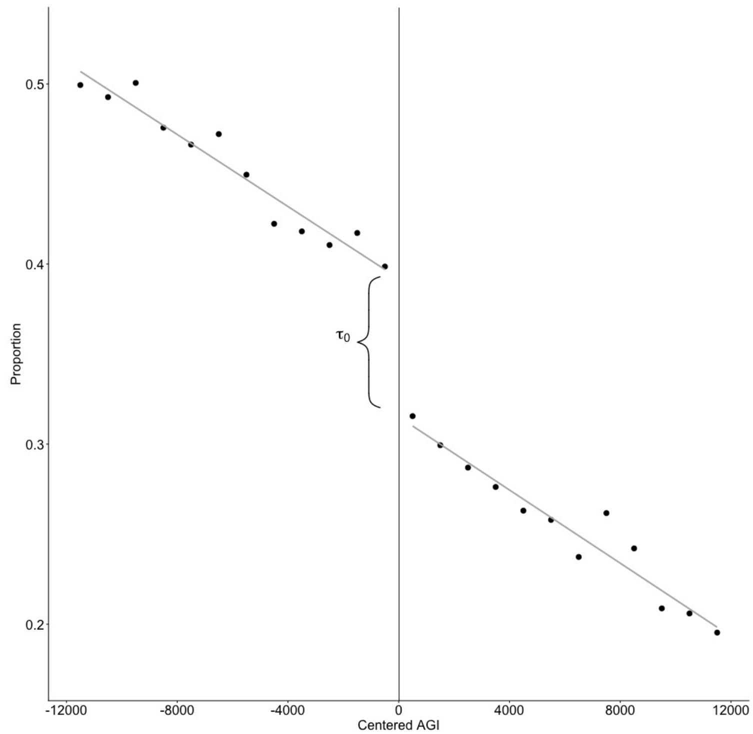
Notes: This figure plots the proportion of students in each bin of width $2,000 who receive SPAD, with the vertical line indicating the SPAD eligibility threshold. Centered AGI is calculated by subtracting the year- and household-size-specific federal poverty level from a student's total AGI. Negative values denote SPAD eligibility.

Figure 2. SPAD Receipt by Centered Adjusted Gross Income.

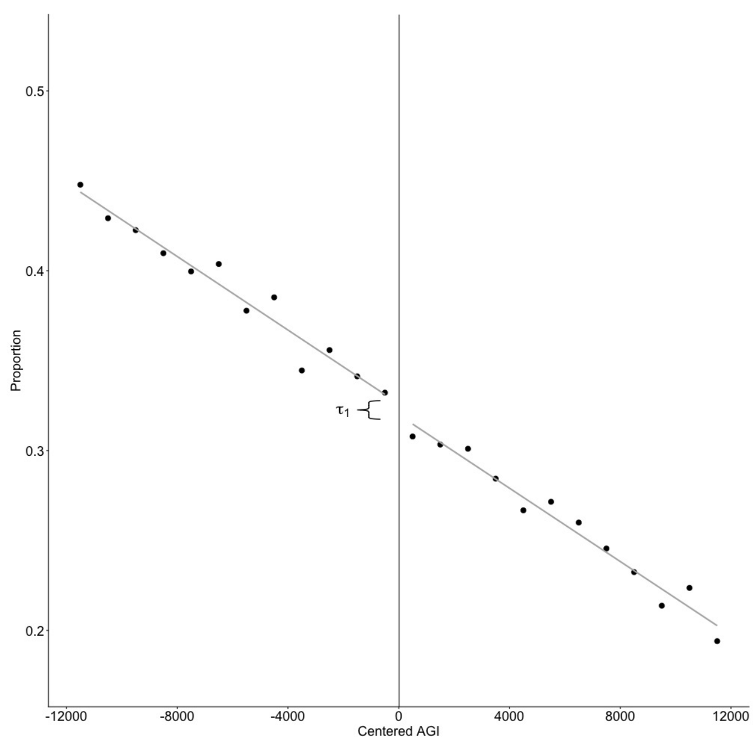
Variable	**(1) SPAD**	**(2) All MSU In-State Students**	**(3) Non-SPAD Aid Recipients**
White	0.42	0.76***	0.79***
Black	0.33	0.09***	0.07***
Asian	0.08	0.06***	0.06*
Hispanic	0.12	0.04***	0.04***
Female	0.62	0.55***	0.56***
First-Generation	0.56	0.21***	0.20***
HS GPA	3.52	3.61***	3.69***
EFC	$380	-	$29,101***
AGI	$12,606	-	$125,318***
Students	6,760	90,336	60,764

Notes: Column (1) contains all students who received SPAD during their first year between fall 2006 and fall 2020. Column (2) includes all Michigan residents who entered MSU for the first time during this period, while column (3) is the subset that received financial aid during their first year but did not receive SPAD. EFC (Expected Family Income) and AGI (Adjusted Gross Income) are only reported for financial aid recipients. AGI is the sum of parental and student AGI. Asteriks denote statistically significant differences from SPAD recipients based on two-sided t-tests. * p<0.10, ** p<0.05, *** p<0.01.

Table 1. Comparison of SPAD Students to Other MSU Student Populations.

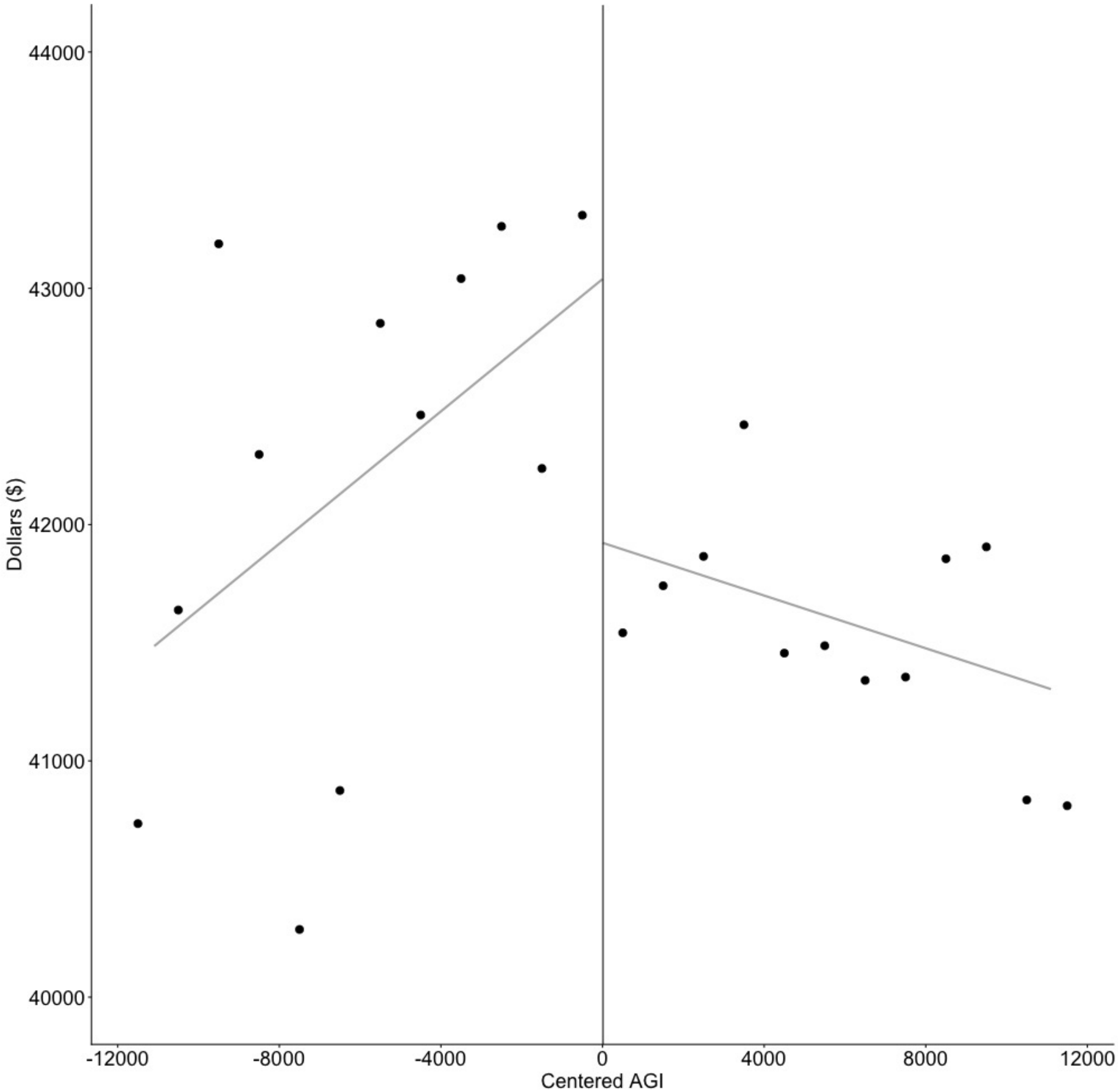


(a) Pre-Existing Discontinuity.



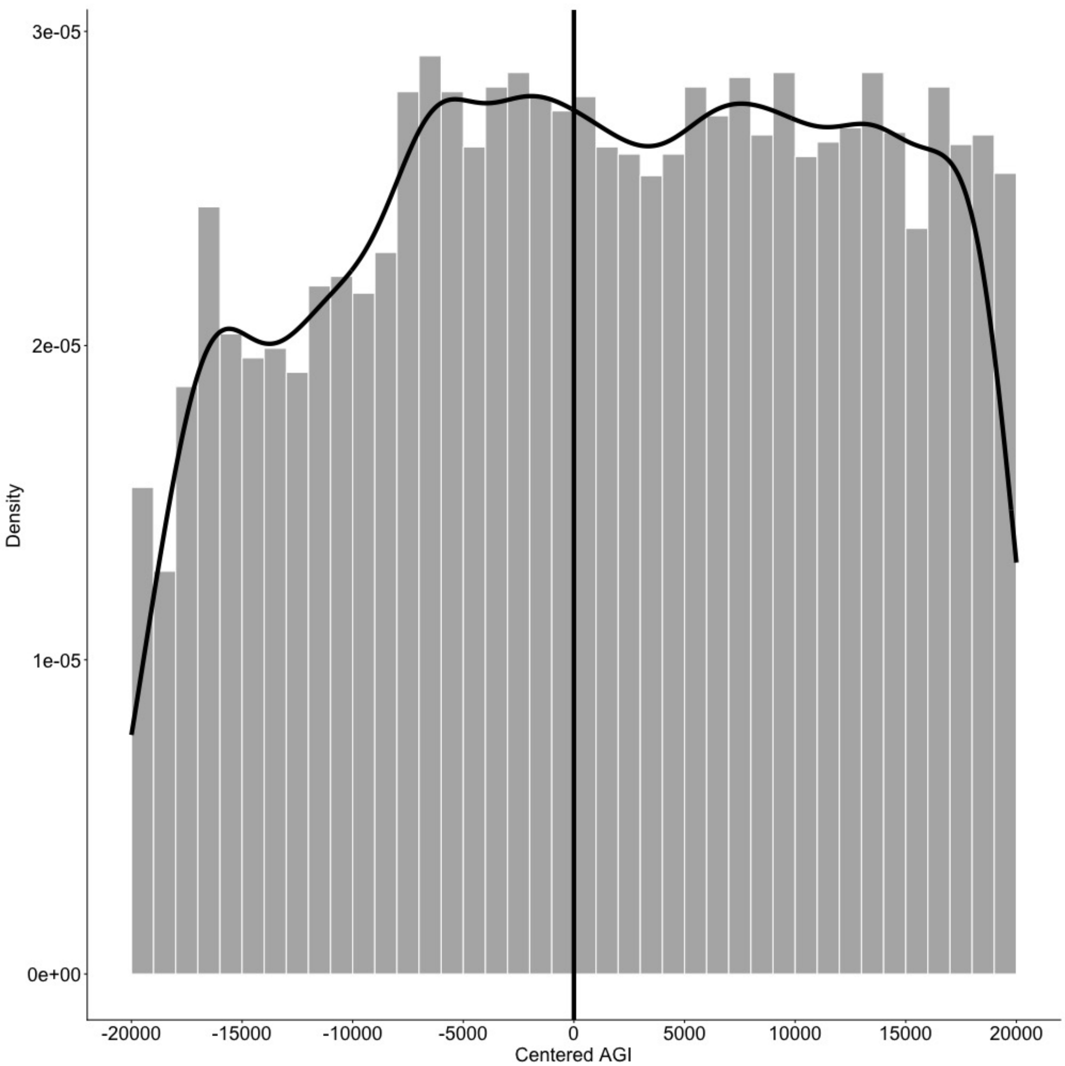
(b) Discontinuity After Treatment.

Figure 3. Difference-in-Discontinuities Visual.



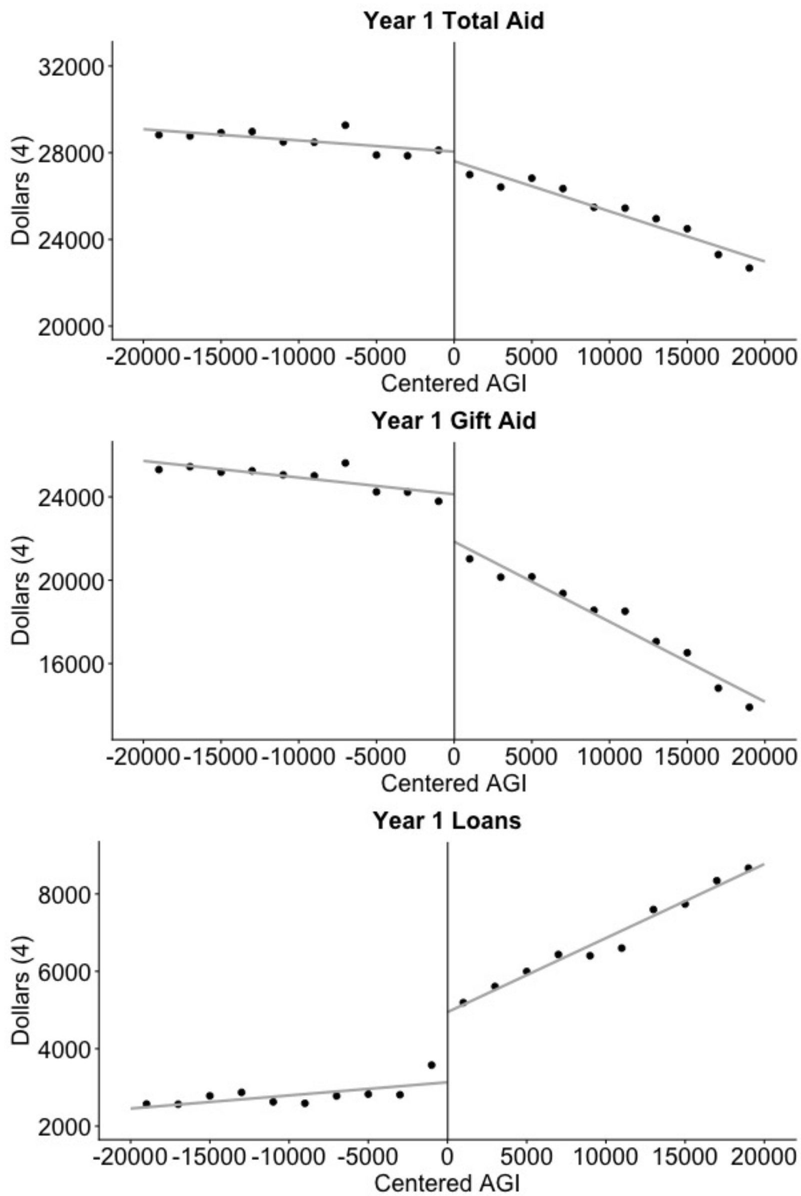
Notes: For students in the optimal bandwidth, this figure plots the median first-year earnings in 2022 real dollars (adjusted using the Bureau of Labor Statistics' Consumer Price Index for All Urban Consumers—Series ID: CUUR0000SEEB01) associated with students' admission majors against centered AGI, using bins of width $1,000. Because students select these majors prior to receiving their financial aid package, the figure captures pre-treatment differences in intended field of study. While the data are somewhat noisy, there is suggestive evidence of a discontinuity at the poverty line: students just below the threshold appear more likely to enter with majors associated with higher initial earnings. This pattern motivates the diff.-in-disc. approach by indicating potential baseline differences in major selection across the eligibility threshold.

Figure 4. Earnings Associated with Admissions Major.



Notes: This figure plots the density of students in each bin of width \$1,000, with the vertical line indicating the SPAD eligibility threshold. Centered AGI is calculated by subtracting the year- and household-size-specific federal poverty level from a student's total AGI. Negative values denote SPAD eligibility. The absence of a visible spike at the threshold suggests no evidence of manipulation in AGI reporting.

Figure 5. Distribution of Centered AGI.



Notes: This figure illustrates the discontinuity in first-year financial aid components at the federal poverty line, which serves as the eligibility threshold for the SPAD program. Each panel plots binned averages using a bin width of $2,000. The left graph shows that total aid is relatively smooth across the threshold. In contrast, the middle graph displays a clear upward jump of approximately $2,500 in gift aid for eligible students, while the right graph shows a similar decrease in loans. These patterns highlight the central feature of the SPAD program: a compositional shift in financial aid from loans to gift aid.

Figure 6. First Year Aid Components.

Aid Component	**(1) 1 Year**	**(2) 2 Years**	**(3) 3 Years**	**(4) 4 Years**	**(5) Non-Recipient Mean**
Total Aid	1,338**	2,798**	3,939**	4,609*	83,364
	(584)	(1,232)	(1,912)	(2,473)	
Gift Aid	5,230***	9,986***	13,619***	16,465***	54,225
	(505)	(1,007)	(1,533)	(2,000)	
Loans	-3,842***	-7,376***	-10,028***	-12,470***	27,756
	(409)	(872)	(1,366)	(1,805)	
Work-Study	-49	189	348	614	1,383
	(119)	(227)	(319)	(395)	
N	7,465	7,465	7,465	7,465	5,257

Notes: Each column reports estimates of SPAD receipt on the cumulative amount of aid received up to that point in a student's enrollment using the specification in Equation (3). Column (5) reports the mean cumulative aid through four year for non-SPAD recipients within the optimal bandwidth. All financial aid amounts are expressed in real 2023 dollars, adjusted using the Bureau of Labor Statistics' college tuition and fees price index (Series ID: CUUR0000SEEB01). * p<0.10, ** p<0.05, *** p<0.01.

Table 2. RD Estimates on Financial Aid.

Outcome	**(1)**	**(2) Non-Recipient Mean**
Persist, Year 2	0.02	0.95
	(0.02)	(0.21)
Persist, Year 3	0.02	0.91
	(0.03)	(0.29)
Persist, Year 4	-0.01	0.85
	(0.03)	(0.36)
Graduate within 4 Years	0.05	0.41
	(0.04)	(0.49)
N	8,727	5,257
Graduate within 5 Years	0.04	0.70
	(0.04)	(0.46)
N	8,063	4,861
Graduate within 6 Years	0.05	0.78
	(0.04)	(0.41)
Ever Graduate	0.05	0.82
	(0.04)	(0.39)
N	7,377	4,489
Time to Graduate (Months)	-0.64	48.71
	(1.14)	(10.93)
N	6,600	4,193

Notes: Each row reports RD estimates of the impact of SPAD receipt on the given persistence or graduation outcome using the specification in Equation (3). The first four rows include all students. "Graduate within 5 years" is restricted to students who entered in fall 2019 or earlier; "Graduate within 6 Years" and "Ever Graduate" are restricted to those who entered in fall 2018 or earlier. These restrictions ensure students have sufficient time to reach the relevant outcome by spring 2024. "Time to degree" is calculated only among students who eventually graduate, based on the number of months between initial enrollment (assumed to begin in September of their entry year as all are fall entrants) and degree conferment. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

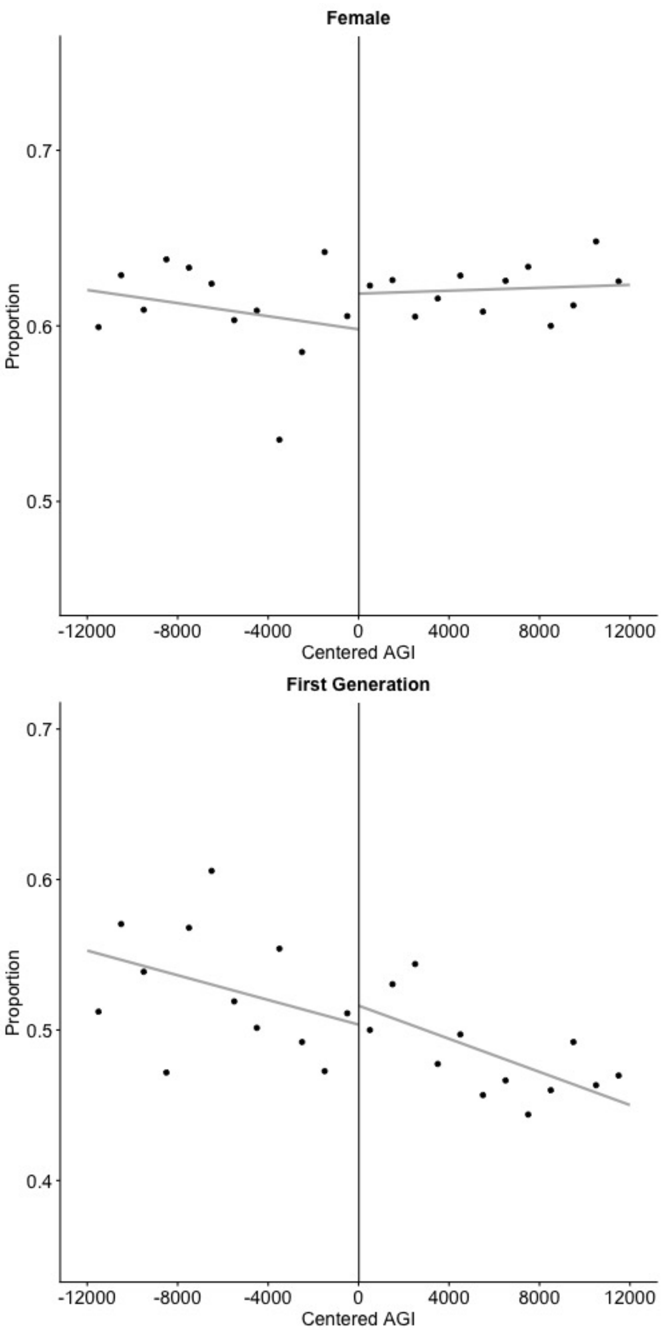
Table 3. RD Estimates on Persistence and Graduation.

Outcome	**All Students (1)**	**Graduates (2)**
Panel A: RD Estimates		
Ever Change Majors	0.03	0.02
	(0.040)	(0.042)
N	8,727	6,880
Change to Lower-Earning Major	0.02	-0.00
	(0.053)	(0.057)
N	5,671	4,976
Panel B: Diff.-in-Disc. Estimates		
STEM	-0.13***	-0.09**
	(0.039)	(0.046)
N	8,727	6,880
Degree Mismatch	-1.59***	-1.44**
	(0.544)	(0.630)
N	8,548	6,718

Notes: Panel A reports RD estimates for whether students ever changed majors or switched to lower-earning majors using the specification in Equation (3). The second row only includes students who changed majors. Panel B reports diff.-in-disc. estimates comparing admissions and final majors in terms of STEM classification and degree mismatch using the specification in Equation (4). STEM includes engineering, biological sciences, mathematics/statistics, and physical sciences. Degree mismatch is measured in ACT points as the difference between a student's ACT score and the median ACT score for the previous five cohorts within their major. The mismatch analysis excludes students without reported ACT or SAT scores.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$.

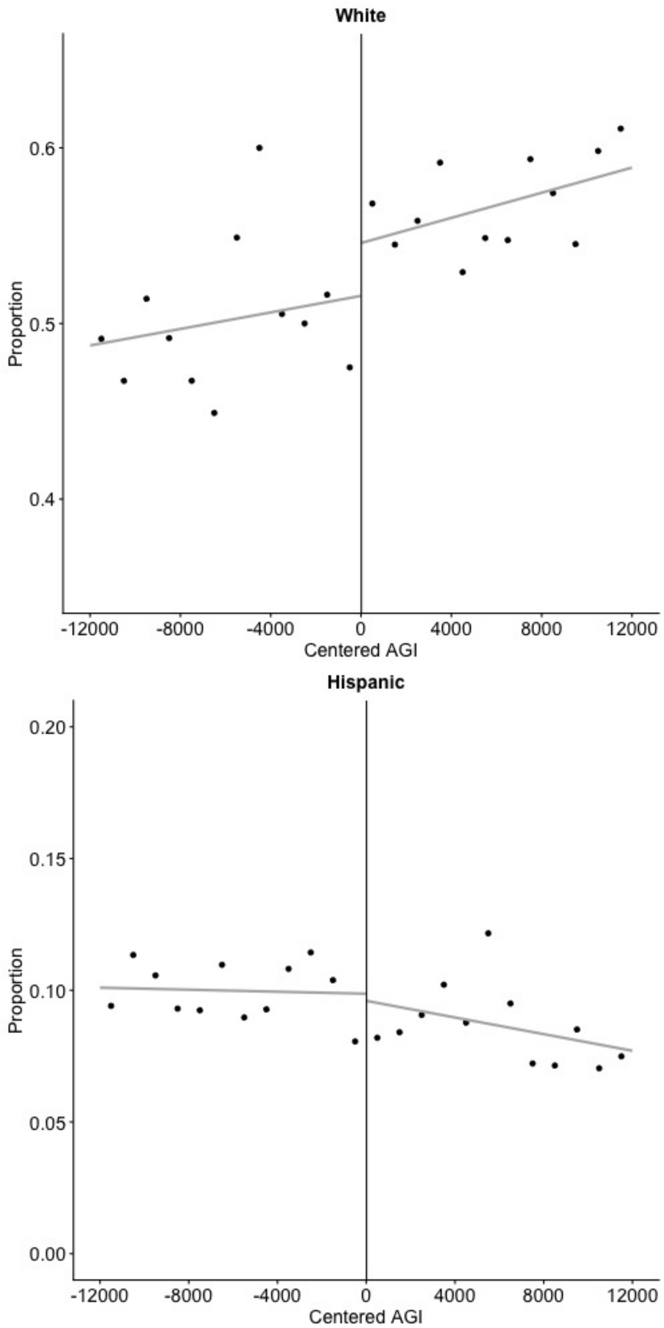
Table 4. Estimates on Major Outcomes.

ONLINE APPENDIX



Notes: For students in the optimal bandwidth, these figures plot the proportion of students in each bin of width \$1,000 with the given characteristic, with the vertical line indicating the SPAD eligibility threshold. Centered AGI is calculated by subtracting the year- and household-size-specific federal poverty level from a student's total AGI. Negative values denote SPAD eligibility. The smooth distribution of characteristics across the threshold provides visual evidence that students just above and below the cutoff are similar in their observable traits.

Figure A.1. Covariate Balance Plots.



Notes: For students in the optimal bandwidth, these figures plot the proportion of students in each bin of width $1,000 with the given characteristic, with the vertical line indicating the SPAD eligibility threshold. Centered AGI is calculated by subtracting the year- and household-size-specific federal poverty level from a student's total AGI. Negative values denote SPAD eligibility. The smooth distribution of characteristics across the threshold provides visual evidence that students just above and below the cutoff are similar in their observable traits.

Figure A.2. Covariate Balance Plots.

Variable	Estimate
White	-0.03
	(0.021)
Black	0.02
	(0.018)
Asian	0.01
	(0.011)
Hispanic	0.00
	(0.012)
Female	-0.03
	(0.021)
First-Generation	-0.01
	(0.021)
HS GPA	0.01
	(0.025)
Household Size	-0.08
	(0.058)
N	8,727

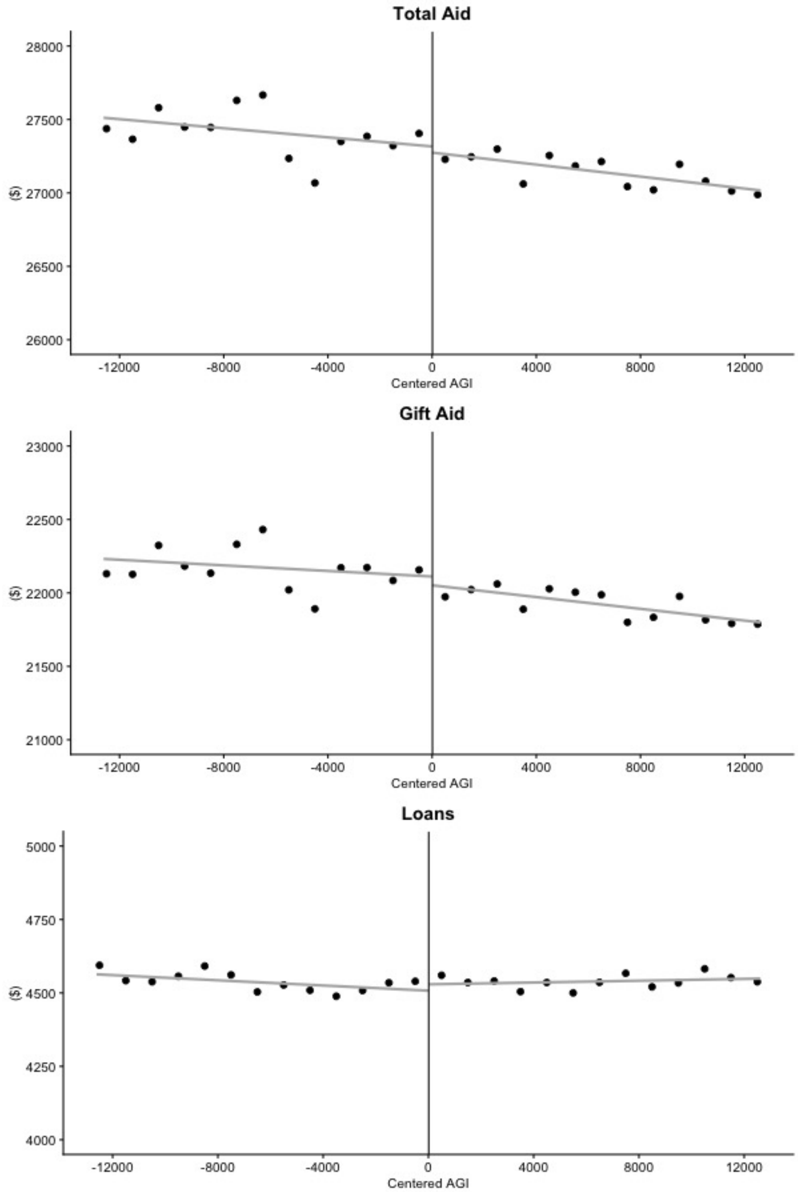
Notes: Estimates are from the covariate balance check specified in Equation 5, showing the change in each characteristic for students below the poverty line. Only students within the optimal bandwidth are included. The lack of statistically significant differences suggests that students just above and below the threshold are similar in their observable characteristics, supporting the validity of the RD design.
p<0.10, ** p<0.05, *** p<0.01.

Table A.1. Covariate Balance Check.

Variable	**Total Aid**	**Gift Aid**	**Loans**
Intercept	25,931***	20,001***	5,636***
	(670)	(630)	(476)
HS GPA	3	374**	-420***
	(158)	(149)	(113)
First-Generation	1,348***	1,334***	-77
	(145)	(137)	(103)
Female	738***	314**	279***
	(148)	(139)	(105)
White	-1,439***	-1,574***	220
	(322)	(303)	(229)
Black	2,125***	1,258***	456*
	(339)	(319)	(241)
Asian	-1,744***	-9	-1,455***
	(408)	(384)	(290)
Hispanic	640*	2,463***	-1,748***
	(388)	(365)	(276)
Household Size	136***	40	80**
	(53)	(50)	(38)
N	8,727	8,727	8,727

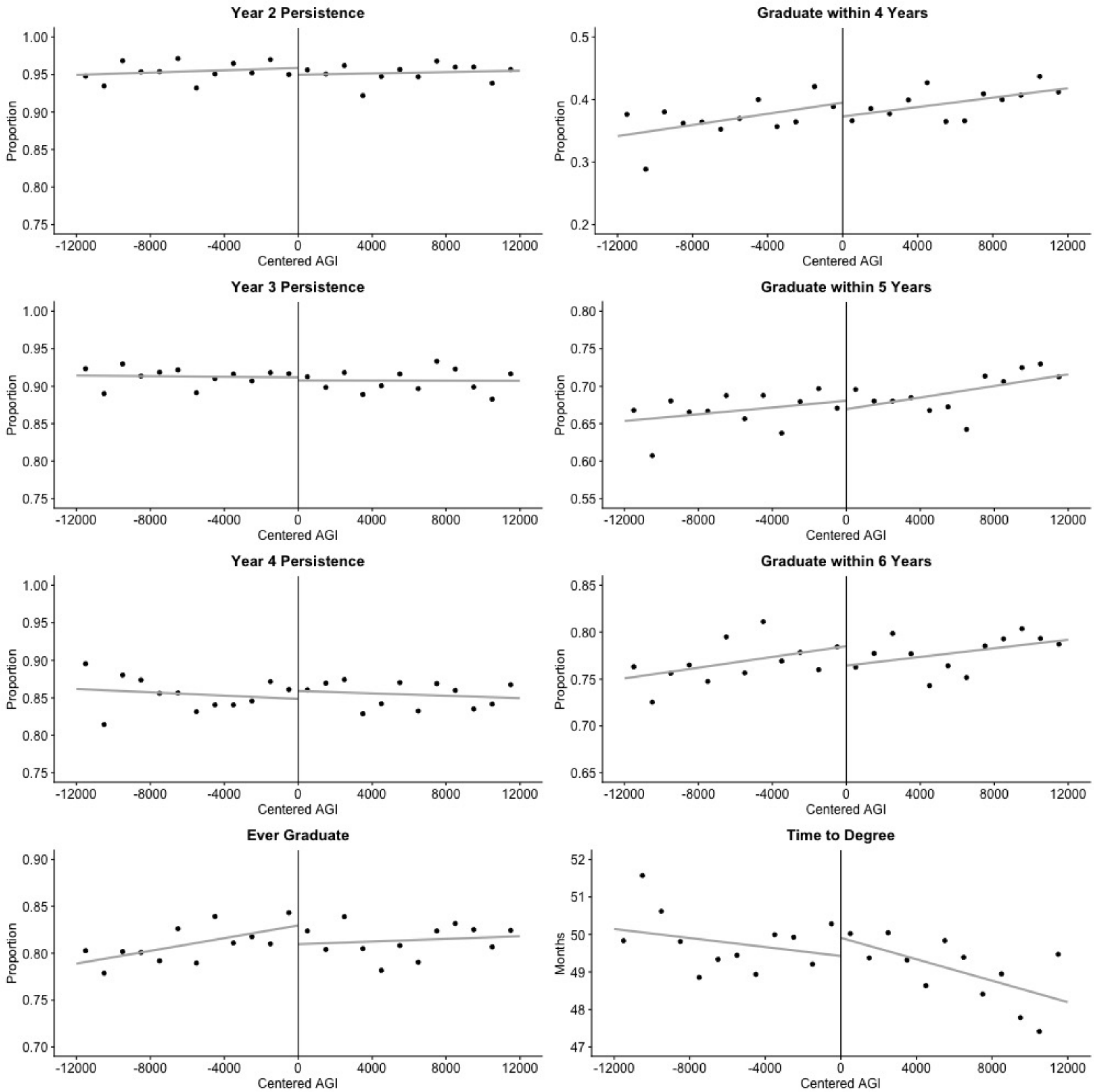
Notes: Estimates are from separate OLS regressions of each first-year aid component on the full set of observable student characteristics in the table. Predicted values from the regressions are plotted in Figure A.3, illustrating the overall similarity in observable characteristics across the poverty line. Only students within the optimal bandwidth are included. p<0.10, ** p<0.05, *** p<0.01.

Table A.2. Estimated Coefficients of Student Characteristics on First-Year Aid.



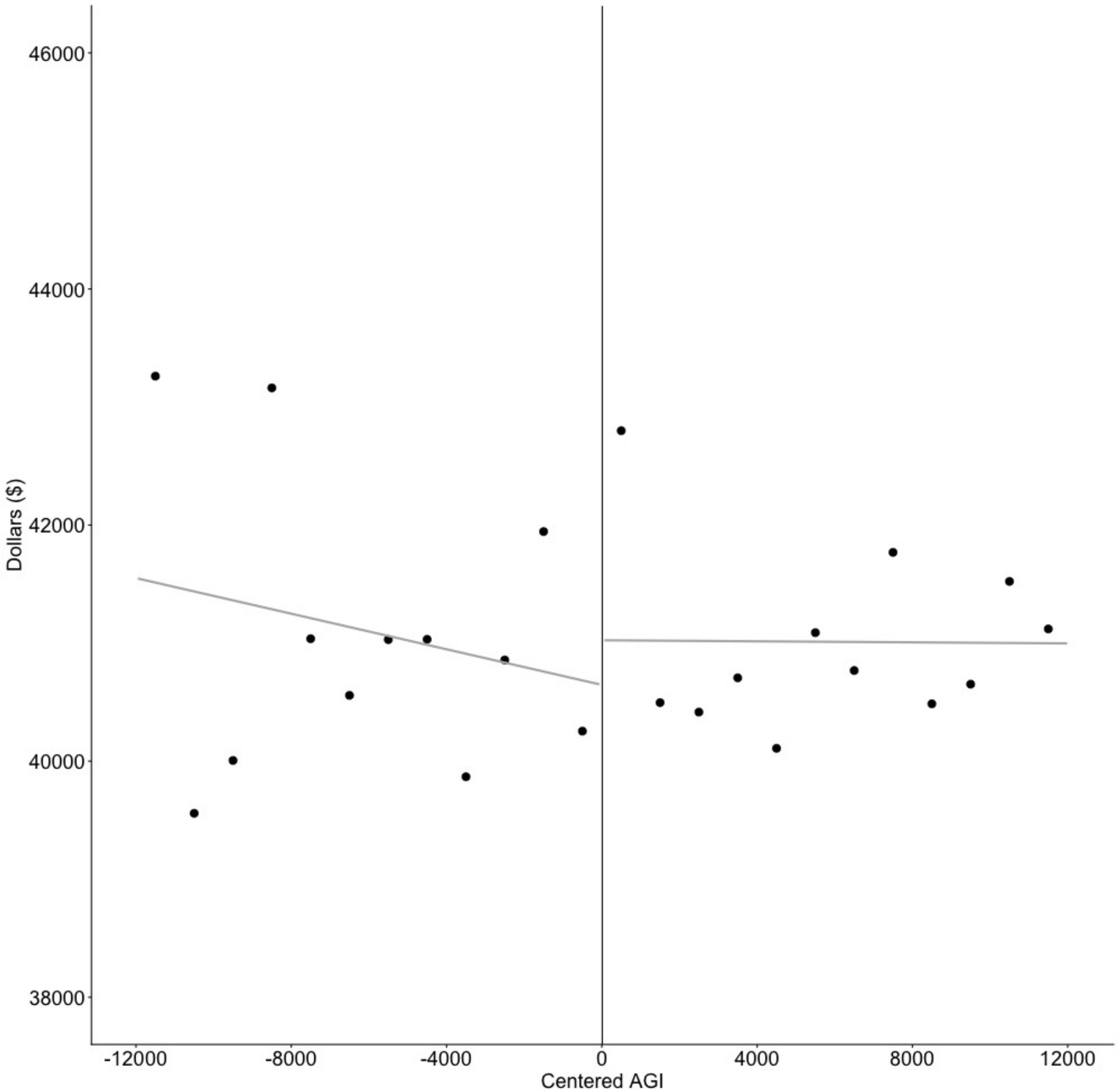
Notes: For students in the optimal bandwidth, this figure plots predicted financial aid amounts against centered AGI, using bins of width \$1,000. Predicted values are obtained from regressions of each aid component on all observable student characteristics (see Table A.2). The plotted values illustrate the continuity of predicted financial aid amounts across the poverty line—denoted by the vertical line—providing further evidence that students just above and below the threshold are similar in observable characteristics. Only students within the optimal bandwidth are included. Centered AGI is calculated by subtracting the year- and household-size-specific federal poverty level from a student's total AGI. Negative values denote SPAD eligibility.

Figure A.3. Predicted First-Year Aid Against Centered AGI.



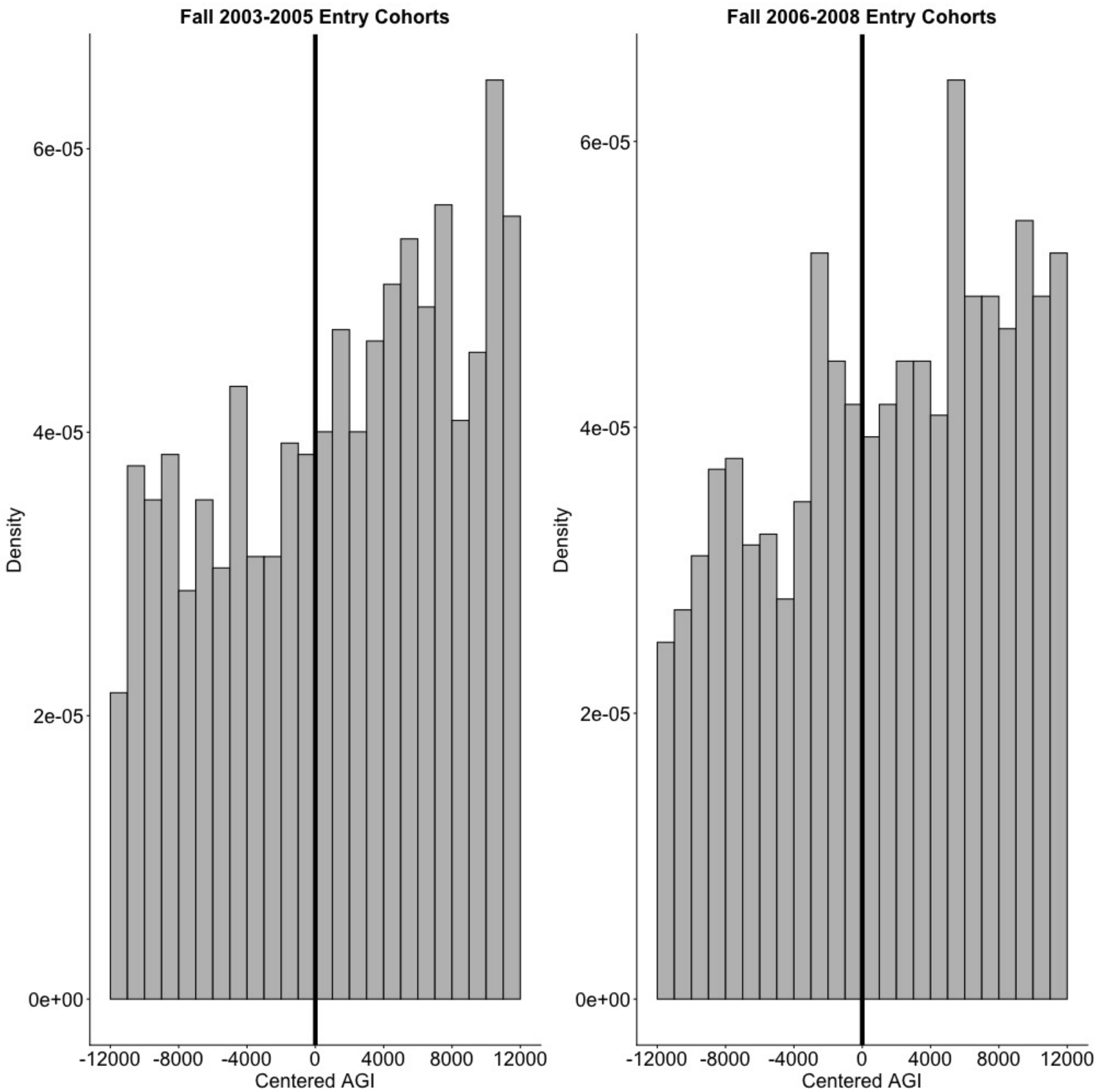
Notes: For students in the optimal bandwidth, this figure provides a visual representation of the persistence and graduation outcomes reported in Table 3, using bins of width $1,000. Consistent with the null results shown in the table, there is little evidence of a discontinuity at the poverty line across any of the outcomes. This reinforces the conclusion that substituting loans for gift aid did not meaningfully impact students' enrollment continuity or degree attainment.

Figure A.4. RD Plots—Academic Outcomes.



Notes: For students within $12,000 of the poverty line, this figure plots median initial earnings associated with students' admission majors against centered AGI using bins of width $1,000. It examines whether students below the poverty line selected higher-earning admission majors prior to the introduction of SPAD as a way to assess pre-existing discontinuities. Unlike the SPAD cohorts shown in Figure 4, there is no clear evidence of a discontinuity at the poverty line during this earlier period. However, these results should be interpreted with caution given the limited number of pre-SPAD cohorts, smaller cohort sizes, and the potential confounding influence of the early 2000s recession.

Figure A.5. Earnings Associated with Admissions Major, Fall 2000-2005 Cohorts.



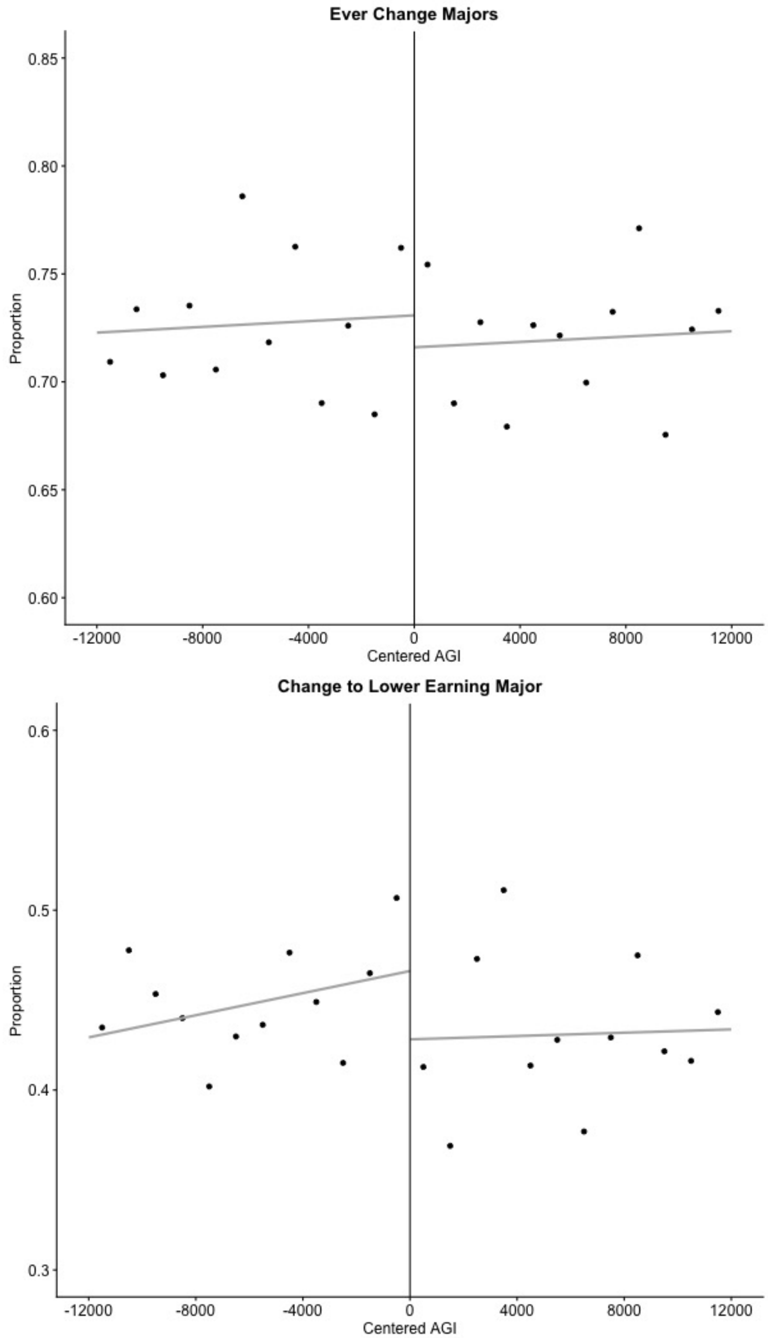
Notes: For students within $12,000 of the poverty line, this figure plots the distribution of centered AGI using bins of width $1,000 to examine whether the introduction of SPAD led to an increase in enrollment of students below the poverty line. Such a change would suggest that the program influenced students' decision to attend MSU. While there is a small visual increase in the density just below the threshold in the four cohorts following the introduction of SPAD (right graph), it does not appear large or widespread enough to constitute strong evidence of changes in enrollment patterns. Overall, the density patterns look similar across cohorts, supporting the identifying assumption that SPAD did not significantly alter the composition of enrolled students at the threshold.

Figure A.6. Density of Enrolled Students, Fall 2003-2005 and Fall 2006-2008 Cohorts.

Variable	Pre-SPAD	SPAD	Difference (95% CI)
White	0.60	0.55	(-0.108, 0.011)
Black	0.25	0.27	(-0.027, 0.078)
Asian	0.08	0.08	(-0.031, 0.035)
Hispanic	0.05	0.06	(-0.021, 0.034)
Female	0.60	0.62	(-0.043, 0.074)
HS GPA	3.41	3.51	(0.026, 0.182)
Household Size	3.83	3.88	(-0.124, 0.206)
N	513	560	-

Notes: This table compares observable characteristics of students below the poverty line across cohorts before and after the introduction of SPAD. The final column reports 95% confidence intervals for the difference in means. First-generation status is omitted because it was not collected prior to fall 2006.

Table A.3. Summary Statistics of Enrolled Students Below Poverty Line.



Notes: For students in the optimal bandwidth, this figure provides a visual representation of the major change outcomes reported in Table 4, using bins of width $1,000. Consistent with the null results shown in the table, there is little evidence of a discontinuity at the poverty line across any of the outcomes. This reinforces the conclusion that substituting loans for gift aid did not meaningfully impact the likelihood of students changing majors or, among those who did switch, movement into lower-earning majors.

Figure A.7. RD Plots—Major Change.

Outcome	**Male**	**Female**	**White, Asian**	**Non-White, Non-Asian**	**First-Generation**	**Non First-Generation**
Total Aid	3,465	5,720*	7,011*	4,096	1,555	10,315**
	(4,092)	(3,070)	(3,939)	(2,689)	(2,554)	(4,689)
Gift Aid	13,537***	18,296***	19,891***	13,871***	14,519***	20,610***
	(3,353)	(2,466)	(3,156)	(2,320)	(2,156)	(3,705)
Loans	-11,375***	-12,737***	-13,654***	-10,348***	-13,259***	-11,399***
	(2,890)	(2,286)	(2,923)	(1,992)	(1,954)	(3,368)
Work-Study	1,302**	163	775	574	296	1,104*
	(592)	(520)	(578)	(537)	(494)	(655)
N	2,879	4,586	4,620	2,845	3,670	3,795

Notes: Each column reports estimates of SPAD receipt on the cumulative amount of aid received up to that point in a student's enrollment using the specification in Equation (3). Analysis in each column only includes students in the indicated subgroup within the optimal bandwidth. All financial aid amounts are expressed in real 2023 dollars, adjusted using the Bureau of Labor Statistics' college tuition and fees price index (Series ID: CUUR0000SEEB01).
* $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.4. RD Estimates on Financial Aid—Heterogeneity.

Outcome	**Male**	**Female**	**White, Asian**	**Non-White, Non-Asian**	**First-Generation**	**Non First-Generation**
Persist, Year 2	-0.00	0.04	0.04*	0.01	0.04	-0.01
	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
Persist, Year 3	-0.02	0.04	0.04	0.00	0.04	-0.01
	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)
Persist, Year 4	-0.03	0.00	-0.04	0.03	0.03	-0.06
	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)
Graduate within 4 Years	0.05	0.04	0.08	0.01	0.06	0.03
	(0.07)	(0.05)	(0.07)	(0.05)	(0.05)	(0.07)
N	3,374	5,353	5,310	3,417	4,389	4,338
Graduate within 5 Years	0.07	0.02	0.07	0.00	0.06	-0.01
	(0.07)	(0.05)	(0.06)	(0.06)	(0.05)	(0.07)
N	3,117	4,946	4,927	3,136	4,009	4,054
Graduate within 6 Years	0.05	0.05	0.04	0.07	0.07	0.02
	(0.07)	(0.05)	(0.06)	(0.06)	(0.05)	(0.07)
Ever Graduate	0.01	0.07	0.04	0.06	0.08	0.01
	(0.06)	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)
N	2,853	4,524	4,567	2,810	3,596	3,781
Time to Graduate (Months)	-2.05	0.17	-2.18	1.32	0.39	-1.97
	(2.00)	(1.38)	(1.62)	(1.65)	(1.43)	(1.92)
N	2,482	4,118	4,258	2,342	3,203	3,397

Notes: Each row reports RD estimates of the impact of SPAD receipt on the given persistence or graduation outcome using the specification in Equation (3). The first four rows include all students. "Graduate within 5 years" is restricted to students who entered in fall 2019 or earlier; "Graduate within 6 Years" and "Ever Graduate" are restricted to those who entered in fall 2018 or earlier. These restrictions ensure students have sufficient time to reach the relevant outcome by spring 2024. "Time to degree" is calculated only among students who eventually graduate, based on the number of months between initial enrollment (assumed to begin in September of their entry year as all are fall entrants) and degree conferment. Analysis in each column only includes students in the indicated subgroup within the optimal bandwidth. * p<0.10, ** p<0.05, *** p<0.01.

Table A.5. RD Estimates on Persistence and Graduation—Heterogeneity.

Outcome	Male	Female	White, Asian	Non-White, Non-Asian	First-Generation	Non First-Generation
Panel A: RD Estimates						
Ever Change Majors	-0.03	0.06	0.01	0.05	0.06	-0.02
	(0.066)	(0.050)	(0.060)	(0.053)	(0.049)	(0.069)
N	3,374	5,353	5,310	3,417	4,389	4,338
Change to Lower-Earning Major	0.05	-0.01	-0.07	0.12*	0.09	-0.09
	(0.081)	(0.068)	(0.082)	(0.065)	(0.064)	(0.091)
N	2,133	3,538	3,516	2,155	2,799	2,872
Panel B: Diff.-in-Disc. Estimates						
STEM	-0.08	-0.16***	-0.11*	-0.13***	-0.09*	-0.17**
	(0.070)	(0.046)	(0.063)	(0.047)	(0.050)	(0.065)
N	3,374	5,353	5,310	3,417	4,389	4,338
Degree Mismatch	-0.49	-2.08***	-1.98**	-1.28*	-2.00***	-0.85
	(0.871)	(0.693)	(0.857)	(0.685)	(0.686)	(0.898)
N	3,288	5,260	5,196	3,352	4,310	4,238

Notes: Panel A reports RD estimates for whether students ever changed majors or switched to lower-earning majors using the specification in Equation (3). The second row only includes students who changed majors. Panel B reports diff.-in-disc. estimates comparing admissions and final majors in terms of STEM classification and degree mismatch using the specification in Equation (4). STEM includes engineering, biological sciences, mathematics/statistics, and physical sciences. Degree mismatch is measured in ACT points as the difference between a student's ACT score and the median ACT score for the previous five cohorts within their major. The mismatch analysis excludes students without reported ACT or SAT scores. This analysis includes all students, not just those who graduate. * p<0.10, ** p<0.05, *** p<0.01.

Table A.6. RD Estimates on Major Outcomes—Heterogeneity.

	Additional Bandwidths		DiD	Placebo Thresholds	
Outcome	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Total Aid	7,201*	4,957***	4,713***	4,810	-2,378
	(4,119)	(1,865)	(1,380)	(3,592)	(3,377)
Gift Aid	18,711***	15,952***	9,748***	5,965*	-1,992
	(3,402)	(1,511)	(1,162)	(3,250)	(2,543)
Loans	-12,609***	-11,712***	-6,191***	-882	-2,162
	(3,031)	(1,356)	(1,018)	(2,364)	(2,747)
Work-Study	1,099	718**	1,157***	-274	1,775***
	(678)	(292)	(239)	(616)	(330)
N	3,655	10,283	9,073	5,047	7,009
Bandwidth	±6,000	±18,000	±12,000	±12,000	±12,000
Cut-off	Actual	Actual	Actual	-15,000	+15,000

Notes: Each column reports estimates of SPAD receipt (or eligibility) on the cumulative amount of aid received through four years. Columns (1) and (2) replicate the main RD specification with narrower and broader bandwidths of $6,000 and $18,000 using the specification in Equation (3). Column (3) reports estimates from the DiD design using students within $12,000 of the poverty line from the specification in Equation (7). Columns (4) and (5) report placebo tests using artificial thresholds $15,000 below and above the true SPAD cutoff. All financial aid amounts are expressed in real 2023 dollars, adjusted using the Bureau of Labor Statistics' college tuition and fees price index (Series ID: CUUR0000SEEB01). * p<0.10, ** p<0.05, *** p<0.01.

Table A.7. Robustness Checks—Financial Aid.

Outcome	Additional Bandwidths (1)	(2)	DiD (3)	Placebo Thresholds (4)	(5)
Persist, Year 2	0.03	0.02	-0.01	-0.03	-0.00
	(0.032)	(0.014)	(0.010)	(0.030)	(0.023)
Persist, Year 3	0.04	0.02	-0.01	-0.01	0.00
	(0.043)	(0.019)	(0.014)	(0.039)	(0.030)
Persist, Year 4	0.03	-0.00	-0.02	0.01	-0.04
	(0.052)	(0.024)	(0.017)	(0.049)	(0.038)
Graduate within 4 Years	0.11	0.02	-0.03	0.04	0.00
	(0.070)	(0.031)	(0.021)	(0.062)	(0.050)
N	4,284	12,051	10,707	5,938	8,247
Graduate within 5 Years	0.04	0.00	-0.01	0.10	0.04
	(0.071)	(0.032)	(0.021)	(0.067)	(0.048)
N	3,962	11,126	10,065	5,393	688
Graduate within 6 Years	0.05	0.02	-0.00	0.08	0.01
	(0.067)	(0.030)	(0.020)	(0.066)	(0.046)
Ever Graduate	0.06	0.03	-0.00	0.10	0.01
	(0.062)	(0.029)	(0.019)	(0.062)	(0.044)
N	3,615	10,179	9,412	4,837	7,115
Time to Graduate (Months)	-0.05	0.05	0.38	0.25	-0.86
	(1.950)	(0.840)	(0.644)	(1.651)	(1.254)
N	3,248	9,092	7,956	4,384	6,275
Bandwidth	±6,000	±18,000	±12,000	±12,000	±12,000
Cut-off	Actual	Actual	Actual	-15,000	+15,000

Notes: Each row reports RD estimates of the impact of SPAD receipt on the given persistence or graduation outcome. The first four rows include all students. "Graduate within 5 years" is restricted to students who entered in fall 2019 or earlier; "Graduate within 6 Years" and "Ever Graduate" are restricted to those who entered in fall 2018 or earlier. These restrictions ensure students have sufficient time to reach the relevant outcome by spring 2024. "Time to degree" is calculated only among students who eventually graduate, based on the number of months between initial enrollment (assumed to begin in September of their entry year as all are fall entrants) and degree conferment. Columns (1) and (2) replicate the main RD specification with narrower and broader bandwidths of \$6,000 and \$18,000 using the specification in Equation (3). Column (3) reports estimates from the DiD design using students within \$12,000 of the poverty line from the specification in Equation (7). Columns (4) and (5) report placebo tests using artificial thresholds \$15,000 below and above the true SPAD cutoff. * p<0.10, ** p<0.05, *** p<0.01.

Table A.8. Robustness Checks—Academic Outcomes.

Outcome	Additional Bandwidths (1)	(2)	DiD/DDD (3)	Placebo Thresholds (4)	(5)
Panel A: RD/DiD Estimates					
Ever Change Majors	0.06	0.03	0.02	-0.02	0.00
	(0.071)	(0.032)	(0.020)	(0.065)	(0.050)
N	4,284	12,051	10,707	5,938	8,247
Change to Lower-Earning Major	0.24**	0.10**	0.01	-0.06	-0.09
	(0.106)	(0.044)	(0.025)	(0.085)	(0.065)
N	2,766	7,837	7,022	3,834	5,358
Panel B: Diff.-in-Disc./DDD Estimates					
STEM	-0.07	-0.09***	-0.02	-0.03	-0.03
	(0.063)	(0.029)	(0.021)	(0.077)	(0.058)
N	4,284	12,051	10,707	5,938	8,247
Degree Mismatch	-0.42	-0.79*	-0.00	0.11	0.17
	(0.862)	(0.406)	(0.284)	(0.847)	(0.654)
N	4,200	11,800	10,505	5,802	8,062
Bandwidth	±6,000	±18,000	±12,000	±12,000	±12,000
Cut-off	Actual	Actual	Actual	-15,000	+15,000

Notes: Panel A reports RD and DiD estimates for whether students ever changed majors or switched to lower-earning majors. The second row only includes students who changed majors. Panel B reports diff.-in-disc. and DDD estimates comparing admissions and final majors in terms of STEM classification and degree mismatch. STEM includes engineering, biological sciences, mathematics/statistics, and physical sciences. Degree mismatch is measured in ACT points as the difference between a student's ACT score and the median ACT score for the previous five cohorts within their major. The mismatch analysis excludes students without reported ACT or SAT scores. This analysis includes all students, not just those who graduate. Columns (1) and (2) replicate the main RD and diff.-in-disc. specifications with narrower and broader bandwidths of $6,000 and $18,000 using the specifications in Equations (3) and (4), respectively. Column (3) reports estimates from the DiD and DDD designs using students within $12,000 of the poverty line using the specifications in Equations (7) and (9), respectively. Columns (4) and (5) report placebo tests using artificial thresholds $15,000 below and above the true SPAD cutoff. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.9. Robustness Checks—Major Outcomes, All Students.

Outcome	Additional Bandwidths (1)	(2)	DiD/DDD (3)	Placebo Thresholds (4)	(5)
Panel A: RD/DiD Estimates					
Ever Change Majors	0.02	0.02	0.03	0.01	-0.00
	(0.078)	(0.036)	(0.021)	(0.071)	(0.053)
N	3,386	9,484	8,404	4,560	6,576
Change to Lower-Earning Major	0.19	0.07	-0.00	0.00	-0.08
	(0.114)	(0.048)	(0.027)	(0.092)	(0.069)
N	2,440	6,877	6,176	3,311	4,728
Panel B: Diff.-in-Disc./DDD Estimates					
STEM	0.02	-0.08**	-0.02	0.01	-0.03
	(0.074)	(0.034)	(0.021)	(0.089)	(0.065)
N	3,386	9,484	8,404	4,560	6,576
Degree Mismatch	-0.25	-0.73	-0.00	0.15	0.20
	(1.003)	(0.473)	(0.284)	(0.989)	(0.738)
N	3,312	9,261	8,224	4,432	6,409
Bandwidth	±6,000	±18,000	±12,000	±12,000	±12,000
Cut-off	Actual	Actual	Actual	-15,000	+15,000

Notes: Panel A reports RD and DiD estimates for whether students ever changed majors or switched to lower-earning majors. The second row only includes students who changed majors. Panel B reports diff.-in-disc. and DDD estimates comparing admissions and final majors in terms of STEM classification and degree mismatch. STEM includes engineering, biological sciences, mathematics/statistics, and physical sciences. Degree mismatch is measured in ACT points as the difference between a student's ACT score and the median ACT score for the previous five cohorts within their major. The mismatch analysis excludes students without reported ACT or SAT scores. This analysis only includes students who graduate. Columns (1) and (2) replicate the main RD and diff.-in-disc. specifications with narrower and broader bandwidths of \$6,000 and \$18,000 using the specifications in Equations (3) and (4), respectively. Column (3) reports estimates from the DiD and DDD designs using students within \$12,000 of the poverty line using the specifications in Equations (7) and (9), respectively. Columns (4) and (5) report placebo tests using artificial thresholds \$15,000 below and above the true SPAD cutoff. * p<0.10, ** p<0.05, *** p<0.01.

Table A.10. Robustness Checks—Major Outcomes, Graduates.

A Difference-in-Differences Specification

The availability of data from fall 2000 onwards—six years prior to the implementation of the SPAD program—enables the use of a DiD approach to estimate the effects of SPAD eligibility. While the primary analysis relies on a RD design around the income eligibility threshold, the DiD framework provides a complementary identification strategy by leveraging variation over time and across income groups. Specifically, the DiD compares outcomes for students just below the federal poverty line (eligible for SPAD) to those just above the line (ineligible), before and after the program's introduction in 2006.

The estimating equation is shown below:

$$Y_{it} = \beta_0 + \beta_1 Post2006_i + \beta_2 Below_{i,t=1} + \beta_3 (Post2006_i \times Below_{i,t=1}) + \gamma X_i + College_{i,t=1} + \epsilon_{it} \quad (7)$$

where $Post2006_i$ is an indicator equal to 1 if student i entered MSU in fall of 2006 or later, $Below_{i,t=1}$ is an indicator of SPAD eligibility equal to 1 if student i's AGI in their first year was below the federal poverty line, and β_3 captures the DiD estimate of SPAD eligibility. The analysis is restricted to students whose AGI in their first year was within \$12,000 of the poverty line.[23]

The key identifying assumption of the DiD design is that, in the absence of SPAD, outcomes for students just below and just above the poverty line would have followed similar trends over time. That is, any divergence in outcomes between the two groups after 2006 can be attributed to SPAD eligibility, not pre-existing differences. To assess this assumption, I estimate and plot event studies using the following equation:

$$\begin{aligned} Y_{it} = \beta_0 + \beta_1 Below_{i,t=1} + \sum_{j=-6}^{14} \psi_j D_{ij} + \sum_{j=-6}^{14} \delta_j (D_{ij} \times Below_{i,t=1}) + \\ \gamma X_i + College_{i,t=1} + \epsilon_{it} \end{aligned} \quad (8)$$

where j indexes years relative to the implementation of SPAD, D_{ij} is an indicator for event time, and δ_j captures the differential effect for eligible students in year j relative to the baseline year (fall 2005). These estimates are plotted in the event study graphs (Figures A.8 to A.10) at the

[23] This matches the optimal RD bandwidth, allowing for ease of comparison between the two analyses.

end of this section.

Pre-trends are generally flat, though standard errors are large for some cohorts, making it difficult to draw strong conclusions about pre-treatment dynamics. However, large and sustained differences emerge in the financial aid plots following SPAD implementation, while there are no consistent and noticeable changes in persistence or graduation outcomes.

Difference-in-Difference-in-Differences

Like the diff.-in-disc. approach used in the main analysis, the DDD specification uses records of students' intended major at the time of application submission—prior to receiving aid packages—and compares them to their final major. This structure allows for estimation of the impact of SPAD eligibility on changes in major field over time, while accounting for baseline differences across cohorts and income groups. To extend the DiD framework into a DDD, I introduce an additional term, Aid_{it}, which equals 1 for final major observations and 0 for intended majors at admission. The estimating equation is shown below:

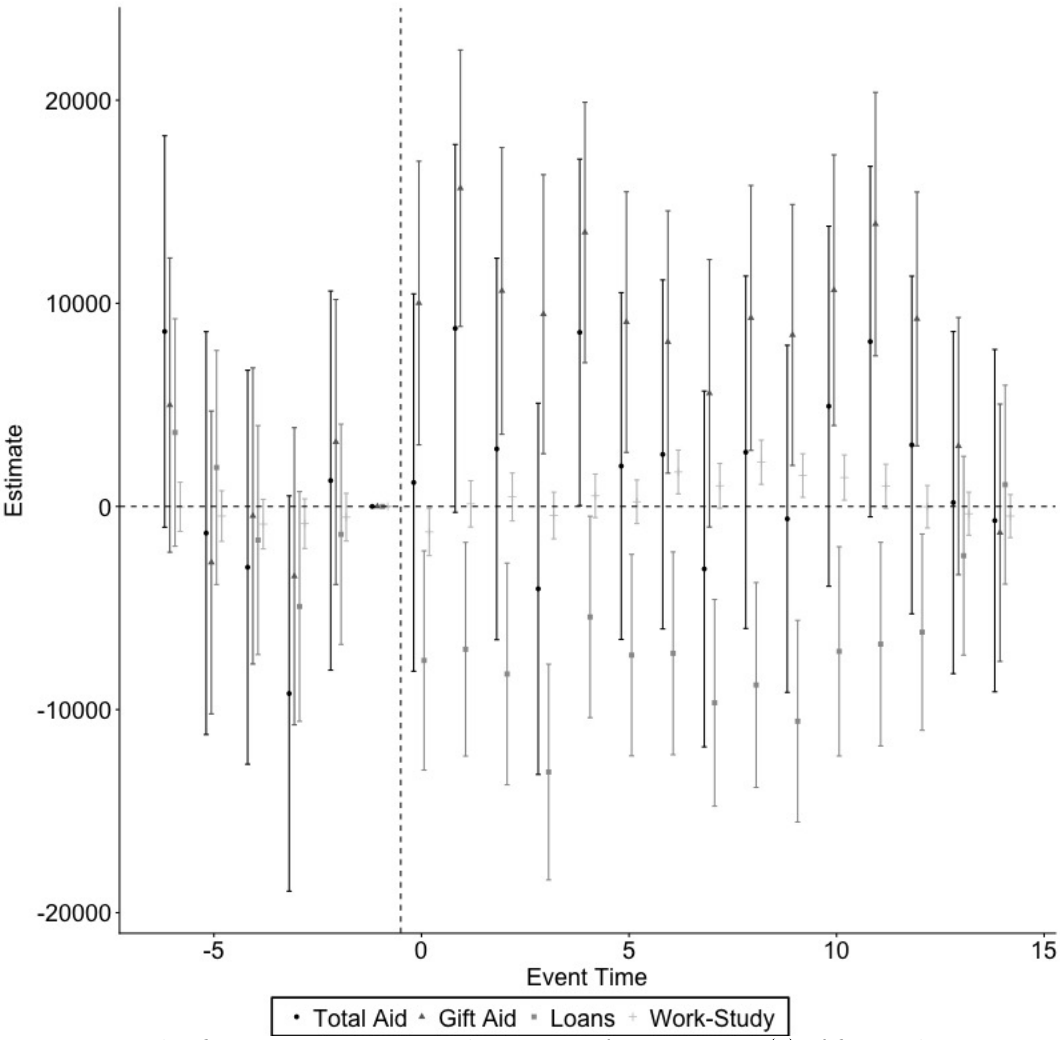
$$\begin{aligned} Y_{it} = {} & \beta_0 + \beta_1 Post2006_i + \beta_2 Below_{i,t=1} + \beta_3 Aid_{it} + \beta_4 (Post2006_i \times Below_{i,t=1}) + \\ & \beta_5 (Post2006_i \times Aid_{it}) + \beta_6 (Below_{i,t=1} \times Aid_{it}) + \beta_7 (Post2006_i \times Below_{i,t=1} \times Aid_{it}) + \\ & \gamma X_i + College_{i,t=1} + \epsilon_{it} \end{aligned} \tag{9}$$

The coefficient on the triple interaction term, β_7, captures the effect of SPAD eligibility on changes in major characteristics between admission and graduation. Intuitively, the DDD estimator represents the difference between two DiD estimates. In this context, it captures how the change in major characteristics from admission to graduation differs between SPAD-eligible and ineligible students, before and after the introduction of the SPAD program.

The identifying assumption for the DDD specification is an extension of the DiD parallel trends assumption. Specifically, it assumes that in the absence of SPAD, the change in major characteristics between admission and graduation would have followed similar trends over time for students just below and just above the poverty line. To assess this assumption, I estimate and plot DDD event studies using the equation below:

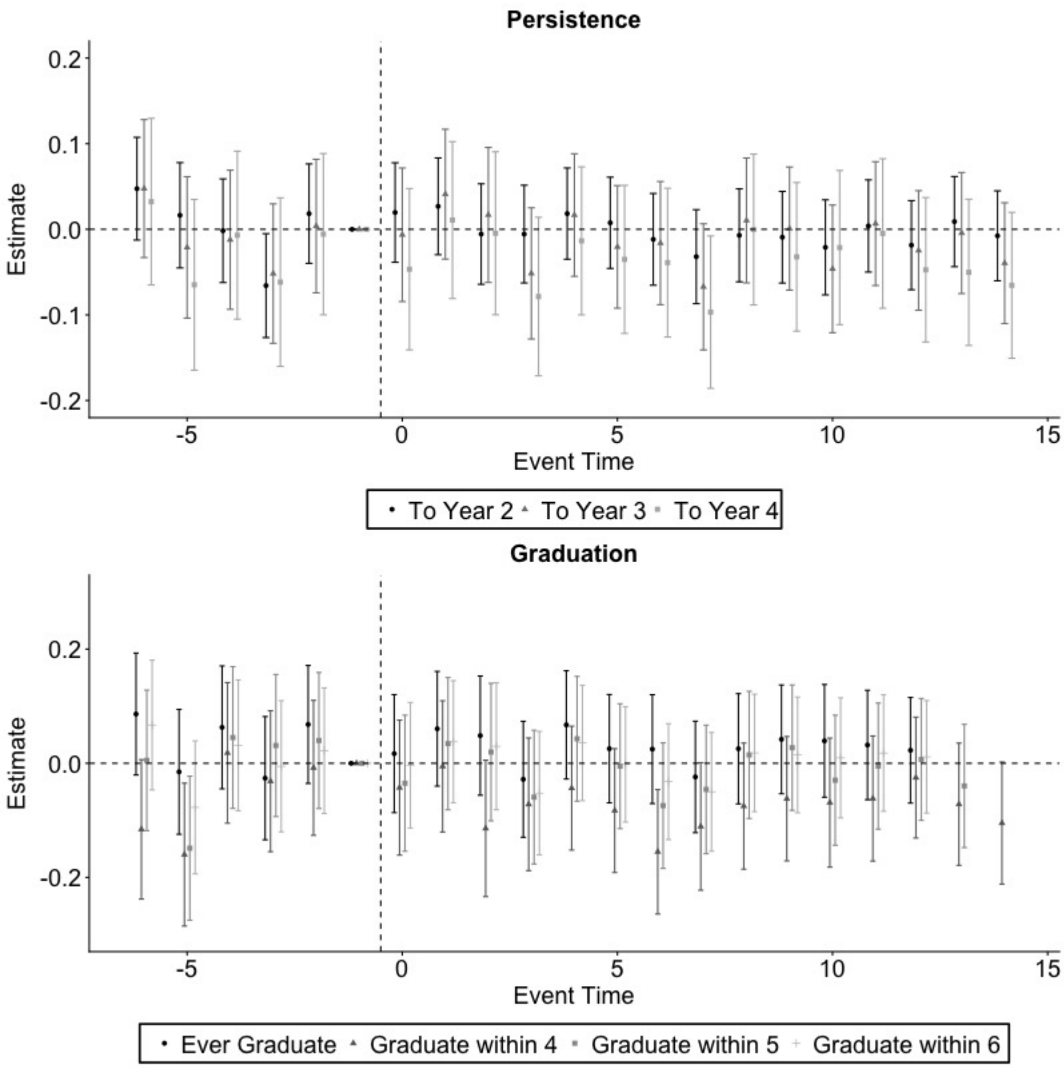
$$
\begin{aligned}
Y_{it} = {} & \beta_0 + \beta_1 Below_{i,t=1} + \beta_2 Aid_{it} + \sum_{j=-6}^{14} \psi_j D_{ij} \\
& + \sum_{j=-6}^{14} \theta_j (D_{ij} \times Aid_{it}) + \sum_{j=-6}^{14} \phi_j (D_{ij} \times Below_{i,t=1}) \\
& + \sum_{j=-6}^{14} \delta_j (D_{ij} \times Aid_{it} \times Below_{i,t=1}) + \gamma X_i + College_{i,t=1} + \epsilon_{it}
\end{aligned} \tag{10}
$$

where j indexes years relative to the implementation of SPAD, D_{ij} is an indicator for event time j, and δ_j captures the differential change in major characteristics (from admission to graduation) for SPAD-eligible students in year j, relative to the baseline year (fall 2005). These estimates are plotted in the event study graphs (Figure A.11) at the end of this section. While both DDD event studies are quite noisy, pre-trends appear stable and there is no evidence of post-SPAD effects.



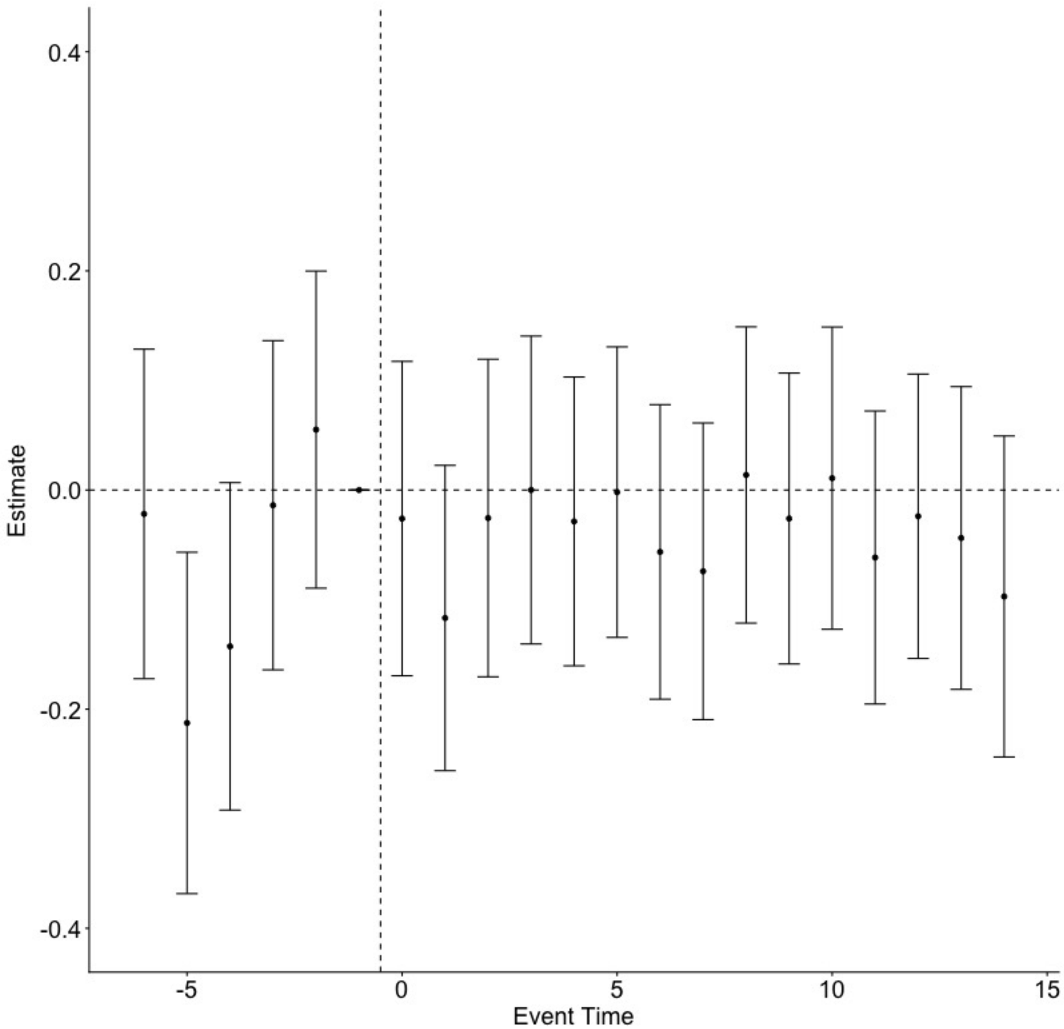
Notes: This figure presents event study estimates from Equation (8) of financial aid components, with fall 2005—the final cohort before the introduction of SPAD—serving as the reference year. Following the implementation of SPAD, there is a clear and sustained shift in the composition of aid: gift aid increases while loans decrease, consistent with the main RD and DiD results. While there are a few irregular patterns in the pre-SPAD cohorts, they are small in magnitude and not suggestive of strong pre-trends, supporting the interpretation of a program-driven shift in aid composition.

Figure A.8. Event Studies—Financial Aid.



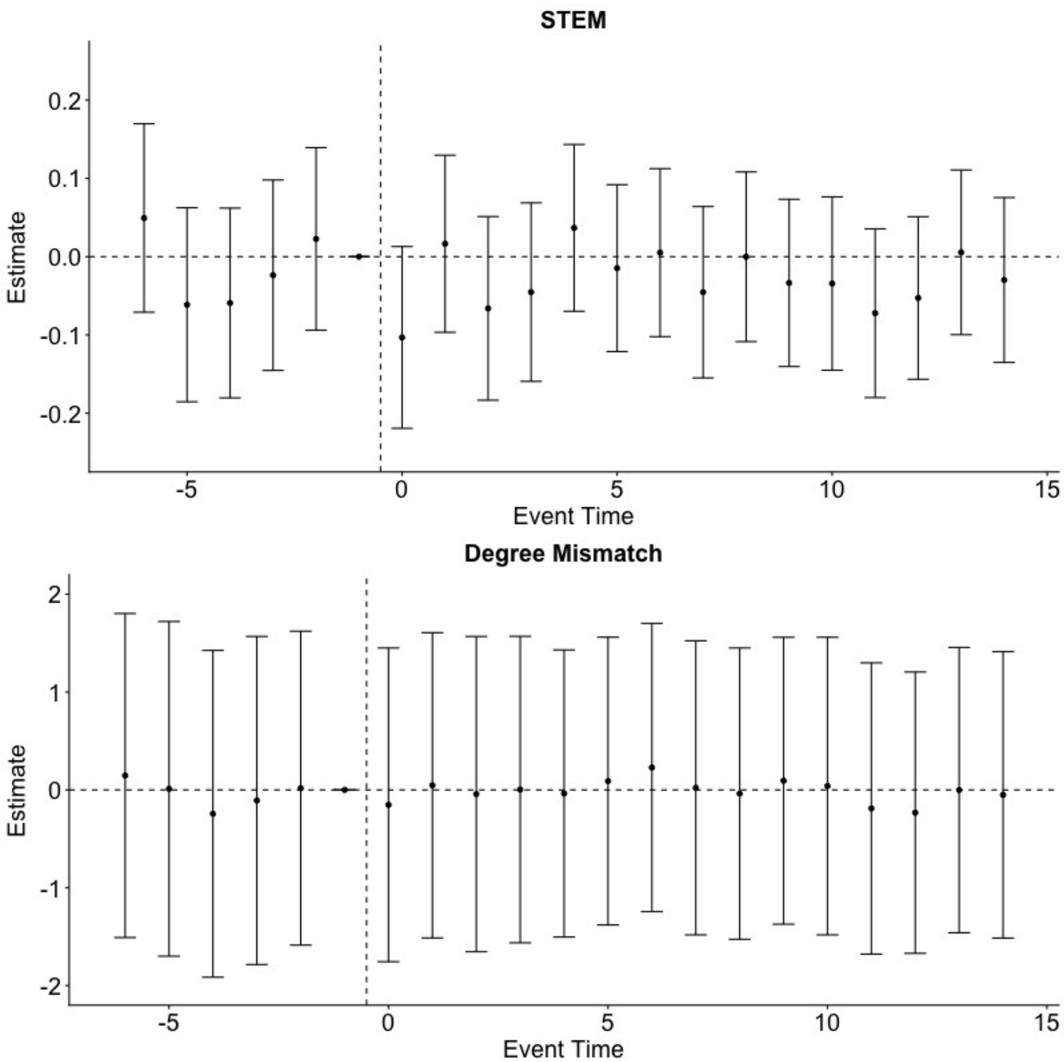
Notes: This figure presents event study estimates from Equation (8) of persistence (top panel) and graduation outcomes (bottom panel), with fall 2005 serving as the reference year. Across both panels, there is little evidence of meaningful changes following the introduction of SPAD, consistent with the effects reported the paper. The estimates are relatively stable over time, with no indication of emerging trends in the post-SPAD period. The penultimate cohort includes only two outcomes because six-year graduation rates and the ever graduated measure are not yet observable. The final cohort includes only one outcome due to the additional limitation that five-year graduation rates cannot yet be measured for that group.

Figure A.9. Event Studies—Persistence and Graduation.



Notes: This figure presents event study estimates from Equation (8) of movement into lower-earning majors, limited to students who changed majors, with fall 2005 serving as the reference year. Following the introduction of SPAD, there is no consistent evidence of increased switching into lower-earning majors. The large fluctuations at event time –5 and –4 likely reflect noise from small cohort sizes, and and many students from these cohorts would have graduated during the Great Recession—a period that may have influenced major decisions independently of aid composition.

Figure A.10. Event Studies—Change to Higher-Lower Earning Major.



Notes: This figure presents event study estimates from Equation (10) for STEM (top panel) and degree mismatch (bottom panel) outcomes, with fall 2005 serving as the reference year. Overall, the graphs show no meaningful impacts on these outcomes, consistent with the results shown in column (3) of Table A.9.

Figure A.11. Event Studies—STEM and Degree Mismatch.